SCIENCE PROGRESS
IN THE TWENTIETH CENTURY
A QUARTERLY JOURNAL OF SCIENTIFIC WORK & THOUGHT

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VOL. XII
1917—1918

LONDON
JOHN MURRAY, ALBEMARLE STREET, W.
1918

PRINTED BY
HAZELL, WATSON AND VINEY, LD.
LONDON AND AYLESBURY,
ENGLAND.

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SCIENCE PROGRESS

RECENT ADVANCES IN SCIENCE

PHILOSOPHY. By HUGH ELLIOT.

IT was pointed out in a former contribution to this section of SCIENCE PROGRESS that the various subjects of inquiry, now floundering in the morass of Metaphysics, may be expected in course of time to separate off from their unfortunate connections, and give rise to new branches of special science. There are many indications that the study of human character will before long undergo an evolution of this nature. We know that even in the eighteenth century Rousseau attempted and failed to construct a science of character. We know that in the nineteenth century John Stuart Mill attempted and failed in the same task. Ribot made some genuine contributions, and Bain wrote a book, which however was too much taken up with attacking the obsolete notions of phrenology to be of any real value at present. The twentieth century has already done far more in this direction than the whole previous century. Mentioning Remy de Gourmont merely *en passant*, we have the fine analysis of Jules de Gaultier, who started upon the subject afresh, without the smallest metaphysical taint, and definitely sketched out solid blocks of true knowledge. Since then, there have been a variety of valuable French and English works, till we come down to the unpretentious little book of Dr. Charles Mercier, entitled *Human Temperaments*.[1] Here we have a vivid portrayal of different types of human character. Dr. Mercier scarcely attempts analysis, but proceeds purely by the natural history method. The work is descriptive only, and none the less valuable for that; for accurate description and accumulation of facts must always precede attempts at co-ordination.

Another work has also been produced recently by the same lively author, entitled *On Causation, with a Chapter on Belief*,[2]

[1] The Scientific Press, Ltd. (see p. 151). [2] Longmans, Green & Co., 1916.

in which Dr. Mercier makes an unsparing attack on logicians in general, and on many particular individuals among them. If radical contempt of authority were the sole mark of scientific value, then this book would be a very important work indeed. In point of fact its daring criticism and vigorous originality are likely to alienate many who would gain much from a perusal of it; for all the cobwebs of pedantry and tradition are here swept away as by a fresh breeze, or as we might perhaps more truly put it, by a raging gale. Yet, valuable as many of Dr. Mercier's criticisms appear, we are by no means disposed to agree with them all. In particular we take objection to his argument that the relation of cause and effect is one in which the conception of force or power is necessary. That there is in the *cause* an active principle, compelling the *effect* to ensue, appears to involve a metaphysical assumption, akin to that of a "vital principle" in physiology. The present writer has had occasion formerly in this review to comment on a fallacy to which Dr. Mercier is still somewhat prone; and he thinks that here at least the logicians have the advantage of Dr. Mercier.

This subject is treated more widely, though far less scientifically, in M. Emile Boutroux' *The Contingency of the Laws of Nature*.[1] We note with satisfaction the statement of M. Boutroux in the Preface that philosophy "should be grounded on the sciences"; we only regret that M. Boutroux, having rendered lip-service to science, does nothing further to carry it into reality. Indeed as early as page 2 he seems to regret his own admission in the Preface, for he there states that purely descriptive science "perverts the relations of things." As in nearly all modern philosophy, he oscillates round the old question of liberty and necessity, of free-will and determinism. But he moves on the old and discredited planes of thought; he has not attained the more hopeful outlook furnished by physiology. He talks about the faculties of the soul, about their relative dignity, etc., as though philosophy were a branch of ethics. The experience of centuries has taught us the futility of this mode of reasoning; nor will philosophy ever advance, so long as philosophers occupy themselves with the moral bearing of their subject. They must adopt the attitude of science, and survey their facts with cold impartiality; they

[1] Translated by Fred Rothwell. (Chicago and London: Open Court Publishing Co., 1916.)

must analyse the human mind as the naturalist studies under the microscope the anatomy of a fly.

To the innumerable works already published on Instinct, one further has been added: *What is Instinct?* by C. Bingham Newland.[1] The author relates in an attractive way many of the most curious instincts of the commoner birds and insects, and explains them by reference to telepathy, and by a theory of group-minds, by which the actions of animals are controlled more accurately and effectively than by any manifestation of mere individual intelligence. Mr. Newland can scarcely expect biologists to adopt his theory, which attempts to explain what is difficult to understand by a doctrine which is impossible to understand. Though we cannot take the philosophy of this book seriously, yet it constitutes pleasant reading, on account of its original observations and genuine feeling for Nature. The author has paid special attention to the " bleating " of the snipe, and reaches the conclusion that it is due not to the tail, but to the wings. In view of a discussion on the subject which took place at the Zoological Society some years ago, this conclusion must be looked upon as heterodox. Nevertheless it has more evidence in its support than was then adduced, and it is not improbable that a further change of conviction may take place.

We are pleased to see that a reprint has been issued by the Cambridge University Press of *The Mechanistic Conception of Life*, by Jacques Loeb,[2] one of our foremost philosophic naturalists. Little need be said of this important work, which was fully reviewed on its first publication by the University of Chicago Press. It records the author's experiments in artificial parthenogenesis; and endeavours to explain various elementary animal activities on a purely mechanistic basis. The book has already received wide attention in this country, and is a standing example of the treatment of philosophic and psychologic problems by scientific methods. We do not perhaps get very far towards explanations; but an advance at all events is made, and in the direction not of darkness and further mystification, but of light.

The Open Court Publishing Company has brought out two

[1] Murray, 1916.

[2] University of Chicago Press, Chicago, Illinois; Cambridge University Press, London.

very useful translations of foreign philosophical works. The first of these is Leibnitz's *New Essays concerning Human Understanding*, which has been admirably translated and annotated by Mr. A. G. Langley. It is indeed remarkable that this work, in which Leibnitz attacked the philosophy of our great English thinker Locke, should never before have been translated into the English language. The translation is taken from vol. v. of the standard German edition of Gerhardt's *Die philosophischen Schriften von G. W. Leibnitz*. The main proposition of Leibnitz is the existence of innate ideas, in opposition to the theory of Locke that all knowledge is derived either directly or indirectly from experience. Although the phase then reached of this ancient controversy is now chiefly of historical interest, the whole problem being transferred to a higher plane by modern biology, yet it is pleasant to note that the present attitude of science is much more nearly allied to the views of the English philosopher than to those of the German.

Finally we note an excellent translation by Miss Margaret Jourdain of *Diderot's Early Philosophical Works*,[1] including the *Pensées philosophiques*, *Lettre sur les Aveugles*, and *Lettre sur les Sourds et Muets*. To the man of science, the most interesting of these is the *Letter on the Blind*, for which Diderot was imprisoned at Vincennes. The latter half of the eighteenth century in France probably marks the nearest approximation between science and philosophy of any period in history. No period therefore is more interesting to the man of science who desires to study the historical and literary origins of modern thought. Diderot was one of the central figures of this brilliant group, which—be it noted once again—was in close alliance with the English thinkers of that time. As Brunetière wrote of Diderot in 1898, "There is no trace of anything but England in the work of the man who has often been described as the most German of Frenchmen."

MATHEMATICS. By PHILIP E. B. JOURDAIN, M.A., Cambridge.

To meet a generally expressed wish, and in view of the present situation, the date for sending in memoirs (on the theory of functions) in competition for the prize founded by King Gustavus V. of Sweden has been again postponed, this time to October 31, 1917.

[1] Open Court, 1916.

At a meeting of the Council of the Mathematical Association of America at the end of 1916 " it was voted to appoint a committee which should, in conjunction with a similar committee at the [American Mathematical] Society, consider the question of possible assistance for the *Revue Semestrielle* and the *Jahrbuch über die Fortschritte der Mathematik*. The committee was empowered to include also in its investigation other international projects of a kind similar to the two named. Mathematicians the country over are feeling increasingly the deplorable influence of the European war as it affects such indispensable aids as the German and French encyclopædias, the two journals above mentioned, and similar reference books. This action has been taken in order that the two great mathematical organisations of America may consider what contribution they may perhaps make in rendering assistance to these valuable journals of record " (*Amer. Math. Monthly*, 1917, **24**, 64). At a meeting of the Chicago Section of the American Mathematical Society held at about the same time, among the topics informally discussed were " the desirability of holding ourselves in readiness to assist the publishers of the *Revue Semestrielle* and the *Fortschritte* in case it becomes necessary, on account of the war conditions, in order to continue the publications " (*ibid.* 97).

A short account of the life and work of Jean Gaston Darboux (1842–1917) is given by Sir Joseph Larmor in *Nature* (1917, **99**, 28). Darboux's work was principally on the theories of surfaces and curves, and his great book on infinitesimal geometry, the *Théorie générale des surfaces*, was published at Paris in four volumes between 1887 and 1896. The second volume of the *Œuvres* of Henri Poincaré, edited by Darboux, was published at Paris in 1916.

History.—J. H. Weaver (*Bull. Amer. Math. Soc.* 1916, **23**, 127–35) gives an interesting introductory study of the work of Pappus of Alexandria. It appears to be doubtful that Sir T. L. Heath will ever bring out any book on Pappus, and Weaver has made a careful translation of the *Collections*. The facts which have come out of Weaver's long study seem to set at naught the accusations of many writers of plagiarisms on the part of Pappus. Weaver also gave (*School Sci. and Math.* 1916, **16**, 674–9) some theorems from Pappus on isoperimetrical figures.

A Spanish translation has lately been published (Madrid,

1916) of the Arabic MS. on algebra written by Ibn Bahr in A.D. 1343 (*Nature*, 1917, **98**, 406–7).

C. Delisle Burns (*Mind*, 1916, **25**, 506–12) gives some extracts from William of Ockham's *Quotlibeta* which show his views on continuity and Zeno's arguments on motion. However, Burns does not notice that these views are apparently borrowed from Roger Bacon, who again apparently borrowed them from an ancient Arabian author.

G. Loria (Scientia, 1917, **21**, *101–21) gives a very good general account of the ways in which imaginaries in algebra have been regarded from the earliest times up to about the middle of the nineteenth century.*

A. Favaro (*ibid.* 1916, **20**, 417–34) criticises in a most learned and convincing fashion the view of the late Pierre Duhem that Leonardo da Vinci's new ideas in statics and dynamics had a large diffusion and a very great influence on thought in the domains spoken of during the sixteenth century and on that of Galileo and his school. Favaro, the editor of the splendid National Edition of Galileo's *Opere*, examines the thesis, particularly in what concerns Galileo and Benedetto Castelli, and proves, in so far as proof is possible in such a case, the independence of their work.

Miss Dorothy Wrinch (*Monist*, 1917, **27**, 83–104) gives an interesting account of the life and work, particularly part ot the mathematical work, of Bernard Bolzano (1781–1848), and A. E. Heath (*ibid.* 1–21, 22–35, 36–56) gives three articles on the life and work of Hermann Grassmann (1809–1877), the neglect of Grassmann's work, and the geometrical analysis of Grassmann and its connection with Leibniz's geometrical " characteristic."

H. Bateman (SCIENCE PROGRESS, 1917, **11**, 508–12) gives a mainly historical account of the connection of the " calculus of functions " with the theory of integral equations.

Here we may notice that C. E. Weatherburn (*Math. Gaz.*, 1917, **9**, 2–5) makes a plea for a more general use of vector analysis in applied mathematics, and E. B. Wilson (*Bull. Amer. Math. Soc.* 1917, **23**, 169–72) considers questions in the notations of vector analysis.

Logic, Principles, and Theory of Aggregates.—Those who know something of what the late Louis Couturat intended to write know that he planned a *Manuel de Logistique*. The manuscript appears to have been mostly written about 1906,

and an extract from the second chapter, on the logical relations of concepts and propositions, is given in the *Rev. de Métaphys.* (1917, **24**, 15–58). In this work the trace of Frege's ideas is almost as marked as that of Russell's, but although functions (which Couturat strangely says are "expressions") of the "second order" (functions of functions) are mentioned, it is added that "these functions give rise to certain difficulties and paradoxes which require attentive study and special precautions, but we have not to deal with them in this elementary treatise" (p. 16). Again (p. 28) the axiom of Frege which allows us to pass from two equal classes to the equivalence of the propositional functions (such as $x \epsilon a$) which correspond to them, is mentioned as giving rise to the difficulties discovered by Russell when it is applied to functions of the second order. Here again further developments are explicitly dismissed.

A translation of those parts of Frege's *Grundgesetze* which deal with function, concept, class, and relation is given in the *Monist* (1917, **27**, 114–27); and with this it is interesting to compare a previous account (*ibid.* 1916, **26**, 415–27) of Dedekind's work on logic and arithmetic.

Philip E. B. Jourdain (*Scientia*, 1917, **21**, 1–12) attempts to point out the function of symbolism in mathematical logic—a question discussed by Rignano and Peano in 1915 (*cf.* SCIENCE PROGRESS, **10**, 116 and 432). Jourdain also (*Monist*, 1917, **27**, 142–51) discusses the "entity" of a number or other logical thing as distinguished from its "existence."

N. Wiener (*Trans. Amer. Math. Soc.* 1917, **18**, 65–72) considers certain formal invariances in Boolean algebras; and E. V. Huntington (*Proc. Nat. Acad. Sci.*, Washington, D.C., 1916, **2**, No. 11) gives a set of five independent postulates for cyclic order.

With regard to the paper by F. Hartogs before mentioned in SCIENCE PROGRESS (1917, **11**, 453), the essential point in what is proved is that it is always possible to construct a well-ordered aggregate such that its cardinal number is greater than that of any given aggregate M. From this follows that, if we assume that all aggregates are comparable, any aggregate M can be well-ordered.

Arithmetic and Algebra.—W. Hope-Jones (*Math. Gaz.* 1917, **9**, 5–8) applies the principles of the theory of probability to approximations in arithmetic, and decides, for example, that

the common rule that "the approximation $\pi = 3\frac{1}{7}$ will generally get the first three figures of your answer right" is a sound working rule, because it will succeed in 843 cases out of 1,000, chosen at random, supposing the answer to contain π as a factor.

L. E. Dickson (*Bull. Amer. Math. Soc.* 1916, **23**, 109–11) gives an extension of the theory of numbers by means of correspondences between fields, and H. S. Vandiver (*ibid.* 111–4) has a note on the distribution of quadratic residues for a rational prime modulus.

The analytic theory of numbers might be classed under "analysis," but the subject will be dealt with under the present heading. We have thus one more example of how the nature of a branch of science cannot be described by means of a finite number of classifications.

G. H. Hardy has written (Cambridge, 1916) a report to the University of Madras on S. Ramanujan's mathematical work in England since 1914. Ramanujan's work is distinguished by strikingly original and unusual methods, and is on extremely important subjects in definite integrals, series, the Zeta function, the analytic theory of numbers, and so on. One of Ramanujan's last papers, of which some account is given in this report, but which was published since the report was printed, is a paper on the expression of a number in the form $ax^2 + by^2 + cz^2 + du^2$ (*Proc. Camb. Phil. Soc.* 1917, **19**, 11–21), where he shows that there are exactly 55 sets of values of a, b, c, d, for which this is true.

G. H. Hardy (*Mess. of Math.* 1916, **46**, 104–7) considers certain multiple integrals and series which occur in the analytic theory of numbers and in particular in his paper in the *Proc. Lond. Math. Soc.* for 1916 (**15**, 192–213; see SCIENCE PROGRESS, 1916, **11**, 268).

A. A. Bennett (*Proc. Nat. Acad. Sci.*, Washington, D.C. 1916, **2**, No. 10) extends the special algebraic work of H. B. Fine to general analysis.

Analysis.—W. A. Hurwitz and L. L. Silverman (*Trans. Amer. Math. Soc.* 1917, **18**, 1–20) consider the consistency and equivalence of certain definitions of the summability of divergent series. It is not by any means necessary that two different generalisations of the conception of convergence should be consistent, but the authors find that all definitions of summability of a certain class are so in fact.

G. H. Hardy and J. E. Littlewood (*Rend. Circ. Mat. di Palermo*, 1916, **41**, 1–18) continue and complete a paper of theirs published in 1912 relating to the summability of series by Borel's exponential method.

G. H. Hardy's shortened paper (*Compt. Rend.* 1916, **162**, 463–5; SCIENCE PROGRESS, 1916, **10**, 434) on the application of Abel's method of summation to the theory of ordinary Dirichlet's series was given in its English and fuller form in the *Quart. Journ. Math.* (1916, **47**, 176–92).

W. H. Young (*Proc. Lond. Math. Soc.* 1916, **15**, 354–9) gives simple proofs of certain results he had communicated elsewhere on functions of what he now calls "upper" and "lower" type (remember Baire's upper and lower semi-continuous functions).

Mrs. (Grace Chisholm) Young (*ibid.* 360–84) proves three fundamental theorems, already known to be true for special functions, on the derivates of a function which is restricted merely to be measurable and finite everywhere.

D. C. Gillespie (*Annals of Math.* 1915, **17**, 61–3) defines $f(x)$ to be "integrable in the Cauchy sense" if the general term in the sum whose limit is the integral is $f(x_{m-1})(x_m - x_{m-1})$, and "integrable in the Riemann sense," if it is $f(\xi_m)(x_m - x_{m-1})$, where ξ_m is any point in the interval $x_{m-1} \ldots x_m$. Since the usual—and correct historically—condition of the continuity of $f(x)$ is not added to Cauchy's criterion of integrability, Gillespie arrives at the paradoxical-sounding conclusion of the equivalence of integrability in Cauchy's sense with that in Riemann's sense.

The lectures on Lebesgue's integral given by C. de la Vallée Poussin of Louvain at Harvard University in 1915 have already been mentioned in SCIENCE PROGRESS (1916, **10**, 617). The same author developed the subject in lectures at the Collège de France in 1915–16, and has now published these lectures as a book: *Intégrales de Lebesgue, Fonctions d'ensemble, Classes de Baire* (Paris, 1916) in the collection of monographs on the theory of functions of which Émile Borel is the general editor. The book gives a very good picture of that branch of the theory of functions which may be said to date from Borel's new definition (1898) of the measure of aggregates, Lebesgue's work on integration and "additive functions of an aggregate," and the researches of Baire and Lebesgue on the analytical representability of functions by infinite series of other functions.

M. Fréchet and J. Pierpont (*Bull. Amer. Math. Soc.* 1917, **23**, 172–5; *cf.* Science Progress, 1916, **11**, 94) continue their discussion on Pierpont's definition of integrals.

S. Pollard (*Proc. Lond. Math. Soc.* 1916, **15**, 336–9) inquires how far the results of the older theory of Fourier's series, which is concerned with direct investigations into convergence, based mainly on Dirichlet's integral, can be deduced from the more modern results as to the summability of the series, based mainly on the properties of Fejér's and Poisson's integrals. Jordan has obtained a result in this order of inquiries, but his criterion deals with convergence throughout an interval; Pollard deduces Dini's more delicate criterion, dealing with convergence at a point, from Fejér's theorem.

G. H. Hardy and J. E. Littlewood (*Proc. Nat. Acad. Sci.*, Washington, D.C., 1916, **2**, No. 10) give a remarkable trigonometric series which is never convergent or summable for any value of its argument, and is thus not a Fourier's series; and a function which has no finite differential quotient for any value of its argument.

W. B. Ford (*Amer. Journ. Math.* 1916, **38**, 397–406) supplements some researches of Paul du Bois-Reymond, Dini, Hobson, and Lebesgue by pointing out a noteworthy class of functions ϕ such that the limit when n is infinite of the integral

$$\int_a^b f(x)\phi(n, x-a)dx$$

should take the well-known Fourier mean value of the arbitrary $f(x)$ at $x=a$, it being assumed that $f(x)$ satisfies suitable conditions in the neighbourhood of $x=a$.

Sir Ronald Ross and Miss H. P. Hudson (*Proc. Roy. Soc.* February 1, 1917) give the second part (*cf.* Science Progress, 1916, **11**, 93–4) of Ross's application of the theory of probabilities to the study of *a priori* pathometry. This part is occupied with the construction and examination of a number of hypothetical epidemics on the basis of the equations of the first part. The conclusions suggest that the rise and fall of epidemics may be explained by the general laws of happenings as studied. At the same meeting J. Brownlee investigated the periodicity of measles epidemics in London from 1703 to the present day by the method of the periodogram.

The problem of Lagrange in the calculus of variations is

that of minimising an integral $\int f(x, y_1, \ldots, y_n, y'_1, \ldots, y'_n)dx$, with respect to curves which join two fixed points and satisfy a certain system of differential equations. For this problem proofs of the necessity of the rule of Euler and Lagrange and the conditions of Weierstrass and Legendre are obtainable without the use of the second variation; but the case of Jacobi's condition is not so satisfactory. D. M. Smith (*Trans. Amer. Math. Soc.* 1916, **17**, 459–75) gives proofs of the necessity of both the conditions of Legendre and Jacobi by means of the second variation, but without the use of complicated transformations.

A. Dresden had obtained in 1908 formulæ for the second derivatives of the extremal integral arising in the problem of minimising or maximising the integral $\int F(x, y, x', y')dt$. In these formulæ the derivatives were expressed in terms of particular solutions of Weierstrass's form of Jacobi's differential equation, and were used to obtain further necessary conditions for a minimum when one or both end-points were variable along a curve or when curves with discontinuous slopes were admitted as solutions of the problem. Dresden (*ibid.* 425–36) uses the same general method in analogous problems involving integrals of a more general type, and also in the cases in which the unknown functions are further conditioned by differential and algebraic equations.

P. R. Rider (*Bull. Amer. Math. Soc.* 1917, **23**, 237–40) gives the corner conditions and the forms of the Carathéodory Ω-function for discontinuous solutions in the calculus of variations for the form of the problem considered by Bliss (1907, 1908), and for an analogous form of the space problem. We may refer also to Rider's note in the *Amer. Math. Monthly* (1917, **24**, 134–6).

On a theorem of H. M. Morse on the linear dependence of many analytic functions of one variable there are three notes by Morse, G. A. Pfeiffer, and G. M^c Green in the *Bull. Amer. Math. Soc.* for 1916 (**23**, 114–17, 117–18, and 118–22 respectively).

T. H. Gronwall (*Trans. Amer. Math. Soc.* 1917, **18**, 50–64), completes Cousin's work on the expressibility of a one-valued function of several complex variables as the quotient of two functions of whole character by giving precision to the conditions of two theorems stated by Cousin.

The researches of T. J. I'A. Bromwich (*Proc. Lond. Math. Soc.* 1917, **15**, 401–48) on the solution of dynamical problems by means of complex integrals, which is more general than the method (Routh, Heaviside) of normal co-ordinates, are of interest to mathematicians in connection with the work of Cauchy, Dini, and W. B. Ford (*Studies on Divergent Series and Summability*, New York, 1916) on the application of complex integrals to the summation of series of normal functions.

W. V. Lovitt (*Amer. Journ. Math.* 1917, **39**, 27–40) discusses some of the singularities of a contact transformation.

Tomlinson Fort (*ibid.* 1–26) generalises the fact that, if all the coefficients of a linear difference or differential equation have the same period ω, then, if $y(x)$ is a solution of the equation, $y(x+\omega)$ is also a solution. He treats equations of the second order, and the fundamental facts developed are so primarily with a view to their application, that he also gives, to self-adjoint boundary-value problems in one dimension.

W. E. Milne (*Bull. Amer. Math. Soc.* 1917, **23**, 166–9) establishes some asymptotic expressions in the theory of linear differential equations.

J. F. Ritt (*Trans. Amer. Math. Soc.* 1917, **18**, 27–49) studies a general class of linear homogeneous differential equations of infinite order with constant coefficients. The first part of his paper is devoted to the theory of the "entire differential operator of genus zero," and, in the second part, on the homogeneous equation of genus zero, Ritt finds it desirable to have information as to the possibility of resolving into partial fractions the reciprocal of a whole function of genus zero, and this forms the subject of another paper (*ibid.* 21–6).

T. H. Hildebrandt (*ibid.* 73–96) applies some of the conceptions of E. H. Moore's "general analysis" and some of the results of the general theory of integral equations to the theory of linear differential equations in general analysis.

É. Delassus obtained a canonical form useful for the study of systems of partial differential equations, but L. B. Robinson and Gunter found independently of one another in 1913 that the form is not absolutely general. Were the integration of the given differential system the only question of interest, there would be no need to improve the above form, in consequence of a theorem due to Riquier; but since canonical forms are often useful in the study of comitants of either differential or

algebraic systems, it is of interest that Robinson (*Amer. Journ. Math.* 1917, **39**, 95–112) has constructed a canonical form which has no exceptional cases and will apply equally to a differential system or a system of polynomials.

Geometry.—R. L. Moore (*Bull. Amer. Math. Soc.* 1917, **23**, 233–6) gives a proof of the simple fact *in analysis situs* that every two points of a continuous curve are the extremities of at least one simple continuous arc that lies entirely on that curve. The theorem can be almost at once applied to a curve in a space of any number of dimensions.

E. K. Wakeford (*Proc. Lond. Math. Soc.* 1916, **15**, 340–2) deduces a proof of the theorem of the double six of lines in space from a projective form of Miquel's theorem on the form of five parabolas, each of which touches four out of five straight lines in a plane.

J. Hodgkinson (*ibid.* 343–53) investigates the interdependence of the nine double points which are sufficient to determine uniquely a plane sextic curve.

Miss Hilda P. Hudson (*ibid.* 385) discusses the Cremona transformations of a plane sextic with ten double points.

H. Hilton and Miss R. E. Colomb (*Amer. Journ. Math.* 1917, **39**, 86–94) investigate the nature of the " orthoptic " locus (the locus of the intersection of two perpendicular tangents) of a given real plane algebraic curve.

L. C. Cox (*ibid.* 59–74) establishes a method which enables us to determine the Cremona transformations with seven fundamental points which correspond to a given linear transformation of the quartic curve.

L. P. Eisenhart (*Trans. Amer. Math. Soc.* 1917, **18**, 97–124) develops the theory of certain transformations of conjugate systems of curves on a surface.

A. E. Young (*Amer. Journ. Math.* 1917, **39**, 75–85) discusses the problem of determining all surfaces having lines of curvature which are either what Bianchi has named " isotherm-conjugate " or associate isotherm-conjugate.

ASTRONOMY. By H. Spencer Jones, M.A., B.Sc., Royal Observatory, Greenwich.

Stellar Evolution.—Much valuable work has been done recently by Mr. J. H. Jeans on the important subjects of cosmogony and stellar dynamics. For these investigations Mr. Jeans has

been awarded the Adams Prize of the University of Cambridge. Some of the results obtained have appeared in papers published recently in the *Phil. Trans.* dealing mainly with the equilibrium of rotating fluids, and the chief astronomical applications are given in a paper to appear in the *Memoirs R.A.S.* entitled, "The Part played by Rotation in Cosmic Evolution." The treatment of the subject necessarily involves much complicated mathematics and this fact may deter the average reader from giving to these papers the consideration which they merit. The excellent summary of the chief results obtained, freed from the analytical complications, which is given by Mr. Jeans in the *Observatory*, vol. xl. pp. 196–203, May 1917, is therefore the more welcome. The method of treating the subject is based on Poincaré's elegant theory of linear series and points of bifurcation, and this is briefly explained. As the physical conditions of a rotating mass change, the whole system of possible configurations of equilibrium fall into a "linear series"; on passing through such a series, with conditions gradually changing, stability can only be lost either (1) at a "point of bifurcation," where two series intersect in a configuration which is common to both, or (2) on passing through a "turning point" at which the variable parameter, which expresses the change in the physical conditions, becomes stationary and then decreases. Also at a point of bifurcation, at which the main series loses its stability, the branch series will be stable if its arms turn up (the parameter increasing) and unstable if they turn down (the parameter decreasing).

The simplest case of a homogeneous incompressible rotating mass is first considered, the density being assumed constant and the angular momentum being made to increase and being taken as the variable parameter. Starting from the spherical configuration, the sequence of configurations is traced through the well-known Maclaurin spheroids, through a point of bifurcation to the Jacobean ellipsoids which are initially stable, and then through another point of bifurcation to the pear-shaped figures of Darwin. Darwin thought he had proved these to be stable; Liapounoff, on the other hand, thought that they were unstable. The detailed investigations of Mr. Jeans prove that the branch series through the point of bifurcation turns down, indicating instability. The only possibility when this stage is reached is dynamical motion, and a violent dis-

ruption occurs into two masses rotating about one another, the ratio of the two masses being independent of the initial density or momentum.

The far more difficult problem of non-homogeneous and compressible masses is then treated. The conclusion is reached that the process of evolution is substantially the same as in the previous case, and that the ultimate result is two detached masses. This theory seems to have an obvious application in binary stars, which are so numerous in all parts of the sky. It is not easy, however, to test it in a decisive manner by means of the observational evidence provided by such systems, although the general agreement is satisfactory. A more decisive test is provided by triple and multiple stars, and an examination of the evidence leads to the conclusion that some such systems can be explained by rotational disruption, whilst others cannot.

The rotational theory was first put forward by Laplace to explain the existence of the solar system. Mr. Jeans examines whether such a system could be formed by any alternative sequence of processes; after trying various hypotheses he reaches the very important conclusion that "unless some new and at present unthought-of factor appears, we seem forced to conclude that rotational theory cannot explain the genesis of our solar system." The outstanding difficulty is the comparative absence of rotation.

Further investigations show also that the rotational hypothesis can account for the formation of spiral and possibly also of ring nebulæ, and that from it a reasonable scheme of cosmic evolution can be evolved. Starting from the condensing, irregular mass of gas, gradually acquiring rotation through tidal forces raised by other masses, this may evolve through a flattened shape into a spiral form, which may condense into streams of stars such as are seen in the Andromeda nebula, and then into a flat galaxy of stars which gradually becomes successively spheroidal and then almost globular. But the evolution of our own solar system still remains as great a mystery as ever.

Double Stars.—An important catalogue of double stars by M. R. Jonckheere is published in *Memoirs R.A.S.* vol. lxi. 1917. M. Jonckheere was director of the Observatory of the University of Lille, and is a well-known double star observer. During his exile in England he has completed the observations

necessary for this catalogue with the 28-in. refractor of the Royal Observatory, Greenwich. The catalogue includes all the double stars with a separation of less than 5" discovered from the date of the publication in 1906 of Burnham's general catalogue, and also those stars previously discovered, but not included by Burnham. The region of the sky covered extends from the North Pole to a north polar distance of 105°. The catalogue contains 3,950 stars, but the total number of known double stars within this area amounts to 9,724, of which the principal discoverers are Aitken (2,915), Jonckheere (1,282), Hussey (1,138), W. Struve (1,110), and Burnham (855). A study of the distribution of double stars leads to the conclusion that the ratio of the number of close double stars to single stars increases as the total density of the stars increases.

Celestial Mechanics.—A résumé of the lectures delivered at the Sorbonne during the first term of the year 1916–17, by Prof. H. Andoyer on the method of determining a planetary or cometary orbit from three neighbouring observations is printed in *Bulletin astronomique*, vol. xxxiv. 1917, pp. 36–67. The author follows generally the procedure of Charlier and Moulton, and combines the advantages of the classic methods, that of Gauss, and that of Laplace as recently modified by Leuschner. For a concise and clear summary of this by no means easy subject this paper can hardly be excelled.

Several important publications have been issued recently by certain observatories: amongst these may be mentioned:

"Photometric Researches: The Eclipsing Variables RV Ophiuchi, RZ Cassiopeiæ," by R. S. Dugan, *Contributions from the Princeton University*, No. 4, pp. 38, 1916.—RV Ophiuchi is found to consist of two stars, one of which emits nearly five times as much light as the other, and has a surface brightness twelve times as great, the radius of the brighter being two-thirds that of the fainter star. The distance between their centres is five times the radius of the fainter star. The sides of the stars facing one another are brighter than the opposite sides, and both stellar discs are elliptical. There is evidence of a difference in brightness of the advancing and following sides of the brighter star. The star RZ Cassiopeiæ was interesting because it appeared as though it might be the first eclipsing variable to be discovered which had no secondary minimum. The observations show that this is not the case,

and amongst other results clearly revealed a variation in the period. This is discussed by Mr. Dugan in *M.N., R.A.S.* vol. lxxvi. p. 729, 1916.

"Determination of the Difference of Longitude between Washington and Paris, 1913–14," reduced by F. B. Little and G. A. Hill: *U.S. Naval Observatory, Washington, Publications*, Second Series, vol. ix. appendix, pp. 100, 1916.—This publication contains an account of the observations for the direct determination by means of wireless telegraphy of the difference of longitude between Washington and Paris, together with the definitive reduction and discussion of the observations.

"Catalogue of Proper Motion Stars, Part II," by J. G. Porter, *Pub. of the Cincinnati Observatory*, No. 18, pp. 113, 1916. —Part I of this work, previously published, covered the hours 0^h—6^h right ascension; the present publication continues the results up to 15^h of right ascension. The observations and their reduction are recorded in the same way as in the previous part. The remaining part of the sky will subsequently be published in Part III. The collection and calculation of a large number of appreciable proper motions of stars in a large area of the sky such as is contained in this work is of considerable importance for statistical discussions in astronomy.

"Researches on the Proper Motions of Stars in the Helsingfors Photographic Zone: Part I, 9 to 12," by R. Furuhjelm, *Acta Societatis Scient. Fennicæ*, vol. xlviii. No. 1, pp. 190, 1916. —At the international astrographic congress of 1909, support was given to the proposal that the astrographic zones should be reobserved with a view to deriving proper motions from a comparison of the plates taken at two epochs. The Helsingfors zones were rephotographed from 1909 to 1913, the original photographs being obtained in 1892 to 1896. M. Furuhjelm has compared corresponding plates in a stereocomparator and has measured differentially the proper motions of all stars which were revealed in this way to be in sensible motion. This volume contains the results of the measurements from 9h. to 12h., 1,016 proper motions being derived. These results are followed by a discussion of the accuracy of measurement and of the distribution in magnitude and direction of the proper motions. It is intended to complete the remaining hours in the same way and then to discuss the whole of the material so obtained.

PHYSICS. By James Rice, M.A., University of Liverpool.

In the January number of the *Phil. Mag.* Dr. R. T. Beatty gives the first part of an investigation which he is carrying out on the energy emitted by luminescent hydrogen in the visible region, and the partition of it among the various qualities corresponding to the lines H_α, H_β, H_γ, and H_δ. A Geissler tube was used as the source, and the light from it was analysed by a specially designed monochromator. The intensity was measured by a photoelectric cell, previously calibrated by means of a carbon filament glow lamp. Special devices were used to measure the potential difference between the electrodes of the hydrogen tube and the current passed through it. The tendency for the tube to become heated by the passage of the discharge, and so alter the pressure of the gas, was obviated to some extent by employing a clockwork arrangement in the primary circuit of the induction coil which alternated periods of quiescence with periods of emission on the part of the tube. By so doing, the tube was kept in a constant condition, and readings of current and potential could be repeated time after time in a satisfactory manner. The photoelectric cell was placed with its window close to the emergence slit of the monochromator and the current through it, and therefore the intensity of the light falling on it was measured by an ingenious null method. The tube was used with the hydrogen dry, and also in the presence of water vapour, and the currents corresponding to the incidence on the cell of the light corresponding to the four lines mentioned were measured. Results were obtained over a wide range of pressures, and the ratios of the light intensities in the various lines tabulated and graphed. According to Bohr's theory of the Rutherford atom of hydrogen, its single electron may rotate around its positive nucleus in any one of a series of circular orbits whose radii bear to one another the same ratios as the squares of the natural numbers, and radiation or absorption of light occurs when the electron leaves one of these orbits and assumes another. Thus the lines of the Balmer series are due to the passage of the electron to the second circular path from any of the paths outside this, H_α corresponding, for instance, to a passage from path 3 to path 2, H_β to passage from path 4 to path 2, and so on. In the normal condition the electron of a hydrogen atom is rotating in the

first or innermost path, and it therefore must absorb sufficient energy to eject it from this path to one of the outer ones before the atom can become luminescent. In the hydrogen tube an atom would acquire this energy by collision with a free electron possessing sufficient energy. An interesting question, therefore, arises as to the relative intensities of the various lines at different pressures. Thus as the pressure in the tube is reduced, and the mean free path of the free electrons increased, their velocity at the moment of an encounter with an atom would be increased also ; and so it might be expected that a greater number of the luminescent atoms at a given instant would be in the state in which the electron was in paths 4, 5, or 6, as compared with those in the state corresponding to path 3. If that were so, it might also be expected that the relative intensity of the light corresponding to the H_β, H_γ, H_δ lines, as compared to the H_α light, would increase with diminution of pressure. The author, however, concludes from an examination of his own and other experimental work that this view is not justified, and that the energy distribution in the lines of the Balmer series is independent of the energy of the colliding electron (or molecule, if excitation is due to impact of canal rays), which is responsible for the luminescence of an atom. By admission of water vapour it is possible to vary the free path of the atom itself, and here the results seem to justify the statement that the intensity distribution is affected by the mean free path of the luminescent atom and the nature of the molecules in its vicinity whose fields of force act on it. If the mean free paths of two atoms, one emitting H_α light and the other H_β light, are less than the distances which the atoms travel during emission, the intensity ratio will remain constant, and be independent of the particular pressure used, so long as it is consistent with the assumption made. Again, if the pressures were so low that the mean free paths were all longer than the distances travelled during emission, a constant ratio of intensities would also result, although a different constant from the former. But within an intermediate range of pressures which permitted one of the atoms to complete its emission in a shorter length than its free path, but destroyed the radiating power of the other by a premature collision, due to the relative shortness of its free path, one would expect the ratio H_β/H_α to alter.

Some evidence in support of these conclusions is to hand, and the author proposes to continue his researches on these and other points of interest, and communicate them as they are completed.

A very interesting paper on the dynamical theorem generally referred to as the "Theorem of Equipartition of Energy" and its bearing on modern views on Radiation and the Quantum Theory appears in the same number of the *Phil. Mag.* It is too long and too closely reasoned to allow of a satisfactory summary, but any one interested in these matters will be well advised to read it. The author, Dr. W. F. G. Swann, exposes in a clear and lucid manner two pitfalls into which careless reasoning may land the unwary when applying this theorem, and incidentally combats certain views as to the impossibility of reconciling the equipartition theorem with the quantum hypothesis, which have almost become a matter of faith with the orthodox.

In the same number of the *Phil. Mag.* is printed a paper by E. A. Biedermann on the energy of an electromagnetic field. He draws attention to a discrepancy which exists when one measures the magnetic energy density at a point by means of the well-known formula $H^2/8\pi$, according as H is taken to represent the mean value of the magnetic force at the point, or the *instantaneous* value of the magnetic force. Of course no discrepancy arises when one considers the current giving rise to the magnetic field as distributed *uniformly* throughout conductors. It is when one adopts the modern view of electronic currents which concentrates the current in the paths of a number of discrete particles, that a difference in results arises according as one or other interpretation of H is adopted. The author proposes to resolve the difficulty by adopting a more general formula for the magnetic energy density, which may be summarised thus: H, as is well known, can be obtained by taking the *vector* product of the electric intensity at a point due to a charged particle and the velocity of that charge and finding the resultant of all these vectors at the point in question. The author proposes in the formula for the magnetic energy density to add to H^2 the square of another quantity G, which is the result of summing the *scalar* products of electric intensity and velocity. On this assumption the author shows that no discrepancy arises, and further,

that it allows one to formulate a Lagrangian function leading by an application of the Lagrange equations to Maxwell's equations for the field. The paper is subjected to some criticism by Dr. G. H. Livens in the March number of the *Phil. Mag.*

In the February number of the *Phil. Mag.* Lord Rayleigh discusses the methods employed for detecting small optical retardations, and the test introduced by Foucault and modified by Cheshire for discovering lack of perfection in optically figured surfaces.

In *Nature* of March 1, Dr. Todd proposes an interesting thermodynamical explanation of the temperature coefficient of the gravitational constant, evidence for which Dr. Shaw has recently claimed to discover. [See SCIENCE PROGRESS, Jan. 1917; Rec. Adv.] The fundamental assumption is that a movement of an attracted mass to or from an attracting body involves a change of temperature. A cycle of the Carnot type can be constructed, and an expression for the attraction between two masses developed which contains a term depending on temperature in addition to the usual Newtonian formula. Some interesting discussion is printed in the following numbers of *Nature.*

In the January number of the *Physical Review* appears a theoretical paper by A. H. Compton on the intensity of X-Ray reflection which, among other results, supports the theory of the atom propounded by Bohr.

In the same number, C. D. Child gives an account of some work, supporting the view that light is produced by the recombination of ions, or, in other words, that the vibrations of the atoms producing light are not started when the atoms are broken into ions, but when the ions recombine into atoms.

PHYSICAL CHEMISTRY. By Prof. W. C. McC. LEWIS, M.A., D.Sc. University, Liverpool.

Osmotic Pressure (continued).—We now come to the problem of how to account for the phenomenon of osmotic pressure at all. For the sake of simplicity we shall restrict ourselves to the ideal solution; the ideal solution being one in which the volume of the mixture is exactly equal to the sum of the volumes of the two constituents separately. In such a case the funda-

mental idea advanced by Tinker is that the *solvent* pressure in the solution is *less* than the *solvent* pressure in the pure solvent itself which lies on the other side of the membrane. To see how this might occur, let us think of equal volumes of two separate gases, A and B. If these are mixed, and the resulting volume is just double the original volume of either, it follows that the partial pressure of each constituent—say the substance A—in the resulting mixture is exactly half of its value in the original state. It is to be observed that this diminution in the pressure exerted by A is brought about, although the effective space occupied by any single molecule is just the same before and after mixing, *i.e.* the space per molecule $= \frac{\text{total number of molecules}}{\text{total volume}}$, and in the case considered we have doubled the number of gas molecules and doubled the volume at the same time. To return to the case of the solution, the partial liquid pressure of the constituent, which we call the solvent, is diminished by the presence of the solute, for exactly the same reason that the partial pressure of the gas A was diminished.

If therefore the solvent and solution are separated by a membrane, permeable to the solvent, impermeable to the solute, the solvent pressure on the pure solvent side is greater than the solvent pressure on the solution, and therefore some solvent is forced into the solution. That is, the phenomenon of osmosis occurs. Further, we can apply an external pressure to the surface of the solution by means of a piston or an inert gas, and so prevent osmosis taking place. This applied pressure is numerically equal to the osmotic pressure of the solution, and, in fact, the method is employed experimentally to determine osmotic pressure. By compressing the solution in this way we obviously diminish the free space in the solution, and, therefore, cause the solvent pressure in the solution to rise, until it is equal to the solvent pressure in the pure solvent itself, and no further osmosis can take place, *i.e.* the solvent is now passing in and out of the membrane at the same rate.

Now we know that liquids are very incompressible. It follows therefore that there must be very little free space in liquids. If this were not the case, it would be quite inconceivable that the application of an external pressure of a few atmospheres by the piston could sensibly affect the free space,

and, consequently, the value of the solvent pressure. As a matter of fact, the free or unoccupied space in a liquid is about one-fifth to one-tenth of the total volume occupied by the liquid.

It is thus seen that the osmotic pressure may be explained as due to a difference between the value of the solvent pressure π in the pure solvent and the value of the solvent pressure π' in the solution. This is the new concept of osmotic pressure, especially for solutions which approximate to the ideal case. In the case of non-ideal solutions the osmotic pressure is a more complex phenomenon, but the underlying idea is the same. It is not proposed to enter into the case of non-ideal solutions in this place. One point only remains to be considered in connection with the ideal solution.

It has been stated, that the osmotic pressure depends upon the difference of the π values. This must not be taken as meaning that the osmotic pressure P is simply $\pi - \pi'$. The actual relation between them is easily shown to be, in the case of a dilute solution :

$$P = \frac{RT}{V} \frac{\pi - \pi'}{\pi'}$$

where V is the volume of one grammolecule of the solvent and R is the gas constant. Tinker has shown further that this expression leads at once to the van 't Hoff law for the osmotic pressure. The new concept of osmotic pressure is therefore quite in agreement with the striking experimental facts demonstrated by van 't Hoff and is capable of accounting for them in a satisfactory manner. We are therefore no longer tied to the bombardment view which attributes the osmotic effects to a simple gas pressure exerted by the solute. Several other relationships, which cannot be discussed here, may likewise be deduced, with the help of the concept of solvent pressure. The position is therefore a hopeful one, and it may not be long before we possess a really comprehensive and satisfactory theory of dilute solutions not simply from the thermodynamic standpoint, but from the molecular standpoint as well.

Addendum.—The problem of Osmotic Pressure was recently the subject of a discussion held by the Faraday Society. The reader is referred to the report of this meeting which will appear in the next issue of the Transactions of the Faraday Society.

INORGANIC CHEMISTRY. By C. Scott Garrett, D.Sc.

Elements.—The question of the atomic weight and other physical characteristics of lead from different radioactive sources has been considerably advanced in papers which have recently been published by Richards and Wadsworth (1 and 2), and by Æchsner de Coninck and Gerard (3). As regards the density at 19·94° C., lead from Norwegian cleveite showed a density of 11·273, whilst Australian radio-lead gave a density of 11·289, when determined in the same manner, as compared with 11·377 for ordinary lead not of radioactive origin. The atomic weight of the Australian radio-lead was found to be 206·35, and that of the Norwegian lead 206·085, so that the decrease in density is almost exactly proportional to the decrease in atomic weight as between the lead from various sources. The atomic volumes, therefore, of all three classes of lead are practically constant, the figures being 18·281, 18·279, 18·277 for Norwegian, Australian, and ordinary lead respectively. In their most recent paper, Richards and Wadsworth have extended their results on the atomic weight of materials of different origins. The new results are:

Ordinary lead	207·18
Radio-lead (Colorado)	207·00
Radio-lead (Australia)	206·34
Radio-lead (bröggerite, Norway) . .	206·12
Radio lead (cleveite, Langesund, Norway) .	206·08

The more carefully the samples were selected, the lower were the figures obtained, so that the authors assume that the higher results are due to accidental mixture with ordinary lead. No new lines appear in the ultra-violet or visible spectrum of any of these samples. It can, therefore, be assumed that lead has a dual structure. Further, no relation could be traced between the magnitude of the radioactivity and the lowering of the atomic weight below the figure for ordinary lead. De Coninck and Gerard's results for the atomic weight of lead tend to confirm the foregoing results. After careful and extended determinations, these workers find that the mean value of the atomic weight of ordinary lead is 206·98, whilst that for lead from uranium minerals is 206·71 after eliminating as far as possible all material of non-radioactive origin.

Catalytic Reactions.—A new field of work has been opened up in a series of investigations made by Lidov on the reactivity of carbon (4). Lidov has attacked the question as to whether carbon unites with nitrogen at the ordinary temperature. Using finely-divided carbon from cotton or sulphite cellulose, in which a metallic catalyst is deposited by having previously moistened the carbonaceous material with a solution of ferrous lactate and calcining in a current of hydrogen, he finds that after the carbon has been sealed up with CO_2-free air for several months, reaction takes place in two successive stages. At first the carbon unites with nitrogen to form active α-monocyanogen, $-C{\equiv}N$, and inactive β-monocyanogen, $-N{\equiv}C$. In this stage the oxygen of the air takes no part, reaction being due to the nitrogen atom. The second stage consists of the addition of oxygen forming respectively α-oxan, $O-C{\equiv}N$, and β-oxan, $O-N{\equiv}C$. Under suitable conditions peroxan, O_2CN, can also be formed.

α-monocyanogen is soluble in cuprous chloride solution, and its oxidation product α-oxan is very similar to carbon dioxide. The inactive compound is insoluble in cuprous chloride solution, but its oxidation product β-oxan is soluble. β-oxan differs from carbon dioxide in that its salts are easily decomposed by heat, and it is not soluble in aqueous alkali. The author claims that his results account for the variation in the amount of nitrogen in the air at different times of the year. Experiments have also been carried out with carbon activated with other metals, chiefly lead and nickel. Carbon activated with lead does not lose its activity for a very long time, whereas that activated with iron and nickel loses its activity rapidly, probably because the metal precipitated on the surface of the carbon becomes converted into its higher oxide.

The loss of activity with these latter metals is not, however, absolute, but is merely a matter of diminution in the rate of reaction. These results are shown by the fact that the amount of air treated with the activated carbon which is required to precipitate barium hydroxide solution is always less for lead than for iron or for nickel. Moreover, the density of this mass of air is always less than that of the mass from carbon activated with iron or nickel, and is on the average very near the density of oxan.

Another interesting observation in this connection is that

the barium salt, which is produced by the continued absorption of air from carbon activated with iron or nickel after the potency of these catalysts appears to diminish, liberates no gas, decomposes acidulated boiling potassium permanganate solution, and gives with solutions of metallic salts precipitates which gradually decompose with the liberation of gas. These observations indicate that the product is either an oxidized or polymerised oxan.

The analogy between the oxans and carbon dioxide at once suggests a search for the former in nature amongst the habitats of the latter gas. Thus Lidov has extended his investigations to the atmosphere, natural waters and mineral carbonates. Different limestones were decomposed with phosphoric acid and the resultant gas was passed over calcined magnesium. The latter material, especially if it contains aluminium, potassium or calcium or magnesium silicide as impurities, is an absorbent for the active monocyanogen. The results of these experiments show that the density of the gas from calcite is considerably lower than that of carbon dioxide. That from aragonite or chalk almost exactly corresponds to the density of carbon dioxide, whilst that from marble and other dense limestones is greater than that of carbon dioxide. Undoubtedly, then, the natural carbonates contain varying amounts of active α-monocyanogen—the calcites consisting indeed chiefly of calcium α-oxanate.

Calcite thus contains both calcium oxide and nitrogen, and on this account ought to be very useful as a fertiliser. Lidov's investigations perhaps require further substantiation before they meet with general acceptance by chemists, but they are undoubtedly of the highest significance, and mark one of the most important advances which have been made in the domain of inorganic chemistry in recent years. They may, perhaps, be invoked to explain the crystallographic differences in the alkaline earth carbonates, and they may even have far-reaching results in the domain of the practical fixation of atmospheric nitrogen. We have no hesitation in laying particular stress on the importance of this work, and the necessity for its further confirmation and investigation.

Another reaction which is to be credited to the extending use of catalysts in chemical reaction is that due to Haworth and Irvine (5), in the case of the preparation of hypochlorous

acid. As is well known, small quantities of this acid are obtained by passing chlorine into water in the presence of precipitated mercuric oxide. These investigators, however, have found that a better yield is obtained by the use of other salts or oxides, more particularly those of copper, and especially copper oxychloride. A 2 per cent. solution of the acid can easily be obtained by bubbling chlorine through agitated bottles containing water and the catalyst—in the case of copper oxychloride, 10 grams per 1¾ litres water.

From this solution after precipitation of the copper, the acid may be collected by distillation. Owing to the extended use of hypochlorous acid in modern drug preparation, these results are of distinct importance.

References

1. Density of Radio Lead from Pure Norwegian Cleveite (*J. Amer. Chem. Soc.* 1916, **38**, 1658).
2. Atomic Weight of Lead of Radioactive Origin (*ibid.* 1916, **38**, 2613).
3. Atomic Weight of Lead (*Compt. rend.* 1916, **163**, 514).
4. (*a*) Combination of Carbon and Nitrogen at the Ordinary Temperature; (*b*) Oxidation of Carbon by Air at Low Temperatures in Presence of Iron and other Metals; (*c*) Carbon Dioxide from Natural Limestone. Reprints from the Reports, Kharkov Technol. Inst. 1916 (*J. Soc. Chem. Ind.* 1916, **35**, 1260).
5. Preparation of Hypochlorous Acid, Eng. Patent 12912 (*ibid.* 1916, **35**, 1059).

List of other Important Papers

Catalytic Oxidation of Ammonia. E. B. Andersen. (*Zeitsch. Elektro. Chem.* 1916, **22**, 441.)

Sub-Salts of Lead. H. G. Denham. (*Trans. Chem. Soc.* 1917, **111**, 29)

Sulphides of Iron. V. Rodt. (*Zeitsch. angew. Chem.* 1916, **29** (i), 422.)

Sodium Potassium Double Carbonates. J. W. Bain and C. E. Oliver. (*Trans. Roy. Soc. Canada*, 1916, iii. **10**, 65.)

Silver Peroxynitrate. M. J. Brown. (*J. Physical Chem.* 1916, **20**, 680.)

Subsidiary Valencies—Complexes with Sulphur Dioxide. F. Ephraim and I. Kornblum. (*Ber.* 1916, **49**, 2007.)

The Corrosion of French, Bohemian, and German Glass Vessels. P. Nicolardot. (*Compt. rend.* 1916, **163**, 355.)

Sulphides of Barium. L. Guitteau. (*Ibid.* 1916, **163**, 390.)

Tautomerism of Nitrosylsulphuric Acid. J. Biehringer and W. Borsum. (*Ber.* 1916, **49**, 1402.)

ORGANIC CHEMISTRY. By P. Haas, D.Sc., Ph.D., St. Mary's Hospital Medical School.

In a previous article (Science Progress, 1915, **37**, 127) attention was drawn to several German patents for the production

of acetic acid from acetylene and the significance of these was pointed out in view of the possible shortage of the usual sources of supply. The question is evidently occupying the minds of French chemists as well, since a French patent (No. 479,656) has recently been granted. It is claimed by the patentee that the mercury salt required for bringing about the combination between acetylene and water should be dissolved in acetic acid. The oxidation of the acetic aldehyde to acetic acid is effected by adding oxidising agents such as hydrogen peroxide, perborates, permanganates, percarbonates, dichromates, ozone, etc., or by means of oxygen or air in the presence of catalysts such as cerium salts, vanadium pentoxide and other substances. A continuous production of acetic acid is obtained by passing 130 parts of acetylene and 80–100 parts of oxygen into a mixture of 400 parts of glacial acetic acid, 100 parts of water, 50 parts of mercuric nitrate, and 10 parts of cerium oxide at 50–100°. From time to time the acetic acid is drawn off and distilled, the residue being returned to the reaction vessel.

The catalytic dehydrating action of alumina at high temperatures has been known for some time and the property has recently been employed by C. D. van Epps and E. Emmet Reid (*J. Amer. Chem. Soc.* 1916, **38**, 2128) for the preparation of acetonitrile by passing acetic acid vapour mixed with a moderate excess of ammonia over alumina heated at 500°. The yield may be as high as 85 per cent., but is influenced by the activity of the catalyst and the rate of flow of the gases, and is lowered by the presence of water in the acetic acid.

Reference has already been made in these columns (Science Progress, October 1916, No. 42, p. 280) to the possibility of the purine ring being synthesised in the animal body from the two degradation products of protein, arginine and histidine. Further evidence in support of this view is furnished by a paper by H. Ackroyd and F. G. Hopkins (*Biochem. J.* 1916, **10**, 551). It has already been observed that the removal of arginine and histidine from the diet of rats was accompanied by a rapid fall of body weight and a diminished excretion of allantoin, the end point of protein metabolism in rats; it is now found that the removal of either one of these two acids does not cause a loss of body weight, or in other words that equilibrium can be maintained in the absence of one of these acids, but not of both. This is explained by assuming that in meta-

bolism each of these acids can be changed into the other. The removal of tryptophan or of vitamines from the diet produces an even greater nutritional disturbance than the absence of the two above-mentioned substances, but there is no corresponding falling off in the excretion of allantoin, which strongly supports the view that arginine and histidine play an important part in purine metabolism. Work on somewhat similar lines is being carried out by Osborne and Mendel (*J. Biol. Chem.*, 1917, **29**, 69) who have shown that corn gluten is deficient in lysine and tryptophan and is therefore not capable of promoting the growth of white mice unless supplemented by the addition of other proteins containing these acids as, for example, caseinogen, lactalbumen, edestin, beef, brewers' grains, etc. The best protein for the purpose is lactalbumen. It suffices to add relatively small quantities of these supplementary proteins, and in any case much less than would be sufficient to maintain growth in the animals experimented on.

As a further illustration of the significance of the various constituents of ordinary food, a paper by W. Stepp (*Zeitsch. Biol.* 1916, **66**, 365) may be quoted. It is there shown that lipoids such as lecithin, kephalin, cerebrone, cholesterol, etc., are essential to life, and that dog biscuits from which lipoids and vitamines have been removed by extraction with alcohol are unable to maintain white mice in health, and that neither vitamines nor lipoids alone are sufficient to render such biscuits suitable for the maintenance of life.

Further light has recently been thrown on the constitution of yeast nucleic acid. The nucleic acids in combination with protein form the highly important nucleoproteins, the chief constituents of cell nuclei. The two best-known nucleic acids are those derived from the thymus gland and from yeast and the prevailing opinion is that all nucleic acids are identical with one or other of these two compounds. By hydrolysis thymus nucleic acid can be split up into phosphoric acid, two purine bases guanine and adenine, and two pyrimidine derivatives thymine and cytosine, together with levulinic acid derived most probably from a hexose; the products of hydrolysis of yeast nuclei are essentially the same, only that the places of thymine and levulinic acid are taken by uracil and ribose respectively. Partial hydrolysis of thymus nucleic acid yielded a so-called nucleotide $C_{11}H_{17}N_2PO_{10}$ which is a com-

pound of phosphoric acid with a carbohydrate and a pyrimidine complex, as is shown by its behaviour on hydrolysis.

$$\begin{array}{l} HO \\ O{=}P - \underbrace{C_6H_{10}O_4 - C_5H_5N_2O_2}_{\text{Nucleotide}} + 2H_2O = H_3PO_4 + C_6H_{12}O_6 + \underbrace{C_5H_6N_2O_2}_{\text{Thymine}} \\ HO \end{array}$$

On the other hand, fermentation of nucleic acid yielded a nucleoside which was proved by hydrolysis to be a compound formed of a carbohydrate with a purine base ; thus

$$\underbrace{C_6H_{11}O_5 - C_5H_4N_5O}_{\text{Nucleoside}} + H_2O = C_6H_{12}O_6 + \underbrace{C_5H_5N_5O}_{\text{Guanine}}$$

Thymus nucleic acid is accordingly looked upon as a tetranucleotide formed by the union of four mononucleotides which differ only in their nitrogen ring components. In a recent communication W. Jones and B. E. Reid (*J. Biol. Chem.* 1917, **29**, 111 and 123) have shown that by the action of dilute ammonium hydroxide yeast nucleic acid yields a dinucleotide containing adenine and uracil of the formula

$$\begin{array}{l} HO \\ O{-}P - O - C_5H_7O_2 - \underset{\text{Adenine complex}}{C_5H_4N_5} \\ HO \qquad\quad\;\; | \\ \qquad\qquad\quad\; O \\ HO \qquad\quad\;\; | \\ O{-}P - O - C_5H_7O_2 - \underset{\text{Uracil complex}}{C_4H_3O_2N_2} \\ HO \end{array}$$

The chief point of interest is that the two mononucleotides are here united to each other through their carbohydrate complexes and not through the phosphoric acid residues, as was at one time thought likely. This is proved by the fact that the dinucleotide yields a tetrabrucine salt and must consequently have four hydroxyl groups. From the behaviour of yeast nucleic acid itself towards reagents it is concluded that all the four nucleotides in this compound are similarly united to each other through their carbohydrate groups.

During the quarter under review a series of important papers have been published by Willstätter and his pupils on the Anthocyanines, or red and blue colouring matters of a number of common flowers. The papers were published in the *Annalen*, 1916, **412**, 113–251, and it is hoped to give an account of some of their work in a future number.

Several important papers connected with carbohydrates

have recently appeared. Space will not permit of their being reviewed here, but a few references may be given: Dale (*J. Amer. Chem. Soc.* 1916, **38**, 2187) on the Preparation of Bromacetyl-glucose, etc.; Irvine and Robertson (*J. Chem. Soc.* 1916, **109**, 1305), Evidence indicating the existence of a new variety of fructose, etc.; Haworth and Shaw (*J. Chem. Soc.* 1916, **109**, 1314) on the Structure of Sucrose.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., F.G.S., University, Glasgow.

Stratigraphical and Regional Geology

Ulrich, E. O., Correlation by Displacements of the Strand-line, and the Function and Proper Use of Fossils in Correlation, *Bull. Geol. Soc. Amer.* 1916, **27**, 451–90.

Schuchert, C., Correlation and Chronology in Geology on the Basis of Palæogeography, *ibid.* 491–514.

Matthew, W. D., Methods of Correlation by Fossil Vertebrates, *ibid.* 515–24.

Knowlton, F. H., Principles governing the Use of Fossil Plants in Geologic Correlation, *ibid.* 525–30.

Schuchert, C., Silurian Formations of South-Eastern New York, New Jersey, and Pennsylvania, *ibid.* 531–54.

Foerste, A. F., Upper Ordovician Formations in Ontario and Quebec, *Geological Survey of Canada, Memoir* 83, 1916, pp. 279.

MacKenzie, J. D., Geology of Graham Island, British Columbia, *ibid. Mem.* 88, 1916, pp. 221.

Darton, N. H., Geology and Underground Water of Luna County, New Mexico, *United States Geological Survey, Bull.* 618, 1916, pp. 188.

Andrews, E. C., Notes on the Structural Relations of Australasia, New Guinea, and New Zealand, *Journ. Geol.* 1916, **24**, 751–76.

Bailey, E. B., and Maufe, H. B., The Geology of Ben Nevis and Glen Coe, *Mem. Geol. Surv. of Scotland*, 1916, pp 247.

Hughes, E. W., Geology of Cil-y-Coed District (Carnarvonshire), *Geol. Mag.* (6), 1917, **4**, 12–25, 75–80.

Strahan, A., Geology at the Seat of War, *ibid.* 68–74.

The nature of the papers forming a symposium on the subject of stratigraphical correlation by Ulrich, Schuchert, Matthew, and Knowlton, is sufficiently indicated by their titles. Ulrich emphasises the importance of diastrophic criteria in the determination of the natural divisions of geological history. He also insists on the value of minute differences between fossils for stratigraphic purposes, based on the belief that combinations of biologically unimportant characters can have existed but once and only for a short period of time.

In his interesting work on the larger geological relations of Australasia, New Guinea, and New Zealand, Andrews comes

to the conclusion that each of these is a distinct geological province. Australia and New Zealand appear to have grown sympathetically in response to some simultaneous dominating agency; but in New Guinea the earth-movements appear to be opposed to those of Australia with a tendency to fill the intervening negative area.

The new Survey Memoir on Ben Nevis and Blen Coe contains some profoundly interesting material for students of Highland geology, and for geologists in general. Original contributions to science are to be found in at least four subjects: the origin of the topography; the structure and succession of the Highland schists; the vulcanology and petrography of the igneous rocks of the Old Red Sandstone; and the contact-metamorphism of schists, sediments, and igneous rocks by the Old Red Sandstone granites.

In his paper on geology at the seat of war Dr. Strahan shows the manifold uses of geology in modern military operations, and discusses the water supply from the geological formations along the western front.

Petrology

LACROIX, A., Sur quelques roches volcaniques mélanocrates des Possessions françaises de l'Océan Indien et du Pacifique, *Comptes Rendus*, 1916, **163**, 177-83.

—— La Constitution des roches volcaniques de l'Archipel des Comores, *ibid.* 213-19.

—— La constitution des roches volcaniques de l'extrême Nord de Madagascar et de Nosy Bé; les ankaratrites de Madagascar en général, *ibid.* 253-8.

—— Les syenites à riebeckite d'Alter Pedroso (Portugal), leurs formes mésocrates (lusitanites) et leur transformation en leptynites et en gneiss, *ibid.* 279-83.

RASTALL, R. H., and WILCOCKSON, W. H., The Accessory Minerals of the Granitic Rocks of the English Lake District, *Q.J.G.S.* 1917, **71**, Part 4 for 1915, 592-622.

MENNELL, F. P., The Rocks of the Lyd Valley, above Lydford, *ibid.* 623-38.

FOYE, W. G., Are the "Batholiths" of the Haliburton-Bancroft Area, Ontario, correctly named? *Journ. Geol.* 1916, **24**, 783-91.

HOLMES, A., A Mineralogical Classification of Igneous Rocks, *Geol. Mag.* (6), 1917, **4**, 115-30.

BENSON, W. N., and others, British Antarctic Expedition, 1907-9: Reports on the Scientific Investigations, Geology, Vol. II., *Contributions to the Palæontology and Petrology of South Victoria Land*, London, 1916, pp. 270.

In his series of papers on the volcanic rocks of the French possessions in the Indian and Pacific Oceans, Prof. Lacroix has begun the finer discrimination of the rocks classed under the omnibus term basalt. He distinguishes the melanocratic

facies of felspar-basalts as "felspathic picrite," an ill-chosen name. A neighbouring rock distinguished by the predominance of pyroxene over olivine is called *ankaramite*, and the melanocratic fancies of nepheline-basalt *ankaratrite*, both terms being derived from Madagascan localities. This is a step in the right direction, as terms like "basalt" are fast becoming useless owing to the great variety of rocks included under them. The same author gives the name *lusitanite* to a mesocratic form of riebeckite-syenite found at Alter Pedroso, Portugal. This rock contains 50–60 per cent. of riebeckite, and is associated with the normal leucocratic riebeckite-syenite.

The useful research of Rastall and Wilcockson on the accessory minerals of the Lake District was carried out by the method of crushing large samples and panning the resulting sands. The results showed, as might have been expected, that thin sections are entirely inadequate to give an idea as to the amount, or even kind, of the accessory minerals present in the rocks. An unexpected feature was the universal occurrence of pyrrhotite and the rarity of magnetite as the prevalent ore-mineral. This work has an obvious bearing on the question of the origin of the heavy minerals in sedimentary rocks.

Mennell has described contact-metamorphism by the Dartmoor granite, which has converted the adjacent Carboniferous sediments into interesting cordierite- and andalusite-bearing rocks.

Foye concludes that the so-called granite batholiths of Ontario were formed by concordant injection into a fissile limestone terrane, which is consonant with the remarkable banding of most of the rocks in this district. He suggests the term *stromatolithic* as a better description of the structure and mode of intrusion.

In his classification paper Holmes proposes a five-fold division of igneous rocks established upon the "saturation" criteria of Shand. Each of the groups is then divided according to the ratio of albite to orthoclase, and of albite to anorthite. It is not made clear whether all the albite in the rock is to be used in each of these ratios. The ratio of felsic to mafic minerals, shown to be very important in recent differentiation work, is not utilised as a factor in classification until the foregoing are exhausted. The paper is of great value in drawing attention to classification on a quantitative mineralogical basis.

Nothing particularly new has emerged in the petrographical results of the 1907–9 Shackleton Antarctic Expedition. It contains detailed descriptions of the rocks of Mount Erebus and other parts of Ross Island, and from the mainland of South Victoria Land, which are illustrated by numerous new chemical analyses.

Natural History of Sedimentary Rocks

JOHNSTON, J., and WILLIAMSON, E. D., The Role of Inorganic Agencies in the Deposition of Calcium Carbonate, *Journ. Geol.* 1916, **24**, 729–50.

VAN TUYL, F. M., A Contribution to the Oolite Problem, *ibid.* 792–7.

JOHNSON, D. W., Contributions to the Study of Ripple Marks, *ibid* 809–19.

COLE, G. A. J., Rhythmic Deposition of Flint, *Geol Mag.* (6), 1917, **4**, 64–8.

ABBOTT, G., Tubular Structures in Rocks which are probably due to Osmotic Action, *Trans. South-Eastern Union of Scientific Societies*, 1916, 20–3.

ANDRÉE, K., Wesen, Ursachen, und Arten der Schichtung, *Geol. Rundschau*, 1915, **6**, 351–98

HÄBERLE, D., Die Gitter-, Netz-, und Wabenformige Verwitterung der Sandsteine, *ibid.* 264–85.

BARRELL, J., Dominantly Fluviatile Origin under Seasonal Rainfall of the Old Red Sandstone, *Bull. Geol. Soc. America*, 1916, **27**, 345–86.

CLARKE, F. W., and WHEELER, W. C., The Inorganic Constituents of Marine Invertebrates, *United States Geological Survey*, Prof. Paper 102, 1917, pp. 56.

FIELD, R. M., A Preliminary Paper on the Origin and Classification of Intraformational Conglomerates and Breccias, *Ottawa Naturalist*, 1916, **30**, 29–36, 47–52, 58–66.

The paper by Barrell on the Old Red Sandstone is of great importance to British geologists. His hypothesis of fluviatile origin of the formation in general is based mainly on lithological features, such as the great thickness and extent of the conglomerates, and the persistence in depth and lateral extent of appearances, mudcracks, etc., which indicate long exposure to the atmosphere. An analogy is drawn between the Old Red Sandstone and the great Tertiary formations of the intermontane regions of the western United States, which were formerly regarded as the deposits of great freshwater lakes, but are now known to be of fluviatile origin.

The discussion by Clarke and Wheeler of 250 new analyses of the hard parts of marine invertebrates and calcareous algæ throws much light on the origin of marine limestone in general, and dolomitic limestone in particular. While the majority of organisms are highly calcareous, some, such as foraminifera, alcyonarians, echinoderms, crustacea, and coralline algæ are highly magnesian; and phosphorus is also abundant in crusta-

ceans, alcyonarians, and one group of brachiopods. These chemical variations are of great significance in the study of coral reefs. In certain groups of organisms, especially crinoids and alcyonarians, it is shown that specimens from warm waters are richer in magnesia than those from cold waters.

The work of Johnston and Williamson on inorganic agencies in the deposition of calcium carbonate is a severely technical piece of chemical work which has, however, a considerable geological bearing. They call attention to the importance of the factor of relative degree of concentration with respect to calcium carbonate in the ocean, since the chance of a permanent deposit is the greater the more nearly saturated the surrounding water is. The conditions of solubility have now been accurately determined by experiment. The facts thus obtained, together with the known calcium carbonate content of oceanic water, especially the warmer surface layers, make it clear that a greater importance than heretofore must be assigned to inorganic deposition of this material, although, of course, organic agencies must still be given first place.

The important paper by Andrée on stratification treats of no fewer than eleven different kinds of bedding, all included under the two great groups of normal or concordant stratification (Parallelschichtung), and discordant or diagonal stratification (Schragschichtung). In the majority of cases bedding is believed to be caused by variations in lithogenetic conditions, although gaps in sedimentation are believed to be also an important contributory cause. There is a full discussion of the phenomenon of rhythmic sedimentation, and the need for exact experimental investigation (such as that recently carried out by G. K. Gilbert) indicated.

Häberle's paper deals with the peculiar spongy or honeycomb weathering of sandstones, of which, perhaps, the best British examples are found in Arran. Wind may play a dominant part in the production of these forms in arid regions, but they are not confined to or characteristic of these regions. The author believes the general cause of the phenomenon to be the solvent action of water circulating through the rock along well-defined paths, loosening the grains by solution of the cement, and thus exposing them more completely to erosive agencies where these paths reach the surfaces of the rock.

In his short paper on the rhythmic deposition of flint Prof.

Cole expands an illuminating suggestion of Liesegang's, that the layers of flint in chalk are due to rhythmic precipitation of silica from a solution moving uniformly through the rock in a direction perpendicular to the bedding planes.

Abbott describes tubular concretions of ferriferous, calcareous, and siliceous composition which occur in various sedimentary rocks. For their origin he suggests osmotic growth in colloids similar to the growths produced by Leduc in certain solutions. Liesegang, however, has applied his theory of rhythmic precipitation to these structures, and probably only the test of experiment will decide between the rival views.

Economic Geology

REINECKE, L., Road Material Surveys in 1914, *Geol. Survey of Canada, Memoir* 85, 1916, pp 244. This gives an excellent summary of the various kinds of roads, road-making materials, and tests for the road-making qualities of stone.

SPENCER, A. C., The Atlantic Gold District and the North Laramie Mountains, Wyoming, *United States Geological Survey, Bull.* 626, 1916, pp. 85.

GRATON, L. C., and MCLAUGHLIN, D. H., Ore Deposition and Enrichment at Engels, California, *Econ. Geol.* 1917, **12**, 1–38.

UMPLEBY, J. C., Genesis of the Success Zinc-Lead Deposit, Cœur d'Alene District, Idaho, *ibid.* 138–53.

GRAHAM, R. P. D., Origin of Massive Serpentine and Chrysotile-Asbestos, Black Lake-Thetford Area, Quebec, *ibid.* 154–202.

MINERALOGY AND CRYSTALLOGRAPHY. By ALEXANDER SCOTT, M.A., D.Sc., University, Glasgow.

Synthetic Mineralogy.—The workers of the Geophysical Laboratory continue to advance our knowledge of this branch of the subject in several valuable papers. Bowen (*Amer. Jour. Sci.* **43**, 115, 1917) has made a thermal investigation of the sodium-potassium nephelites. The sodium salt—$NaAlSiO_4$—exists in two enantiotropic forms, nephelite and carnegieite, with a transformation point at 1248° C. The form of the potash salt which is stable below 1540° C. is kaliophilite, which is isomorphous with sodium-nephelite, but above that temperature an orthorhombic modification is formed. The latter forms a eutectiferous series with carnegieite, but the low-temperature modifications give a continuous series of solid solutions. The composition of natural nephelite is to be explained by the presence of albite and anorthite molecules in solid solution in the alkaline nephelites.

Johnston, Williamson, and Merwin (*Amer. Jour. Sci.* **41**,

473, 1916) have examined the stability relations and the conditions of formation of the several forms of calcium carbonate. Under ordinary conditions, calcite is the only stable form, but the two unstable modifications, aragonite and μ-$CaCO_3$, may be deposited under particular circumstances. The numerous other forms, vaterite, conchite and so forth, which have been described, are shown to belong to one or other of the above modifications. The two first-named authors (*Jour. Geol.* **24,** 729, 1916) discuss the rôle of inorganic agencies in the deposition of calcium carbonate and conclude that the two chief factors are the variation of temperature and the concentration of free carbon dioxide in the ocean. These affect the relative degree of saturation to a considerable extent, and are obviously of importance in connection with the formation both of chemically precipitated and organic limestones.

A question of some economic importance is considered by Zies, Allen, and Merwin in a paper on the reactions involved in secondary copper sulphide enrichment (*Econ. Geol.* **11,** 407, 1916). On allowing copper sulphate solution to react with natural sulphides, a precipitation of sulphide, accompanied by an oxidation to sulphate, ensues, the reactivity varying with the nature of the sulphide. Starting with pyrites, the first product of the reaction is covellite and the final chalcocite, while pyrrhotite is altered to chalcopyrite. The process is not reversible, except possibly in the case of bornite, where the direct reaction does not occur. The rate of reaction is greater at high temperatures, but the products, except for secondary reactions, are essentially the same. The stability of the various minerals towards copper sulphate is also determined. Allen and Lombard (*Amer. Jour. Sci.* **43,** 175, 1917) have devised a method of determining the dissociation pressure of sulphides, and have applied it to the minerals pyrites and covellite through a pressure range extending from 1 mm. to 500 mm. of mercury. The method is likely to be useful in the synthesis of those sulphides which can be prepared in the dry way.

Boeke (*Zeit. Anorg. Chem.* **98,** 203, 1916) discusses the utility of the tetrahedron in the graphical representation of the equilibrium of four-component systems. In a paper on the composition of tourmaline (*Neues Jahrb. f. Min.* 1916, ii. 109) the principles of *n*-dimensional geometry are applied to

more complicated systems. A consideration of a large number of analyses shows that the departures from Penfield and Foote's formula—$H_{20}Si_4B_2O_{21}$—are comparatively slight. In a further paper (*Cent. f. Min.* 1916, 313) the same author criticises Andersen's work on the system anorthite-forsterite-silica with reference to those natural minerals, in the quaternary system lime-magnesia-alumina-silica, which are not represented on the freezing surface.

Niggli (*Zeit. Anorg. Chem.* **98**, 241, 1916) extends his investigations of the isothermal equilibrium of systems involving carbon dioxide. Systems including the alkali carbonates, lime, titanium dioxide, and silica have been examined and the numerous results obtained are utilised in the construction of pressure temperature diagrams of such complexes as $Na_2O-CaO-CO_2$, $Na_2O-TiO_2-CO_2$ and the analogous potassium systems.

Le Chatelier (*Compt. Rend.* **163**, 948, 1916) discusses the conditions of formation of cristobalite and criticises Fenner's views regarding the stability of this mineral and tridymite. The latter when heated for a considerable time at 1700° C. showed no trace of conversion to the former. The same author (*Bull. Soc. franc. Min.* **39**, 150, 1916) has found thin hexagonal plates of tridymite in the devitrefication products of lead glass, thus showing that this habit of tridymite does not necessarily postulate a pneumatolytic origin (cf. *Lacroix, ibid.* **39**, 154, 1916).

Physical Properties of Minerals.—Joly (*Phil. Trans. Roy. Soc.* A. **217**, 51, 1917) gives the results of a further study of pleochroic haloes. In addition to uranium and thorium haloes, a third type due to radium emanation is described. The structure of the thorium and emanation haloes can be explained by the ionisation effects of the α-rays concerned, but in the uranium haloes, while the outer concentric rings occupy the theoretical positions, the inner ones are slightly displaced. The only feasible explanation of this is that at the period of formation of the haloes (Middle Palæozoic in the specimens examined) the ranges of the α-rays were longer than at present, a hypothesis which might reconcile the radio-active and geological estimates of geological time.

Balzac (*Atti R. Accad. Lincei*, **25**, I. 811, 1916) has determined the variation of the crystal angles of epidote and clino-

zoisite through a temperature range extending from 15° C. to 400° C. The different results obtained in the two cases are considered evidence in favour of the individuality of the two minerals.

Crystal Structure.—L. Vegard (*Phil. Mag.* **32**, 65, 505, 1916, **33**, 395, 1917) has determined the crystal structure of a number of substances. Gold, silver, and lead all have the same lattice as copper, the face-centred cube, and an examination of the spinel group confirms the results obtained, independently, by Bragg and Nishikawa. Interesting results are obtained in the case of the closely-related tetragonal group comprising zircon, rutile, cassiterite, xenotime, and anatase, all of which crystallise in the ditetragonal bipyramidal class. While the lattices, as usual, are atomic, the atoms form groups of the type RO_2 (where R = Si, Ti, Zr or Sn), the three atoms in a group being arranged in a linear fashion, and the distance between the R- and the O-atoms varying with the nature of the former. The structure of zircon is represented by twelve face-centred lattices of which two are composed of zirconium, forming a lattice of the diamond type, and two of silicon similarly associated, the remaining eight being composed of oxygen atoms of which every two are associated with one R-atom. In rutile the titanium is considered to replace both silicon and zirconium so that the metallic atoms form the centred-prism tetragonal lattice. Cassiterite is like rutile, but in anatase the titanium atoms form a lattice of the diamond type with the " molecular " axes parallel to the tetragonal axis and not perpendicular to it as in the other cases. Xenotime was at first thought to have a structure such that the oxygen atoms were all associated with the phosphorus, but a more careful examination showed that it was like zircon, and of the structure YO_2PO_2. In all these cases the departure from the cubic form is due to the arrangement of the oxygen atoms, the ratio $c:a$ being greater or less than unity according as the molecular axis is parallel or perpendicular to the tetragonal axis.

No reflections could be obtained from thorite on account of its metamict nature, the crystals being physically amorphous though the external tetragonal form persisted.

These structures appear to afford interesting evidence on the question of the existence of molecules in the solid state. In the case of such substances as rock-salt, examined by Bragg, it

seems impossible to assign any one chlorine atom to a particular sodium atom to form a molecule in the ordinary chemical sense. The structure ascribed to zircon, however, indicates that in the solid state the formula should be ZrO_2SiO_2 and not $ZrSiO_4$, while rutile and cassiterite should be $(TiO_2)_2$ and $(SnO_2)_2$ respectively. Xenotime, in the solid state, should be represented by YO_2PO_2 and not YPO_4. Vegard explains this departure from the usual formula by assuming that the constitutional of a substance varies with the state, and that the structure of the solid form cannot be expressed by the ordinary constitutional formula. Hence xenotime, despite its structure, may be regarded as an orthophosphate and rutile as an oxide or as a titanate.

Fock (*Cent. f. Min.* 1916, 392), on the other hand, argues that chemical molecules do exist in the solid state, and regards the atomic conception of crystal structure as erroneous. Rinne (*Zeit. Anorg. Chem.* **96**, 317, 1916) also subscribes to this view and holds that Bragg's work shows the existence of groups of atoms linked together to form molecular complexes. The affinities of an atom may be divided so that, for example, in rock-salt the single valency of each sodium atom may be partitioned among six chlorine atoms and in the cubic form of zinc sulphide the two valencies of zinc among the four sulphur atoms which are equidistant from the zinc atom.

In a long theoretical paper on the structure of crystals (*Neues Jahrb. f. Min.* 1916, ii. 47) the same author discusses the structural differences between the various forms of matter and points out the utility of the Laue radiograms in the study of morphotropy and isomorphism.

Pfeiffer (*Zeit. Anorg. Chem.* **97**, 161, 1916), again, argues that the crystal structures so far determined can be explained in terms of Werner's co-ordination theory. Thus the co-ordination number of sodium is six, which is in harmony with the fact that in rock-salt each sodium atom is equidistant from six chlorine atoms, while the numbers "eight" and "four" for calcium and fluorine respectively correspond with the relations in fluorspar. Radicles such as SO_4 and CO_3 are considered to occupy the place of one atom, and hence to have a single co-ordination number.

Smits and Scheffer (*Proc. Akad. Wetensch. Amsterdam*, **19**, 432, 1916) maintain that the distance between adjacent mole-

cules is comparable with the distance between the atoms in a single molecule, and that the difference is too small for detection by X-ray methods. As Bragg's model of rock-salt is not in accord with the symmetry of the crystal, an alternative structure, containing no atom at the centre of the elementary cube, and taking into account the valency of the atoms, is proposed. In this connection Vegard (*loc. cit.*) states that under certain conditions a lattice may explain the symmetry elements of a crystal where the symmetry of the lattice is not the same as that of the crystal.

The last-named author has also determined the lattices of the nitrogen and iodine atoms in ammonium iodide and its tetra-methyl derivative and has discussed the probable arrangement of the hydrogen atoms. The relations between the topic parameters of these substances cannot be explained by simple substitution as the number of elementary lattices is different in the two cases.

An investigation of the structure of the rhombic alkali sulphates by Ogg and Hopwood (*Phil. Mag.* **32**, 518, 1916) indicates that the metal and sulphur atoms are probably arranged in a pseudohexagonal lattice of the type suggested by von Federov. The authors hold that the identity of the absolute volume of the unit cells of the lattices of the rubidium and ammonium salts is conclusive evidence against the valency-volume theory of Pope and Barlow. This is further discussed by Tutton (*Proc. Roy. Soc.* A. **93**, 72, 1917), who reaches the same conclusion.

BOTANY. By E. J. SALISBURY, D.Sc., F.L.S., East London College.

Anatomy.—Whatever views may be held as to the relative primitiveness of Dicotyledons and Monocotyledons there can be no question but that the two had their origin from a common stock. Research undertaken from different standpoints is constantly bringing to light fresh resemblances or widening our concept of those already known to exist. One of the most obvious of the anatomical differences between the two groups is the presence or absence of intrafascicular cambium. But this distinction is by no means universal, and the collection of records of the occurrence of such a cambium amongst Monocotyledons by Mrs. Arber (*Annals of Botany*, Jan. 1917) is a

very useful reminder of the state of our knowledge on this point. Exclusive of a number of instances amongst Grasses and Sedges, the phenomenon has been observed in some twenty-four genera, and in addition to the examples already known the author records an intrafascicular cambium in the axes of *Eremurus himalaicus*, *Nothoscordum fragrans*, and *Asparagus officinalis*. This brings up the number of Liliaceous genera exhibiting the feature to fourteen, and the other families in which it occurs include the Orchidaceæ, Palmæ, Juncaginaceæ, Zingiberaceæ, Typhaceæ, Musaceæ and Commelinaceæ, besides the Gramineæ and Cyperaceæ already referred to.

Evidently then the potentiality for secondary thickening was very widespread and probably future research will reveal its occurrence in the remaining three Cohorts in which so far a cambium has not been observed.

The number of investigated gametophytes of *Lycopodium* is so small that any addition to our knowledge is welcome. Chamberlain has recently described those of *Lycopodium laterale*, *L. scoriosum*, and *L. volubile* (*Bot. Gaz.*, Jan. 1917). In the first of these species the prothallus is green and leafy and possesses a protocorm closely resembling that of *L. cernuum*. In the two latter the prothallus is subterranean, and there is no protocorm present.

The adult sporophyte of *L. volubile* is Selaginella-like in habit and, in common with *L. scoriosum*, possesses a dorsiventral stele with the xylem in horizontal plates. It is therefore of interest that in both these species the stele of the young sporophyte shows a radial construction with apparently centripetal development.

Genetics.—Bateson in the *Journal of Genetics* (Dec. 1916) has described an interesting case of bud variation. He finds that the root shoots of the double white Bouvardia known as " Bridesmaid " produce flowers which are pink and in every respect correspond to the kind known as " Hogarth." Since the root cuttings arise endogenously he suggests that Bridesmaid is a periclinial chimæra in which the epidermis and hypodermis belong to the white form and consequently offspring raised from seed are of the Bridesmaid type. Since the original parent was doubtless raised from seed its peculiar organisation, assuming that Bateson's explanation be correct, can only be

the result of a segregation of characters in the vegetative cells of the plant.

Physiology.—Osterhout (*Bot. Gaz.*, June 1917) has investigated the effect of potassium cyanide on permeability and finds that, in common with ether, chloroform, and alcohol, it produces a temporary decrease in permeability. The author suggests that this may be an outcome of its inhibiting effect upon oxidation.

ZOOLOGY. By CHAS. H. O'DONOGHUE, D.Sc., F.Z.S., University College, London.

Protozoa.—A very great deal of work has been done, particularly in America, on the multiplication and life-history of *Paramœcium*, and the main results of this are generally known. In spite of the great care taken in many of these experiments, the supply of food has not been critically controlled, and this beyond doubt plays a part in the matter of "depression periods." Hargitt and Fray have investigated "The Growth of *Paramœcium* in Pure Cultures of Bacteria" (*Jour. Exper. Zool.* vol. xxii. Feb. 1917), and for the first time investigated by modern bacteriological methods the effect of a strict food control. The experiments had to be terminated, but in spite of that a good deal of useful information has been accumulated and the authors put forward several suggestions regarding the precautions necessary in order to satisfactorily prepare the cultures and compare more exactly than has been done before the results obtained with those of the controls.

Other papers include: "On the Reactions of *Amœba* to Isolated and Compound Proteins," by Schaeffer (*ibid.* Jan. 1917), "The Life-history of *Amœbæ* of the *Limax* Type in the Human Intestine," by Swellengrebel and Winoto (*Parasit.* vol. ix. Feb. 1917).

Invertebrata.—G. H. Parker has conducted a series of experiments on the biology of the anemone *Metridium* (*Actinobola*) *marginata* which form a study of behaviour as tested by operations, etc. During the period under review the results are recorded in a series of papers: "Nervous Transmission in the Actinians," "The Movements of the Tentacles in Actinians," "Pedal Locomotion in Actinians" (all in *Jour. Exper. Zool.* vol. xxii. Jan. 1917), and rounded off by a more or less

general and comparative survey; "Actinian Behaviour" (*ibid.* Feb. 1917). The animals represent a low grade of organisation as might be expected, and a certain amount of independence of the parts render the "organic unity of the animal as a whole" very weak. In the taking in of food, for example, several parts are involved, but it is only the neuro-muscular apparatus of the tentacles whose activity affects the action of the animal as a whole, the other parts being to a considerable degree independent. The entire evidence bears out this, and the author concludes that "Actinians exhibit only a low degree of organic unity which is shown at its highest in their locomotion. They are organically more nearly a sum of parts than a unit.

Other papers include: "On a Larval Actinian Parasitic in a Rhizostome," by Badham (*Quart. Jour. Micro. Sci.* vol. lxii. Feb. 1917); "Observations on the Influence of Salt and other Agents in modifying the Larval Development of the Hookworms, *Ankylostoma duodenale* and *Necator Americanus*," by Nicoll (*Parasitology*, vol. ix. Feb. 1917); "On the Development of *Ascaris lumbricoides* Linn. and *Ascaris suilla* Dug. in the Rat and Mouse," by Stewart (*ibid.*); "Sclerostome Parasites of the Horse in England, II.—New Species of the Genus *Cylichnostomum*," by C. L. Boulenger (*ibid.*); "On the Nervous System and other Points in the Structure of *Owenia* and *Myriochele*," by M'Intosh (*Ann. and Mag. Nat. Hist.* March 1917); and "On the Scolex in the Cestode Genus *Duthiersia* and on the Species of that Genus," by Beddard (*Proc. Zool. Soc.* Feb. 1917).

"A Cytological Study of Artificial Parthenogenesis in *Cumingia*," by Morris (*Jour. Exp. Zool.* vol. xxii. Jan. 1917), throws considerable light on the phenomenon dealt with. The eggs can be made to develop parthenogenetically by exposing to certain temperatures and then to hypertonic sea-water. The nearest approach to normal development occurs in eggs that do not form polar bodies, and it is interesting to find that in these cases the two nuclei formed from the first polar spindle reunite to form a cleavage nucleus. The same may happen, but does not do so frequently, with the second polar spindle. No sign of normal development is exhibited by eggs that have given off two polar bodies. Conklin has investigated the "Effects of Centrifrugal Force on the

Structure and Development of the Eggs of *Crepidula*" (*ibid.* Feb. 1917). By means of a centrifuge it is possible to separate the egg into three zones—yolk, oil, and an intermediate zone containing nucleus, centrosphere, and cytoplasm. He finally concludes that "The differentiation of daughter cells does not depend upon a differentiation of their centrosomes or nuclei, for the spindles may be turned about without changing the differentiations; nor does it depend entirely upon the segregation of the movable parts of the cytoplasm or of the yolk in one cell or the other, for these segregations may be reversed without changing the differentiations; nor does it depend entirely upon the position and direction of the mitotic figure and the cleavage plane with reference to the egg-axes, for these may be forcibly changed as in equatorial first or second cleavages without changing the normal course of differentiation in those cells after the force has ceased to act. These may be contributory factors in the differentiation of cells, but the principal factor is evidently to be found in the spongioplasm which always tends to come back to its normal form if it is stretched or distorted, and which probably differs in structure in different parts of the egg and in different stages of development."

Other papers include: "The Rhythmic Contractions in the mantle of Lamellibranchs," by Redfield (*ibid.*).

A minute species of *Phoronis*, *P. ovalis*, was described in 1856 by Strethill Wright, but since that day has been lost sight of, and it has been suggested that the form was really an immature condition of a larger species. Harmer, however, in "On *Phoronis ovalis* Strethill Wright" (*Quart. Jour. Micro. Sci.* vol. lxii. Feb. 1917), has been able to announce its rediscovery on the coast of Northumberland, and also to show that Wright was justified in considering it as a species. Examples, still very small, with mature gonads are described and a very good account of the anatomy is here provided.

The lice found on man have been dealt with fairly fully in *Parasitology* for Feb. 1917. Nuttall furnishes a very full account of the structure and mechanism of "The Copulatory Apparatus and the Process of Copulation in *Pediculus humanus*." In "A Contribution to the Bionomics of *Pediculus humanus* (*Vestimenti*) and *Pediculus capitis*," by Bacot, we have an account of the laying, hatching of eggs, moulting, and general habits, accompanied by full tables. *P. humanus* is the larger

and more active form with an average length of life of thirty-four days, while *P. capitis* is smaller and averages only twenty-seven days.

Rennie has written "On the Biology and Economic Significance of *Tipula paludosa*" (*Ann. App. Biol.* vol. iii. Jan. 1917). The larva of this fly, better known as "leather jacket," has become of much importance in view of the necessity of preserving as much of the crops as possible. Under certain conditions very serious damage has been caused by this larva, and the present paper is an attempt to investigate not merely the life-history of the fly, but also the conditions unfavourable to the larva and those under which it will attack crops. Although the experiments have been in progress for two years or so, no definite preventive measures are proposed, but a very sound foundation for further work is assured.

Other papers include: "The Homoptera of Indo-China," by Distant (*Ann. and Mag. Nat. Hist.* Jan. 1917); "The Effect of Long-continued Heterozygosis on a Variable Character in Drosophila"[1] (*Jour. Exp. Zool.* Feb. 1917); "Notes on Exotic *Chloropidæ*," by Lamb (*Ann. and Mag. Nat. Hist.* Jan. 1917); "New Species of *Tabanidæ* from Australia and the Fiji Island," by Ricardo (*ibid.* Feb. 1917); "New Species of *Hæmatopoda* from India," by Ricardo (*ibid.*); "Notes on Exotic *Helomyzidæ*, *Sciomyzidæ*, and *Psilidæ*," by Lamb (*ibid.* March); "On some New Mites of the Suborder Prostigmata living on Lizards," by Hirst (*ibid.* Jan.); "*Corylophidæ* [Coleoptera] from the Seychelles and Rangoon," G. Scott (*ibid.*); "Some Systematic Notes on Melolonthine Coleoptera," by Arrow (*ibid.*); "*Cassidinæ* and *Bruchidæ* [Coleoptera] from the Seychelles Islands and Aldabra," by Maulik (*ibid.*); "Coleoptera, Heteromera (excluding *Tenebrionidæ*) from the Seychelles Islands and Aldabra," by Champion (*ibid.* Feb. 1917); "On New Species of Indian *Curculionidæ*, Part III." by Marshall (*ibid.*); "Descriptions of New *Pyralidæ* of the Sub-families *Epipaschianæ*, *Chrysanginæ*, *Endotrichinæ* and *Pyralinæ*," by Hampson (*ibid.* Jan. 1917); "Notes on Fossorial Hymenoptera: xxv. On New *Sphecoidea* in the British Museum, xxvi. On the Genus *Homonotus* Dahlb.," by Turner (*ibid.*); "Descriptions and Records of Bees, lxxiv." by Cockerell (*ibid.* March 1917): "Further Notes on the New Zealand Amphipod *Hyale*

[1] By Marshall and Muller.

grenfelli Chilton," by Chilton (*ibid.*); "Barnacles from the Hull of the *Terra Nova*" (*ibid.* Feb. 1917), "On the Structure and Function of the Mouth-parts of the Palæmonid Prawns" (*Proc. Zool. Soc.* Feb. 1917), both by Borradaile; and "The Embryonic Development of *Trichogramma evanescens* Westw.; Monembryonic Egg Parasite of *Donacia simplex* Fab.," by Gatenby (*Quart. Jour. Micro. Sci.* vol. lxii. Feb. 1917).

Vertebrata.—Very useful additions have been made to our knowledge of the development of the lower Chordata. By the publication of "A Note concerning the Collar Cavities of the Larval *Amphioxus*," Smith and Newth (*Quart. Jour. Micro. Sci.* vol. lxii. Feb. 1917) throw considerable light on this somewhat vexed question and render it possible to compare the collar cavity in this form with the mandibular cavities present in the embryos of *Petromyzon* and the Elasmobranchs, and their results and conclusions have been accepted by MacBride, from whose former conclusions they differ. Certain further details in the early development are dealt with in a paper "On the Development of the Cape *Cephalodiscus* (*C. gilchristi* Ridewood)" by Gilchrist (*ibid.*), who has been able to work on early stages fixed in a variety of ways. After an indeterminate cleavage a blastula stage occurs which later becomes solid. The gastrula-like condition is reached in a somewhat complex way without any typical invagination but by the formation of a lumen in the yolk-mass which then becomes connected to a posterior involution. Further points in the formation and fate of the body-cavities and of the larval nervous system are described, so that several gaps left by previous workers are now filled in.

Other papers include: "A Revision of the Clupeid Fishes of the Genus *Pellonula* and of Related Genera in the Rivers of Africa," and "The Fishes of the Genus *Clupea*," both by Regan (*Ann. and Mag. Nat. Hist.* Feb. 1917); "The Prechordal Portion of the Chondrocranium of *Chimæra collieri*," by Allis (*Proc. Zool. Soc.* March 20, 1917), and "The Early Development of the Spleen of Lepidopterus and Protopterus," by Purser (*Quart. Jour. Micro. Sci.* vol. lxii. Feb. 1917).

Watson has furnished "A Sketch-classification of the Pre-Jurassic Tetrapod Vertebrates" (*ibid.*), which is based on a consideration of all parts of the skeleton of the Amphibia and Reptilia of that period. The characters are divided into two

categories "adaptive" and "non-adaptive," the former restricted to certain lines, and the latter more fundamental and widespread. The "non-adaptive" characters have been largely employed for the separation of the major groups.

Other papers include: "On the Lizards of the Genus *Philochortus* Matschie" (*Proc. Zool. Soc.* Feb. 20, 1917) and "Descriptions of New Lizards of the Family *Lacertidæ*" (*Ann. and Mag. Nat. Hist.* March 1917), both by G. A. Boulenger.

The genitalia of *Tupaia* are described by Wood Jones (*Jour. of Anat.* vol. li. Jan. 1917), who finds that these organs show a mixture of characteristics, some reminiscent of the Metatheria and some suggestive of the Primates. The animal possesses a very long ventral pelvic symphysis, and this affects the length of certain parts of the genital ducts. Little enough is known of these animals, and what there is suggests that they occupy an important position in the mammalian series. Certain muddles in nomenclature have been cleared up by O'Donoghue in "A Note on the Ductus Caroticus and Ductus Arteriosus and their Distribution in the Reptilia" (*ibid.*). A fresh examination of the conditions in *Sphenodon* has shown this animal to be quite primitive in respect of its arterial roots.

Other papers include: "An Easy Way of Demonstrating the Nuclei of Nerve Fibres," by Reburn (*Quart. Jour. Micro. Sci.* vol. lxii. Feb. 1917); "Changes in the Composition of the Entire Body of the Albino Rat during the Life-span," by Hatai (*Amer. Jour. of Anat.* vol. xxi. Jan. 1917); "Notes on the Species of Cavia" (*Ann. and Mag. Nat. Hist.* Jan. 1917), "A New Bat of the Genus *Scotœcus*" (*ibid.* March), "A New Species of Aconœmys from Southern Chili" (*ibid.*), all by Thomas; and "On the External Characters of the *Felidæ*," by Pocock (*ibid.* Jan.).

"The Chromosomes of Human Spermatocytes," now dealt with by Wieman,[1] naturally arouse much interest and have been the subject of many suggestions, including the extraordinary one that in white men the number of chromosomes is double that in black men. The ordinary somatic number is apparently twenty-four in both races of men, and two of these are chromosomes forming an X Y pair. In the primary spermatocyte twelve bivalent chromosomes appear. The material used was obtained fairly fresh and the results of importance.

[1] *Amer. Journ. of Anat.* vol. xxi. 1917.

Reagan has conducted a long series of "Experimental Studies on the Origin of Vascular Endothelium and of Erythrocytes" (*ibid.*). Two distinct rival classes of theories have been put forward on the above question: on the one hand it was maintained that all vascular-forming tissue grows into the embryo from the yolk-sac, and the second is that such tissue can arise independently in almost any place. At first sight it appears quite an easy point to determine, but experience has shown it to be fraught with difficulties. The present author has overcome these to a large extent by careful and critical operations on growing chick embryos, whereby pieces were isolated before the formation of vascular tissue. His evidence is all in favour of the possibility of the local formation of the tissue; indeed, he goes so far as to suggest that prevascular tissue may come from more than one germ-layer.

General.—Pearl has three papers, "The Experimental Modification of Germ-cells," Parts I. and II. (*Jour. Exp. Zool.* Jan. 1917), Part III. (*ibid.* Feb. 1917). and a general discussion of "The Selection Problem" (*Amer. Naturalist*, Jan. 1917). The last points out that although Darwin's theory is logically satisfactory, what we want to know is not what can, but what does take place. He points out that the elimination of individuals is sometimes selective but at other times not, and that the evidence in favour of the transmission of such characters as are selected to the offspring is by no means satisfactory. For these reasons he regards selection as not the primary or even the major factor in Evolution. It is insisted that for a satisfactory explanation we must attack not the animal when adult but the germ-cell, and finishes with some pride and much justice by a reference to "that branch of biological science in which America has taken a leading place."

Other papers include: "Mendelian Factor Differences versus Reaction System Contrasts in Heredity," by Goodspeed and Clausen, Part I. (*Amer. Naturalist*, Jan. 1917), Part II. (*ibid.* Feb.); "The Personality, Heredity, and Work of Charles Otis Whitman, 1843–1910," by Davenport (*ibid.* Jan.); "Note on the Sex of a Tadpole raised by Artificial Parthenogenesis," by Gatenby (*Jour. Micro. Sci.* vol. lxii. Feb. 1917); "A Re-examination of the Applicability of the Bunsen-Roscoe Law to the Phenomena of Animal Heliotropism" (*Jour. Exp. Zool.* Jan. 1917).

ANTHROPOLOGY. By A. G. Thacker, A.R.C.Sc.

The *Journal of the Royal Anthropological Institute* for the second half of 1915 (Vol. xlv), the publication of which was somewhat delayed, contains two interesting and important articles dealing with the Bantu race. One of these is by the Hon. Charles Dundas, and is entitled "The Organisation and Laws of some Bantu Tribes in East Africa," and the other, by Miss A. Werner, is called "The Bantu Coast Tribes of the East Africa Protectorate." C. E. Fox and F. H. Drew contribute to this number of the *Journal* the second section of their article on "Beliefs and Tales of San Cristoval (Solomon Islands)," the first part having appeared in the preceding number of the *Journal*. Dr. W. H. R. Rivers writes a short but ingenious and interesting article on "Descent and Ceremonial in Ambrim." The island of Ambrim is one of the New Hebrides group. This contribution deals with the difficult subject of mother-right and father-right. It is held by some writers that matrilineal descent is always and everywhere an earlier form of society than patrilineal descent, but this idea is condemned by Dr. Rivers. In Ambrim, however, it is the case that mother-right preceded father-right, and the same is probably true of some other parts of Melanesia. Society in Ambrim is now organised on a patrilineal scheme, but from a study of ceremonial in the island Dr. Rivers concludes that traces of an earlier matrilineal condition may still be detected.

Miss Margaret Murray deals with a cognate subject in a paper headed "Royal Marriages and Matrilineal Descent," and in the same number of the *Journal* an article by E. Grant Brown, dealing with "The Taungbyon Festival, Burma," will be found.

In the last two numbers of this Review I have described some of Prof. Elliot Smith's contributions to the study of the origin and spread of early cultures. A criticism of some of Prof. Elliot Smith's views is contained in an article by Harold Peake entitled "The Origin of the Dolmen," which will be found in *Man* for August 1916. The recent numbers of *Man* contain articles which are less brief than is usual in that magazine, this being a change which appears to me to be an improvement. The following articles may be mentioned: "Kava-drinking in New Guinea," by A. C. Haddon, in October; "The Flaking of Sub-Crag Flints," by J. Reid Moir, also in October;

"Some Australian Classes, named and nameless," by Richard C. E. Long, in November; "A Stone-headed Club from Southern Kordofan," by Prof. Seligman, also in November. The club, which is described and figured in this paper, is of special interest, since it resembles the implements of Bushmen rather than those of Negroes. Further articles are "Notes on the Galla," by J. H. Phillipson in December, this author having lived more than four years in East Africa; and "Photographs of Welsh Anthropological Types," by H. J. Fleure, also in December. The photographs here studied, which are fifty-nine in number, are those of Welsh Baptist ministers, and were originally collected by Dr. J. Beddoe. It is interesting to note that the "Bronze-Age type" is proportionately more numerous among these ministers than in the Welsh population generally, which is another indication, added to many others, that this type forces its way to the front in all British communities, for, as the author points out, the Baptist ministers were regarded as leaders of the people.

The Royal Anthropological Institute and the Prehistoric Society of East Anglia have adopted the excellent practice of holding joint meetings from time to time. One such meeting was held in London on March 13, 1917. The President of the East Anglian Society (Mr. A. E. Peake) delivered his Presidential Address, which dealt with "Researches at Grime's Graves in 1916": and other papers were "Plateau Deposits and Implements," by Reginald Smith, and "The Position of Prehistoric Research in England," by J. Reid Moir; and "The Menhirs of Madagascar," by A. L. Lewis.

ARTICLES

THEORIES REGARDING THE ORIGIN OF THE SOLAR SYSTEM

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THE question of the origin of the solar system is one that has been a source of speculation for over a hundred years; but, in spite of the attention that has been devoted to it, no really satisfactory answer has yet been obtained. There are at present three principal hypotheses that appear to contain a large element of truth, as measured by the closeness of the approximation of their consequences to the facts of the present state of the system, but none of them is wholly satisfactory. These are the Nebular Hypothesis of Laplace, the Planetesimal Hypothesis of Chamberlin and Moulton, and the Capture Theory of See. Darwin's theory of Tidal Friction is scarcely a distinct hypothesis, but is mentioned separately on account of its application to all of the others. The main features of these hypotheses will be outlined in the present paper.

The Hypothesis of Laplace.—According to Laplace, the solar system formerly consisted of a very much flattened mass of gas, extending beyond the orbit of Neptune, and rotating like a rigid body. In consequence of radiation of energy this slowly contracted, and in so doing gained so much in angular velocity that the centrifugal force at the equator became greater than gravity, and a ring of matter was left behind along the equator. Further contraction would detach a series of rings. These were then expected to break up in such a way that each produced a gaseous planet. This might later evolve in the same way as the original nebula, thus producing satellites. The criticisms of this hypothesis in its original form are very well known, and will only be summarised here.

1. The angular momentum of the system when the gaseous central body extended to the orbit of any planet can be calculated, and is not nearly sufficient to cause detachment of matter. Poincaré showed that this objection could be met if the nebula

were initially highly heterogeneous, with all but $\frac{1}{700}$ of its mass in the central body.

2. The matter left behind would not form definite rings; for a gas has no cohesion, and consequently the separation of matter along the equator would be continuous and lead to another gaseous nebula, not rotating like a rigid body.

3. A ring could not condense into a planet.

4. According to the latest work of Jeans, viscosity is inadequate to make a mass of gas as large as a Laplacian nebula rotate like a rigid body.

5. No satellite could revolve in a shorter time than it takes its primary to rotate: this condition is violated by Phobos, the inner satellite of Mars, and by the particles constituting the inner edge of Saturn's ring.

6. All satellites should revolve in the same direction as their primaries rotate: this condition is violated by one satellite of Saturn and two of Jupiter.

The second, third, and fourth objections seem quite unanswerable at present. The theory of Gravitational Instability, due to Jeans, is an attempt to pass directly from the symmetrical nebula to an unsymmetrical one with a secondary nucleus, without the ring as an intermediate stage.

It will be noticed that Laplace's hypothesis implies that all the planets were formerly gaseous, and hence must have been liquid before they became solid. The question of the course of evolution of a gaseous mass initially heterogeneous with several strong secondary condensations has not hitherto been considered; such a mass would be free from at least the first four of the objections offered to the standard form of Laplace's hypothesis, and its history would serve as a hypothesis intermediate between this and the Planetesimal Hypothesis.

The Planetesimal Hypothesis.—This hypothesis has been formulated by Chamberlin and Moulton[1] to avoid the serious defects of the Nebular Hypothesis. It really consists of two separate assumptions, either of which could be discarded without necessarily invalidating the other. The first of these involves the close approach of some wandering star to the sun. This would raise two tidal projections at opposite sides of the sun, and if the disturbance was sufficiently violent, streams of matter

[1] T. C. Chamberlin and R. D. Salisbury, *Geology*, vol. ii.; F. R. Moulton, *Introduction to Astronomy*; T. C. Chamberlin, *The Origin of the Earth*.

would be expelled from them. On account of the perturbations of their paths by the second body, these would not fall back into the sun, but would go on revolving round it as a system of secondary nuclei, with a large number of very fine particles also revolving round the sun; each particle, however small, would revolve independently, so that the system would in this respect resemble the heterogeneous nebula mentioned at the close of the last paragraph. The mathematical investigation of this hypothesis would be extremely difficult, but there seems to be no obvious objection to it. It will be seen that the nuclei would be initially liquid or gaseous, having been expelled from the sun. Thus this hypothesis implies a formerly molten earth. The smaller particles would soon become solid, but the gaseous part initially expelled and not under the influence of a secondary nucleus would remain gaseous, although its density would be very small. The orbits would be highly eccentric.

The second part of the hypothesis deals with the later evolution of the secondary nuclei. Its authors believe that these would steadily grow by picking up the smaller particles, which are called planetesimals, and in the process they would have the eccentricities of their orbits reduced. That this is qualitatively correct can easily be proved mathematically. There is, however, a serious objection to its quantitative adequacy. Consider any arbitrary planetesimal. Its chance of colliding with another planetesimal in a definite time is proportional to the sum of the surfaces of the planetesimals, while its chance of colliding with a nucleus is proportional to the sum of the surfaces of the nuclei. Further, if the eccentricities of the planetary orbits are to be considerably affected by accretion, the mass picked up by each planet must be at least as great as the original mass of the planet. Now the more-finely divided the matter is, the more surface it exposes, and hence before accretion the mass picked up must have presented a much larger surface than the planet did.

Hence collisions between planetesimals must have been far commoner than collisions between planets and planetesimals. Further, as the velocity of impact must have been comparable with an orbital velocity on account of the high eccentricity of the orbits, the colliding planetesimals must in nearly all cases have turned to gas; for it is known that meteors entering the earth's atmosphere at such velocities are volatilised. Hence

nearly all of the planetesimals must have turned to gas before the nuclei could be much affected by accretion. We are thus back to the heterogeneous gaseous nebula.

If the planetesimals moved initially in nearly circular orbits this objection does not arise, but it can then be shown that the product of the mass and the orbital eccentricity of each nucleus would diminish with the time. It can thus be seen that Jupiter could never have been smaller than Uranus is now. There is no obvious objection to this form of the hypothesis, but there is no reason to suppose that solid planetesimals did originally move in nearly circular orbits.[1]

A further hypothesis that has come to be associated with the present one, although not an essential part of it, is the belief that the earth has always been solid. There are many serious difficulties in the way of this.

1. The mode of formation of the nuclei described in the first part of the Planetesimal Hypothesis implies that they were initially liquid or gaseous. This is not, however, a direct objection; one part of the hypothesis might be true and the other false, as they are not interdependent.

2. Only one satisfactory explanation of the elevation of mountains by the folding of the earth's crust has been offered; this attributes it to a horizontal compression at the surface. Now, if a solid earth grew by the addition of small particles from outside, these would be deposited in a layer on the surface, in a perfectly unstrained condition. Thus, during the whole process of growth the same surface condition would always hold, namely, that there is no horizontal compression at the surface, however much deformation may take place within. Hence any stresses available for mountain-building must have been accumulated after accretion ceased; if the theory that the earth was formerly molten should be proved to give insufficient surface compression to account for known mountains, then *a fortiori* the theory of a permanently solid earth gives insufficient compression, as the available fall of temperature is less.

3. It is by no means clear that a solid earth growing by accretion would reman solid. A particle falling from an infinite distance to the earth under the earth's attraction alone would develop a velocity almost enough to volatilise it on impact, and the actual velocities must have been considerably greater

[1] *Monthly Notices of R.A.S.* vol. lxxvii. 1916.

than this, as the planetesimals would have a velocity relative to the earth before entering its sphere of influence. If, then, the particles required to form the earth were all brought together at once, the resulting body would be gaseous. On the other hand, if the accretion were spread over a long enough time, heat would be radiated away as fast as it was produced, and the body would remain solid. In the absence of a criterion of the rate of growth it is impossible to state whether an earth growing by accretion could remain solid or not.

Holmes [1] has found that the hypothesis of a cooling earth, initially in a liquid state, leads to temperatures within the crust capable of accounting for igneous activity, whereas the view that the earth is now in a steady state, its temperature gradient being maintained wholly by radio-activity, is by no means certain to lead to adequate internal temperatures. Assuming the former fluidity of the earth, he has developed a wonderfully consistent theory of the earth's thermal state. The present writer, using Holmes's data, finds [2] that the available compression of the crust is of the same order of magnitude as that required to produce the existing mountain-ranges.

It seems, then, that whatever we may assume about the origin of the earth, the hypothesis that it has at some stage of its existence been liquid or gaseous agrees best with its present state. The hypothesis of Laplace, however modified, implies the former fluidity of the earth, and so does the standard form of the Planetesimal Hypothesis.

The Capture Theory of See.[3]—Like the Planetesimal Hypothesis, this has been developed during the present century to avoid the objections that have been offered to that of Laplace. The main features of the two theories are very similar. Both involve the idea of a system of secondary nuclei revolving in independent orbits about the primitive sun, with sparsely distributed small particles between them, and the impacts of the small particles on the nuclei are supposed in course of time to act on the orbits of the latter in the same way as a resisting medium; namely, the eccentricities of the orbits tend to diminish, and satellites tend to approach their primaries. The

[1] "Radio-activity and the Earth's Thermal History," *Geol. Mag* February—March 1915, June 1916.

[2] *Phil. Mag.* vol. xxxii. December 1916.

[3] *The Capture Theory of Cosmical Evolution*, by T. J. J. See.

Capture Theory is not, however, stated in so precise a form as the Planetesimal Theory. It is not definitely stated whether all the small particles would revolve in the same direction or not. If they did, then there would be little or no secular effect on the mean distance of a planet. If, however, they moved indifferently in the direct and retrograde senses, then their collective effect would be the same as that of a medium at rest, and the friction encountered by the planets in their motion would cause them to approach the sun. The fact that such a secular effect is stated by See to occur implies that the particles at any point are not on an average supposed to move with the velocity appropriate to a circular orbit at that point, so that the conditions would be such as to ensure that collisions between them would be violent. The small particles are described by the somewhat vague term of "cosmical dust"; if this means that they were solid, the Capture Theory, like the Planetesimal Theory, fails on the ground that the collisions between the small particles would cause the system to degenerate to a gaseous nebula long before any important effect had been produced on the nuclei. If, on the other hand, they were discrete molecules, then the system would be a heterogeneous gaseous nebula at the commencement, and this objection does not apply. It is clear, however, that the planets cannot have entered the system from outer space, for then their orbital planes would be inclined to one another at large angles, which the subsequent action of the medium could scarcely affect, whereas actually all the major planets keep very close to the ecliptic. All must, then, be regarded as having always been members of the solar system, however much their orbits may have changed. They are supposed to be derived from the secondary nuclei of a spiral nebula.

The most important difference between the Planetesimal and Capture theories lies in the history attributed to the satellites. In the former, each satellite is supposed to have always been associated with its present primary, having been near it when originally expelled from the sun. In the Capture Theory, primaries and satellites are both supposed to have initially moved independently round the sun in highly eccentric orbits. If, in the course of its movement, a small body came sufficiently near a large one, and had a sufficiently small relative velocity, then a permanent change would take place in the character of

its orbit, and it is possible that, under the influence of the resisting medium, this would ultimately lead to its becoming a satellite. The mechanism of the process has not been worked out in detail, and, in view of the extremely complicated nature of the problem, it would be very dangerous to predict whether it is feasible. All the satellites in the system are supposed to have been captured in this way by their primaries. In both hypotheses the satellites are considered to have approached their primaries after becoming associated with them owing to the secular effect of the resisting medium.

The Theory of Tidal Friction.—All the theories so far mentioned agree in the fact that each commences with a particular distribution of matter, and tries to predict the course of the changes that would follow if this were left to itself. The success or failure of such hypotheses to lead to a system resembling the present solar system is the measure of their truth or falsehood. The method is thus essentially one of trial and error, and when a theory is found unsatisfactory, the next step is to modify it in such a way as to avoid the defects that have been detected. In this way a succession of different hypotheses may be obtained, each giving a better representation of the facts than the previous one. Destructive criticism may thus be of positive value. Such a method must necessarily yield the truth very slowly, and must further involve a large number of assumptions concerning the initial conditions; in addition, the set of initial conditions that leads to the correct final state may not be unique.

The Theory of Tidal Friction, due to Sir G. H. Darwin,[1] is of a totally different character. It starts with the present conditions, and by means of a single highly plausible hypothesis obtains relations that the properties of the system must have satisfied at any epoch, provided only that this is not too remote for the calculation to be possible, and that no unknown causes have operated that could invalidate the work. The initial conditions thus obtained are then unique, and the only way of disproving the hypothesis would be to discover some new agency of sufficient magnitude to upset the course of the involution. Whatever hypothesis may ultimately be found to account for the present solar system, the Theory of Tidal Friction must therefore form a part of it.

[1] *Scientific Papers*, vol. ii.

The physical basis of the theory is very simple. The attractive force due to the moon is always greatest on the side of the earth nearest to it, and least on that farthest away, while its value at the centre of the earth is intermediate. The centre of the earth being regarded as fixed, then, the moon tends to cause the parts of the earth nearest to and farthest from it to protrude, thus forming a bodily tide. If the earth were perfectly elastic, the high tide would always occur with the moon in the zenith or nadir; no energy would be dissipated, and there would be no secular effect. If, however, it is viscous the tides would lag somewhat, and their attractions on the moon would, in general, produce a calculable secular effect on the moon's motion and the rotation of the earth. The only case where viscosity would produce no secular effect is when the deformed body rotates in the same time as the deforming one revolves. The tide then does not move round relatively to the body, but becomes a constant fixed deformation, directly under the deforming body, and ceases to produce a secular effect. In the ultimate steady state of a viscous system, then, the viscous body will always keep the same face turned towards the perturbing one. In the solar system there are certainly two examples of this condition, and no other explanation of it has been advanced. Mercury always keeps the same face towards the sun, and the moon towards the earth; with less certainty it is believed that the same is true of Venus and the satellites of Jupiter.

Now if the viscosity of a substance be zero, that substance is a perfect fluid, and there can be no dissipation of energy inside it. If, on the other hand, it be infinite, then we have the case of perfect elasticity, and again there can be no dissipation. If the viscosity steadily increase from 0 to infinity, then the rate of dissipation of energy when the same periodic stress is applied increases to a maximum and then diminishes again to zero. The balance of probability seems to imply that the earth was formerly fluid, and, if this can be granted, the fact that most of it is now almost perfectly elastic at once indicates that dissipation of energy by tidal friction must have been important in the past. On this hypothesis Sir G. H. Darwin traced the system of the earth and moon back to a state where the moon was close to the earth, the two always keeping the same face towards each other, and revolving in some time between three

and five hours. The lunar orbit was practically in the plane of the equator ; the initial eccentricity is uncertain, as it depends altogether on the actual variation of the viscosity with the time.

The question that next arises is, what was the condition just before this ? The natural suggestion is that the two bodies formed one mass. The cause of the separation is, however, open to some doubt. It has been thought that the rapidity of the rotation would be enough to cause instability, in which case the original body might break up into two parts. Moulton, on the other hand, has shown that the actual rotation could not be so rapid as to make the system unstable. It is more likely that Darwin's original suggestion is correct, namely, that at the epoch considered the period of rotation was nearly double the period of one of the free vibrations of the mass ; consequently the amplitude of the semidiurnal tide would be enormous, and might easily lead to fission in a system not possessing much strength.

The Prevalence of Direct Motion in the Solar System.—On all of the theories of the origin of the solar system that have here been described it is necessary that all the planets should revolve in the same direction On the Planetesimal Theory this would be the direction of the motion of the perturbing body relative to the sun at the time of the initial disruption. In addition to this, however, all the planets except probably Uranus and Neptune have a direct rotation, and all the satellites except those of these two planets and the outer ones of Jupiter and Saturn have a direct revolution. The fact that three satellites revolve in the opposite direction to the rotation of their primaries is in flagrant contradiction to the original form of the Nebular Hypothesis. It was, however, suggested by Darwin that all the planets might have originally had a retrograde rotation, and that the friction of the solar tides has since reversed the rotation of all except the two outermost. Jupiter and Saturn would then be supposed to have produced their outer satellites before the reversal took place, and the others afterwards. An objection to this theory has been raised by Moulton, who points out that the secular retardation of the rotation of Saturn due to solar tides is only about $\frac{1}{16000}$ of that of the earth, so that there probably was not time for this to occur. On the other hand, this retardation is proportional to the seventh power of

the diameter of the planets: if we can grant then that these planets were formerly much more distended than at present, the viscosity remaining the same, the available time may be adequate. At the same time, solar tidal friction may be adequate to explain the facts that one of the satellites of Mars and the particles at the inner edge of Saturn's ring revolve more rapidly than their primaries rotate, which would not be the case on the unmodified Nebular Hypothesis.

Direct rotation and revolution of satellites on the Planetesimal Theory are shown by Moulton to be probable as a result of a very ingenious argument involving the mode of accretion. Whether it is quantitatively adequate is not proved, and the present writer would prefer to regard these motions as having been direct since the initial disruption. Let us suppose, for instance, that disruption would occur when the disruptive force had reached a definite fraction of surface gravity. It can easily be seen that both are proportional to the diameter of the disturbed body, and hence their ratio is independent of it. Other things being equal, then, a nucleus of any size would be equally likely to be broken up and give a set of dependent nuclei, which would then revolve round it in the direct sense. Secondary nuclei expelled at the same time and close together would remain together, and their relative motion might be in either sense. Thus we should expect both direct and retrograde revolutions, but the former would predominate. The fact that the retrograde satellites are on the outsides of their systems is to be attributed partly to the greater stability of retrograde orbits of large size and partly to the fact that they would experience less resistance from the medium. Capture may be possible; in the present state of our knowledge we can neither affirm nor deny it. Direct rotation is presumably to be attributed to the attraction of the disturbing body on the tidal protuberance before and during expulsion, and to secondary nuclei with direct motions falling back into the parent body. Subsequent evolution would take place in a similar way to that indicated by Darwin.

The Hypothesis of a Heterogeneous Nebula.—A system of nuclei revolving in a tenuous gaseous nebula would experience a viscous resistance from it, and hence would probably evolve in much the same way as See has indicated in the Capture Theory; accretion must probably be almost negligible, so that

the original nuclei must have had nearly their present masses. The original eccentricities of the orbits of both planets and satellites would be considerably reduced; the inclination to the plane of the ecliptic would be small at the commencement, and would remain so; if the medium revolved the effect on the major axes of the orbit, and hence on the periods would probably be small. Direct satellites would approach their primaries, and retrograde ones would ultimately be left on the outskirts of their sub-systems. Given suitable initial conditions, then, a system might be developed that would bear a strong resemblance to the existing solar system. The resisting medium itself would gradually degenerate and approach the sun on account of its internal friction; the zodiacal light may be the last remnant of it. It may, however, be regarded as certain that there has been no large amount of resisting matter near the earth's orbit for a very long time; there has probably been ample time for the evolution of the earth and moon to take place from the state that Darwin traced them back to. The moon was then probably formed from the earth by the disruptive action of the solar tides; but, as this would be a resonance effect, increasing in amplitude over thousands of vibrations, whereas the formation of a system of nuclei in the way suggested by Moulton would take place at once, there need be no surprise that the former event led to a single satellite of $\frac{1}{80}$ of the mass of the primary, while the latter formed several, the largest having a mass of $\frac{1}{1000}$ of its primary.

The unsymmetrical nebula here considered might have been produced in the manner described in the last section. A symmetrical nebula becoming gravitationally unstable would lead to an unsymmetrical one, as was proved by Jeans, but it is difficult to see how the phenomenon of retrograde and direct motions occurring to the same subsystem could occur on this hypothesis. On the whole, then, the most plausible hypothesis seems to be that a gaseous nebula with a system of secondary and tertiary nuclei was formed round the sun by tidal disruption owing to the close passage of another star, and that this has been subsequently modified by gaseous viscosity, and at a later stage by tidal friction. The moon was probably formed from the earth by solar tidal disruption, this method being abnormal in the system, and the later evolution of the earth and moon has been dominated by bodily tidal friction.

ON THE GELATION OF THE NATURAL EMULSOIDS[1]

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THE question of the structure of gels has long been a matter of controversy. Perhaps the theory which has found the most favour is that which considers them as systems of two liquid phases persisting from the sol stage. However, Hatschek [1916] has shown, from the stress-elongation curve, that this theory is untenable. Since it is now recognised that the distinction between the suspensoids and emulsoids is rather a question of the affinity between the two phases than of the solid or liquid state of the disperse phase, there seems to be the less reason for refusing, as he indeed predicted, to extend v. Weimarn's theory to the natural emulsoids and regard their gelation as a crystallisation process.

As early as 1835 M. L. Frankenheim suggested that these bodies were aggregates of small crystals, and attributed their density and easy solubility to the existence of pores. In 1879 it was proposed by K. v. Nágeli that gels consisted of molecular complexes, with crystalline properties, separated by skins of water and forming meshes in which the water was contained by molecular attraction. Since that time a number of unsuccessful attempts have been made to devise a geometrical structure which would account for the properties of gels. Any mathematical theory must, however, be consistent with the considerations that (1) the elasticity of gels varies considerably, some being practically inelastic, and (2) that, if the structure is due to the action of the directive forces of crystallisation, it would be likely to vary according to the nature of the gel. The latter principle finds confirmation from the work of Zsigmondy and Bachmann [1912] on the crystallisation (gelation) of sodium and potassium oleate, palmetale and stearate solutions. These considerations lead to

[1] Received March 12, 1917.

the view that a single network may not account for the elastic properties of different gels.

As was pointed out by Hatschek [1914] the lens-shaped form of gas-bubbles occurring in gels must have some relation to their structure. He found that the bubbles place themselves at right angles to pressure applied to the gel and parallel to tension. By measuring the angles between pairs of bubbles in 10 per cent. gelatin Hatschek found certain values approximately repeated a number of times, although other measurements varied enormously, so that whether the angles have much to do with the crystalline structure is doubtful. However, planes of cleavage are suggested and the simplest structure that would conform approximately to the conditions is that of piled shot or a brick-stack. In this relation it is remarkable that ultra-microscopic examination of gelatin gels of from 1 to 6 per cent., from which the liquid has been expressed, shows flocks of separate grains of irregular shape with clear spaces separating the flocks. Increasing concentration reduces the size of the grains, as would be required by v. Weimarn's theory, and, at the same time, the empty spaces gradually disappear, until, at about 6 per cent. the solid phase fills the whole field and the single grains can no longer be differentiated. Agar and silicic acid show a similar globulitic structure. [Bachmann, 1911; Zsigmondy and Bachmann, 1912.]

During experiments on banded precipitates in gels the slow motion of solid particles through gels was frequently observed. Moreover, many precipitates appeared gradually to settle down, leaving the gel apparently unchanged above. This suggests rather that the individual gel particles are easily separated than that they are joined fast in a network.

It may be noted in this connection that the solid particles deposited by the chemical reaction of solutions diffusing into gels are frequently spherical in shape, resembling natural spherulites. Their form may be attributed to the adsorption, by the granules, of the reaction components from the diffusing solutions so that the value of v. Weimarn's $\frac{P}{L}$ at the surface of the granules is greater than in the surrounding gel, and, owing to the diffusion of further supplies of nutrient matter, becomes larger than the original number of condensation centres. The grains are therefore unable to grow as regular crystals and

develop as starlike growth figures, globular aggregates, or as spherical crystals [Bradford, 1917]. It is evident that the slower the velocity of crystallisation of a particle the more easily will the value of $\frac{P}{L}$ increase in this way. Now in the case of large unwieldly molecules, like those of the natural emulsoids, the directive force of crystallisation may very well be small, and for this reason, coupled with the inertia of the molecules and the viscosity of the medium, fresh molecules might take a considerable time to fit themselves into the crystal structure. Consequently molecules or aggregates might arrive faster than needed for the regular growth of crystal faces and result in the production of globular crystals, or even in extreme cases of aggregates piled up without regard to orientation. Perrin's large-grained emulsions of mastic and gamboge exhibit this spherical shape clearly, and Moore observed chains of globules in dilute silicic acid [1915].

On the basis of such a formation it is perhaps a little difficult to understand the coherence of gels and to explain exactly their stress-elongation curves. The coherence is to some extent affected by the films which form on the surface during setting. And there may be amicroscopic filaments which come into play. The existence of an elastic surface layer around the disperse particles might also contribute to the effect. It will also be remembered that the liquid expressed from a 5 per cent. gelatin gel contains from 0·16 to 0·46 per cent. gelatin [O. Bütschli, 1896], while that from a 2·23 per cent. agar gel varies from 0·09 per cent. at 5° to 0·47 per cent. at 36° [Hardy, 1900]. The viscosity of this liquid would be appreciably greater than that of water.

The above figures show that the concentration of the liquid in equilibrium with the solid phase varies with the temperature. And, since at a few degrees higher temperature, the whole mass was fluid it is difficult to resist the conclusion that the excess has a crystallised cut on cooling. This is in accordance with the influence of concentration on the temperature of gelation [Rona, 1902; Rohloff, 1907; Levites, 1908].

A similar inference may be made from another point of view. P. P. v. Weimarn's researches show how to prepare a given substance in any desired degree of dispersity, and indicate that the disperse particles are crystalline however great the specific

surface. From his formula, $N = K\frac{P}{L}$, a sol or a gel results according as a large number, N, of crystallisation centres are produced from a large excess concentration, $P(= C - L)$, or a small solubility L. Intermediate values of P and L give rise to micro or macro crystals. If his formula applies to substances for which this intermediate stage vanishes, by reason of their slow velocity of crystallisation, the transition point between the sol and gel stages should correspond to the act of crystallisation. From the general behaviour of sols, increase of concentration beyond a limiting value does lead to precipitation. Since, in the case of the lyophile sols, there is considerable affinity between the two phases, a temperature variation is indicated, as is confirmed by Hardy's figures for the liquid expressed from agar. And the transition or gelation point should in general be a function of the concentration and temperature of the sol. The influence of concentration is well exemplified by mastic. Dilute alcoholic solutions of mastic poured into water produce sols, while a greater concentration of mastic gives fairly permanent gels. Whether or not the precipitate forms a coherent gel or merely sinks to the base of the vessel would depend on its mass and degree of dispersity. If the precipitate is sufficiently voluminous it would drink up the liquid and form a coherent gel.

The gels of camphorylphenylthiosemicarbazide investigated by Hatschek [1912] led him to suspect a crystalline structure formed by growth of amicrons into a network: a conclusion which he hesitated to accept on account of the mathematical difficulties. These gels appear to be typically elastic, and are produced (1) when an up to about 5 per cent. hot alcoholic solution of the substance is suddenly cooled, (2) from the gradual or rapid cooling of toluene solutions of similar concentration, or (3) by pouring a concentrated alcoholic solution into petroleum. The latter method gives gels with a concentration as small as 1 : 400. The solubilities in alcohol, toluene and petroleum are respectively 0·5 per cent., 1 per cent. and very small. P. P. v. Weimarn's theory easily supplies an explanation of the first and third cases, though the second is more difficult to understand, for the slow cooling of an alcoholic solution leads to the production of macrocrystals, though the substance is less soluble in alcohol than in toluene. The explanation may lie

in a larger value of the factor K in v. Weimarn's formula, or a slower velocity of crystallisation from toluene solution.

Considering the general case of a substance of very high molecular weight and low diffusivity. Let this be treated with a liquid such that the solubility, L, shall neither be too great in the cold, nor increase too rapidly on heating. Then, on applying heat, the process will be reversible and the Noyes-Nernst formula for the velocity of solution, $V = \frac{D}{\delta} S(L - C)$ will hold. Since L is not very large and D is very small, the velocity of solution will also be very small and the sol stage will persist. Further heating will lead in time to the breaking up of the aggregated particles of the sol and an approach to true solution. If the stage of single molecules is reached this may be regarded as a true solution, though the diffusion constant will be that of a sol. Notwithstanding the considerable size of the molecules the system may be considered homogeneous and v. Weimarn's formula applied. On cooling, with reduction of L, aggregation will commence, but the velocity of crystallisation, $V = \frac{D}{\delta} S(C - L)$, will be a minimum so that the diminution in L will gradually give a larger and larger P and a still greater value of $\frac{P}{L}$. Actually $\frac{dN}{dL} = -K\frac{C}{L_2}$. The velocity of crystallisation will also increase. And if, when spontaneous crystallisation takes place, N has become sufficiently large, a gel will be formed. Gelation will therefore be dependent on the concentration, solubility, and the factor K, which represents the degree of association and viscosity of the sol. Consequently such a sol, of which the association had been diminished by long heating, would be less likely to gelate on cooling than one which had been freshly solated.

It would be somewhat remarkable if such substances should not occur, and the reversible emulsoids appear to fulfil these conditions completely. They form sols of considerable concentration, viscosity and aggregation, and have very low diffusion constants, although little is known about their true solubilities. Heating at constant temperature decreases their viscosity while cooling increases it—changes which are now seen to be in complete accordance with Einstein's formula.

Osmotic pressure varies in the opposite sense. They are generally not very stable bodies, but the behaviour of gelatin is particularly remarkable. By continued heating at 100° the submicrons grow smaller and fewer, until, after thirty-six hours [Levites, 1908] only amicrons are present and the viscosity has changed from 2·29 to 1·39 and become constant. The sol now refuses to gelate and is known as β-gelatin. This is very soluble but, on evaporating nearly to dryness and cooling, it passes through a gel stage which by drying forms a mass resembling sheet gelatin.

The heat coagulation of albumin must be supposed to be due to the chemical production of an insoluble compound. In this case gelation is irreversible.

If it be conceded that the solubility of silicic acid depends on the hydration and aggregation of the molecule its gelation follows at once from v. Weimarn's theory. The gel is formed immediately (in the same way as that of aluminium hydroxide, for example), when a moderately concentrated solution of sodium silicate is neutralised with mineral acid. This is due to the sudden production of a highly aggregated insoluble form of silicic acid, owing to the concentration, viscosity and aggregation of the reaction medium. On the contrary, when the silicate is slowly stirred into the acid medium, which has the ordinary properties of a liquid, probably a true solution of silicic acid is formed at first. This is indicated by its high diffusivity. Changes then take place, possibly in the hydration of the molecule, which reduce the solubility and cause gradual aggregation with increase of viscosity [Garrett, 1903] and sol formation.

If it is preferred to regard the immediate precipitation of silicic acid as due to electrolytes, its gelation is no less in conformity with v. Weimarn's theory.

Returning to the reversible sol-gel transformation, it has been seen that the low diffusion constant of the natural emulsoids, by retarding crystallisation, allows the accumulation of a sufficiently large excess concentration which, together with their large value of K, will cause gelation on cooling. This low diffusion is also responsible for the permanency of the gels and hysteris of sols, since it prevents the subsequent development of larger crystals in gels and retards changes in the aggregation of sol particles.

If one regards the precipitated particles as consisting of aggregates of amicroscopic crystals or filaments, the specific surface would be so great that the volume of the compressed surface layer of adsorbed liquid would be considerable and would explain the contraction of the total volume of liquid and gel and the evolution of heat during imbibition. The amicrons may also be regarded as surrounded by a liquid envelope due to the molecular cohesion between the liquid and solid phases. From this point of view the phenomena exhibited by gels should be influenced by the lyotrope series of dissolved salts which affect the surface tension and the affinity of the liquid and solid phases. The connection between adsorption of solvent and imbibition, and the similar effect of the Hofmann series on both, was pointed out by Lillie [1907].

If osmotic pressure is due, as Lillie suggests, to adsorption of solvent by the dissolved particles, the osmotic pressure of emulsoid sols should be decreased by salts in the order CNS<I . . . <SO_4 as was found to be the case. The diminished pressure should hardly be caused by aggregation of the sol particles, since the precipitating effect of the cations has not commenced at the concentrations employed, although anions hinder the precipitation in the opposite order [Pauli, 1913]. Hatschek found the same series for the clearing of oil emulsions. The temperature of gelation should, however, be diminished in the order CNS>I . . . >SO_4 as was found by Levites [1908]. The change in viscosity with time [Schröder, 1903] follows in the same way as due to a diminution in the precipitating forces. If the velocity of crystallisation is reduced, aggregation will be hindered and precipitation will not take place until a lower temperature. Dialysis will leave a sol containing smaller particles, the effect being similar to that of prolonged heating. Similarly the elasticity of gels is increased by salts which favour imbibition and *vice versa*. And the double refraction of deformed sols shows the same lyotrope effect [Lieck, 1904].

From these considerations the application of v. Weimarn's theory to the gelation of the natural emulsoids appears to account for most of the properties of gels, including their remarkable hysteresis so suggestive of their connection with vital phenomena, nor is the theory necessarily inconsistent with their elasticity and the thermal anomaly of stretched gels, though,

from analogy with rubber, the latter property at least would seem to be connected with the solid phase, since this hydrocarbon exhibits these phenomena in the solid state, which may correspond to that of a dried gel.

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HISTORY IN TOOLS

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In modern teaching, political history has overshadowed all other aspects of man, and the general history of civilisation has not yet received recognition. It matters nothing whether Aristotle, Euclid, Newton or Pasteur, lived under a republic or a despotism ; but it is of the first importance in history to know the influence of such thinkers and discoverers. The movement of man's mind in ideas, knowledge, and abilities should be one of the principal and most stimulating subjects in education. This would not be a materialistic limitation, and one side of it has been admirably written already in Lecky's *History of Morals*.

Among the activities of man, the development of his means of work must certainly be considered. But while there are many books on offence and defence, arms and armour, there is none that traces the history of the mechanical aids. Thousands of writers have described the sculptures of the Parthenon, not one has described the means used in performing that work. It is a mystery to us how fluted columns with an entasis could be produced, true to a hundredth of an inch, in the diameters between the deep groovings.

In taking up the neglected history of tools,[1] the nature of the materials used is the first view to consider. After the stone ages, the order of metals,—bronze and then iron,—is tolerably well known. Of late years an earlier age of copper has been noticed in several countries ; and this again may be divided into an age of native copper and an age of smelted copper. The use of copper in the American hemisphere was entirely limited to native copper, never smelted ; in fact it was the stone age, including a malleable stone. Native copper is also found in various places in Europe and Asia, and it seems only

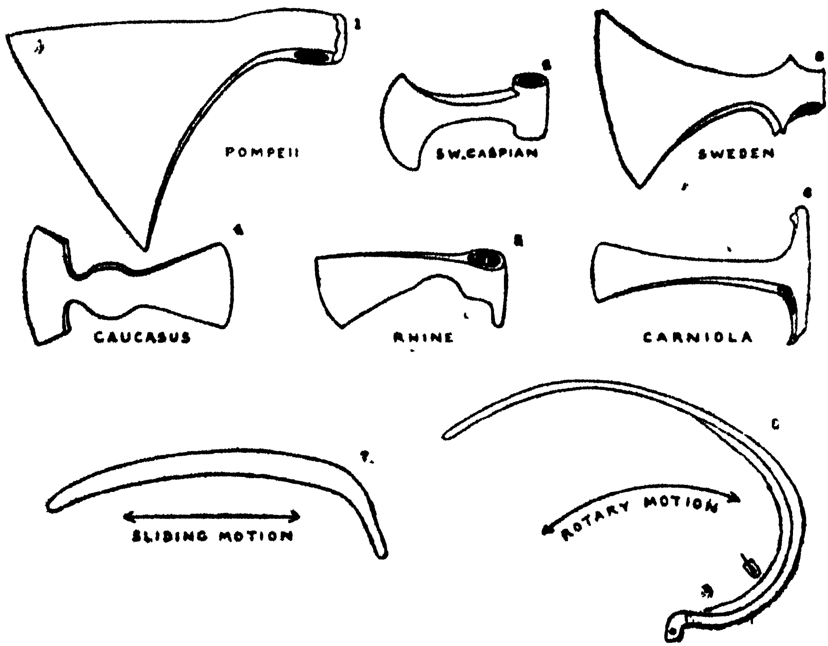
[1] A first step in historical treatment I have attempted, in a catalogue comparing the tools of Egypt with those of other lands,' *Tools and Weapons*, with 3,000 figures.

reasonable to suppose that it would be worked before smelting was discovered. What points to this is the pillowy form of tools in the earliest metal age of most countries. This form could not be cast except in closed moulds, but it would be the most natural for hammered native metal. The earliest stage of casting was the mere limiting of out-poured metal in an open mould, and hence flat castings, such as are found in Egypt, and such as appear in other countries after the hammered forms. The order of use of metallic materials, then, seems to be native copper, smelted copper, bronze, iron, steel, and brass. Copper may be hardened by small impurities and much hammering, until it is equal to any bronze; the main purpose in using bronze was probably to facilitate casting, especially for closed moulds. The *cire perdue* process also needed bronze, and that was a favourite mode of work, from early Egypt to early Britain. In both those lands the metal was run to an astonishing thinness, often only a fiftieth of an inch, a mere film over the sand-core.

When the variations of the forms of tools in different countries are compared, much is seen to depend upon climate. In the north (figs. 3, 4) sockets are much larger and deeper than in the south (figs. 1, 2); this is due to the softer and more stringy nature of northern woods, which would be bruised and crushed in the leverage of a small socket. Neither oak nor ash nor beech could compare with the Syrian *skum* for resisting a wrench. The varying purposes also led to very different forms; the slight socket and large blade for a fighting axe, when the blade was not gripped in the cleavage; the splitting axe with a long socket to enable a side-wrench to be given; the cleaving axe with a long back to the socket (figs. 5, 6) to aid in a lifting pull to get it out of the wood. In the agricultural tools there are clear distinctions between the scythe or sickle worked with a sawing motion from the hand at the end of the blade (fig. 7), or the reaping sickle with a circular arc around the wrist which rotates it (fig. 8), or the pruning-hook to top off high vine-sprays in the south (fig. 46), or the bill-hook to cut copse-wood in the north. The different kind of motion must be considered before we can understand the varying use of each tool. In weapons, similarly, the width of spear or arrow-head is conditioned by the defence. On bare bodies wide cutting blades are the most effective, to attack clothed bodies a narrower

blade is needed, and for piercing armour of leather or metal a mere spike is required.

These forms which result from the necessities of use, and the guidance of utility, may very probably be evolved in many different centres quite independently. We know, in modern times, the Patent Office shows how often a simple thing may be reinvented. The case is different, however, when we look at artistic style ; in that, each race or country has its own characteristics which cling to it for ages, and are seldom adopted by



FORMS OF SOCKET: 1, 2, small for hard wood ; 3, 4, lengthened for softer wood ; 5, 6, for lifting.

FORMS OF REAPER : 7, sliding cut, Swiss ; 8, rotated round wrist, Egypt.

others. When a design recurs we can generally trace its descent, sometimes through thousands of years. Sometimes principles of form also have an astonishing persistence. The northern and Syrian peoples used flanged edges to stiffen tools, the Egyptian and most Mediterranean peoples would have none of them. The European and Asiatic used socket-holes, the Egyptian always rejected them. The European cast in flat moulds, and used punched ornament ; the Asiatic cast in closed moulds, and used cast relief ornament. The Asiatic and

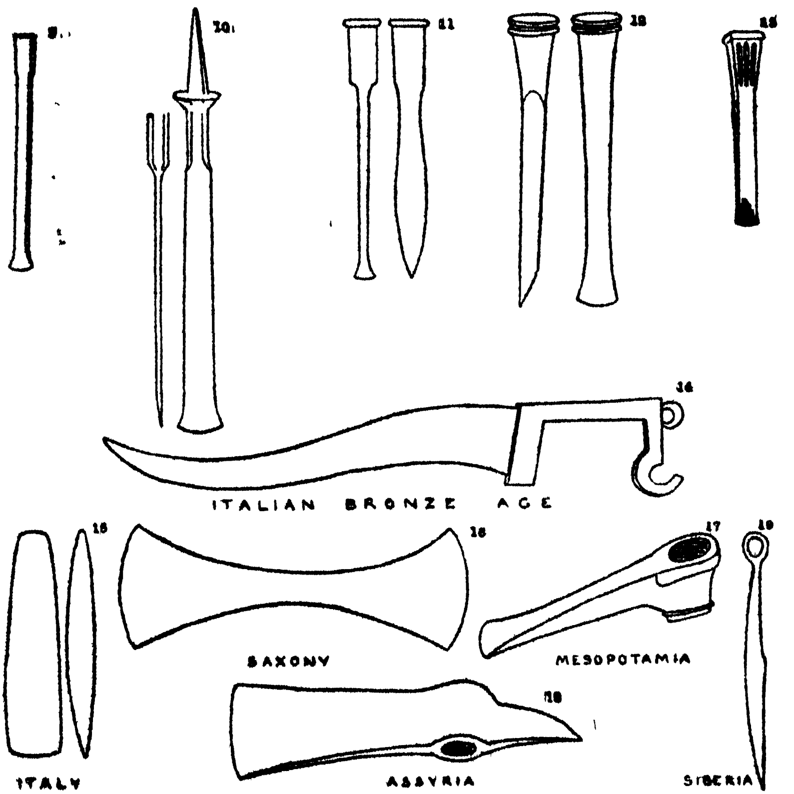
East European used re-curved outlines, the European and Egyptian used straight or simply curved outlines. In all these respects we see a fundamental artistic difference between races.

Another curious aspect of the subject is the worship or reverence given to weapons. Spears were kept in the temples of Italy as means of divination, and immense ceremonial spear-heads are known from early Mesopotamia, Italy, Sweden, Britain and China. The scimetar was adored in Scythia, and the Quadi adored their swords as deities. The driving of a nail into the temple of Jupiter in Rome was the means of averting pestilence. The double axe was a usual tool, and also a sacred form; ceremonial copies, which could not be hafted (fig. 16) were made in various northern centres, apparently as standard weights.

Several stages of inventive activity may be discovered, when a great outburst of new types appears. The most prolific period seems to have been in the later bronze ages, about 900 B.C. The most perfect forms of bronze chisels were then devised (figs. 9 to 13), both tang and socket chisels, wide chisels, deep mortise chisels, saws with a uniform rake to the teeth to cut in one direction, great knives of a flamboyant form (fig. 14) with double curves,—all due to north Italian genius. About the same time, or a little later, the Chalybes on the Assyrian side were developing iron and steel tools on modern lines, socket and tang chisels, saws, rasps, and the early stages of files and centre-bits. These were in use about 700 B.C. It is also noticeable how a great wave of ethical ideas appears in that age in Judæa, Greece and Egypt; it seems to have been a potent stage of thought in many branches.

Some tools which have been, and still are, very usual in other lands, are little known in the West. The adze had a very long career, from the early prehistoric age of Egypt, and is still the common tool of the East. It is often now confused with the axe, under the general name of celt; but it is essentially different, being unsymmetrical in side view, and used across the plane of motion. One common form of it, from about 1500 to 400 B.C., has scarcely been noticed hitherto; it has two projections on the side-edge to hold up the lashing which attached it to the handle. It is strange to see how a tool which was commonly used in many countries for a thousand years, has now disappeared from life as totally as the mammoth.

It is too often supposed that because some thousands of years have passed in the history of a tool, therefore we must now be in possession of far better forms than those of past ages. This is true in many cases, but by no means always. The forms of the chisel were perfected 2,500 years ago; and the beauty of work in the bronze age chisels (fig. 10) with perfectly even



9 to 14.—Bronze Age inventions of Italy; not used by Egyptians.
15 to 19.—Forms not used by Egyptians.

blades, dished octagonal flanges to the tang, or square sockets ribbed on the outside for strength (fig. 13), has never been exceeded. In other tools there has been an actual loss of good design. The Egyptian form of the Roman shears has one leg detachable for sharpening (fig. 36); it was held in place by two slots engaging T-shaped pins, it could be detached in a second, and yet was quite firm. Such a facility for sharpening

is a great advantage, but the form has entirely disappeared. Another Egyptian form was the iron sickle (fig. 8), with a trough groove to hold a strip of steel teeth; this was adapted from the old Egyptian wooden sickle with flint saws inserted, and when steel was valuable it was a great advantage, yet it entirely died out from use. The use of saws and crown drills with fixed teeth of corundum or gem stones, for cutting quartz rocks, was the regular system of work in Egypt 6,000 years ago, and in Greece 4,000 years ago. The cores produced were so perfect and clean-cut that, as Sir Benjamin Baker said, any engineer would be proud to turn out such good work with the best diamond drills. The saws were over eight feet long, sawing blocks of granite 7½ feet long.

This splendid work was quite forgotten, the Roman had no such grand tools, and some thousands of years passed before such means were reinvented fifty years ago.

In other cases we can trace the gradual evolution of a tool down to the present day. The carpenter's saw was at first merely a blade roughly hacked on the edge; by 4,500 B.C. it had regular teeth, sloping equally both ways; by 900 B.C. the Italian gave a rake to the teeth to make them really cut in one direction, instead of merely scraping as before. No ancient saw, however, had a kerf, cutting a wider slit than the thickness of the blade; we do not know when that was invented in the Middle Ages. The Egyptian used a push-saw as the earliest form; the pull-saw was the only one in the West and the Roman world; the push-saw came back into use in the last few centuries, though the pull-saw in a frame is still universal in the East. The world did without shears for many ages, cloth being cut with a rounded-blade knife (fig. 34). About 400 B.C. the mechanical genius of Italy invented the shears, which in two or three centuries more were fitted to the fingers, and thus started the scissors. The snuffers in Exodus is a mistranslation; the early tools for trimming a lamp were a small knife and pair of tweezers to trim the wick, and a point to part the strands.

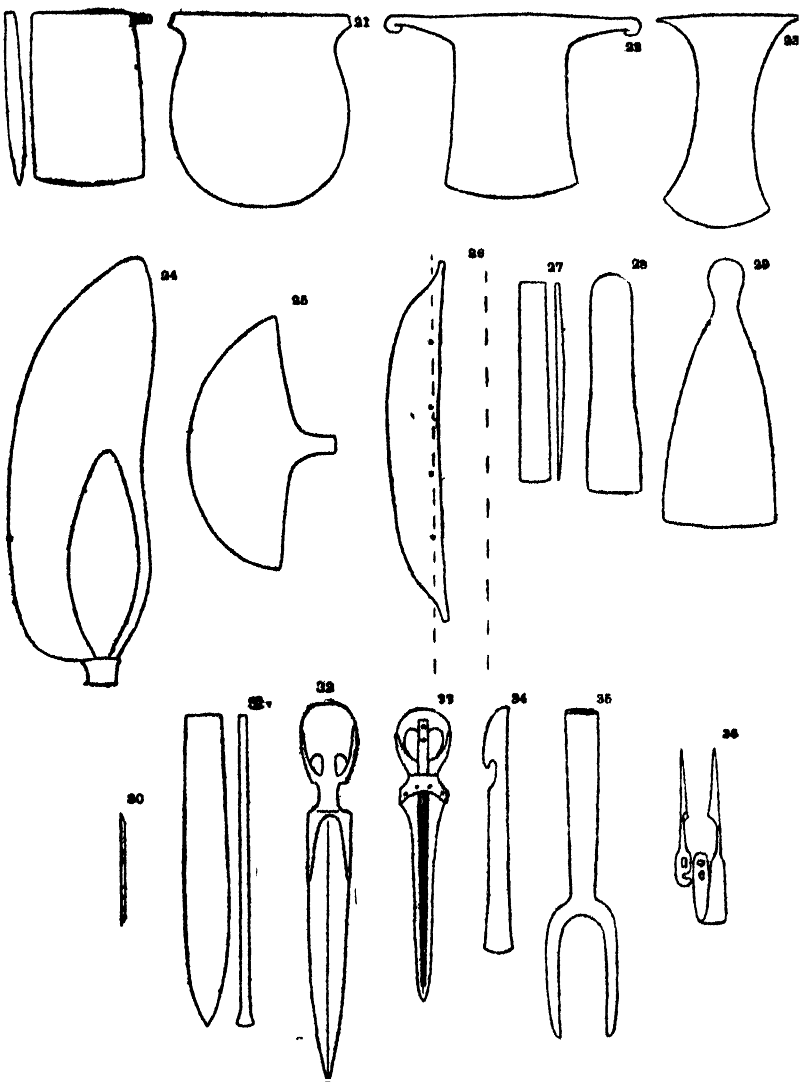
In some cases it is curious to see how long men remained on the brink of an invention. Copper wire was made by cutting and hammering, from 5,500 B.C., yet the drawing of wire remained unknown for 6,000 years or more. When the first drawn wire was made is not yet fixed, but it seems to have been unknown to the Romans. Thick beaten wire was made into chain with

round links as far back as the second dynasty, 5,200 B.C.; and links doubled up, and looped through each other, appear in the sixth dynasty, 4,200 B.C. Yet chains were not commonly used till much later. The Gauls excelled in such work, as they used chain cables and rigging in place of rope, to resist the Atlantic gales. The screw was a Greek invention, and greatly used by the Romans as a means of motion. Then centuries passed before the nut and screw, for fastening, was invented; and again centuries before the screw used to fasten wood, which first appears less than two hundred years ago.

The light that the distribution of tools throws on the status of ancient civilisation is most valuable historically. Not only does the using of certain tools show a level of work and ability, but the resistance to the adoption of forms known elsewhere shows that there was a sufficient ability already in a country. In the present day the forms of common tools differ in various parts of Europe, because each country has a civilisation strong enough to carry on without copying another country. A large improvement in one country is the only condition on which other countries will borrow from it, and only then if the changes will suit other conditions. When we find that countries, known to have been anciently in connection, each steadily resisted various forms of tools used by the other, we have good evidence that each civilisation was on such a level that it could supply all its wants without great benefit by imitating another. This form of evidence gives some insight into dark ages, of which but little detailed knowledge is preserved; it suffices to show whether countries were far below one another, or on such an equality of work that each was independent.

In Egypt there were many forms of tools and weapons, which were then the standard types, and yet these are never found in other lands. The earliest axe (fig. 20) is a plain square form, from about 6,000–5,000 B.C. Then a round axe (fig. 21) was adopted till nearly 3,000 B.C. After that wider lugs were developed to enable it to be firmly bound on to a handle (fig. 22); and this was made in a lighter and longer form as a battle-axe (fig. 23) used mainly about 1,500 B.C. None of these forms are found in other countries, yet had the lands around Egypt been much behind in their axe forms, they would naturally have been influenced by Egyptian types, as there was trade intercourse during all these periods. The only adoption

of such forms was due to entirely independent reinvention of the axe with lugs in South America, without any intermediate



20 to 36.—Forms of tools peculiar to Egypt.

example. The form is a natural one to adopt in hammered copper, for getting a firm attachment to the handle.

Other adaptations of the axe were the large blade of curved outline on the end of a pole (fig. 24), the half-round halberd

(fig. 25), and the long edge set in a stout baton (fig. 26) for a cutting blow. All of these were common in Egypt, but never spread elsewhere.

The adze in Egypt was at first a straight long blade of copper with parallel sides (fig. 27). Later it developed a rounded head-end (fig. 28), with contracted neck (fig. 29), to aid in binding it on a handle. Neither of these was copied in any other country.

The chisel was at first sharp at both ends, and held by the middle (fig. 30). Later there is a deep mortising chisel with an equal curve of each face (fig. 31). Neither of these Egyptian forms appears anywhere else.

The dagger, from prehistoric times onward in Egypt, had a crescent handle held in the palm of the hand (fig. 32), so as to use the weight of the arm end-on for a thrust; whereas the European dagger was always held as a knife, across the hand. The Egyptian ornament was by parallel ribs along the axis (fig. 33); in all other countries the ornament is by lines parallel to sloping edges. Some forms are entirely restricted to Egypt, as the cutting-out knife (fig. 34) with a curved blade for cutting linen, the forked spear-butt (fig. 35), and, in Roman times, the shears with detachable leg (fig. 36), and the sickle with replaceable teeth (fig. 8).

Here, then, are seventeen tools and weapons, mostly of general importance and use in Egypt, which were none of them required by the neighbouring lands, where there must have been some useful equivalents.

The converse is equally true; many forms were used around Egypt which never were adopted there. In Cyprus and other lands the earliest axes are of a pillowy form (fig. 15), with bulging faces. In Europe the double axe (fig. 16) was not only a tool and a weapon, but also a sacred symbol and a standard weight. In Mesopotamia the sloping socketed axe was usual (fig. 17), in Assyria the pickaxe (fig. 18). Not one of these was made by any Egyptian, and only two such were rarely brought in by Greeks in late times.

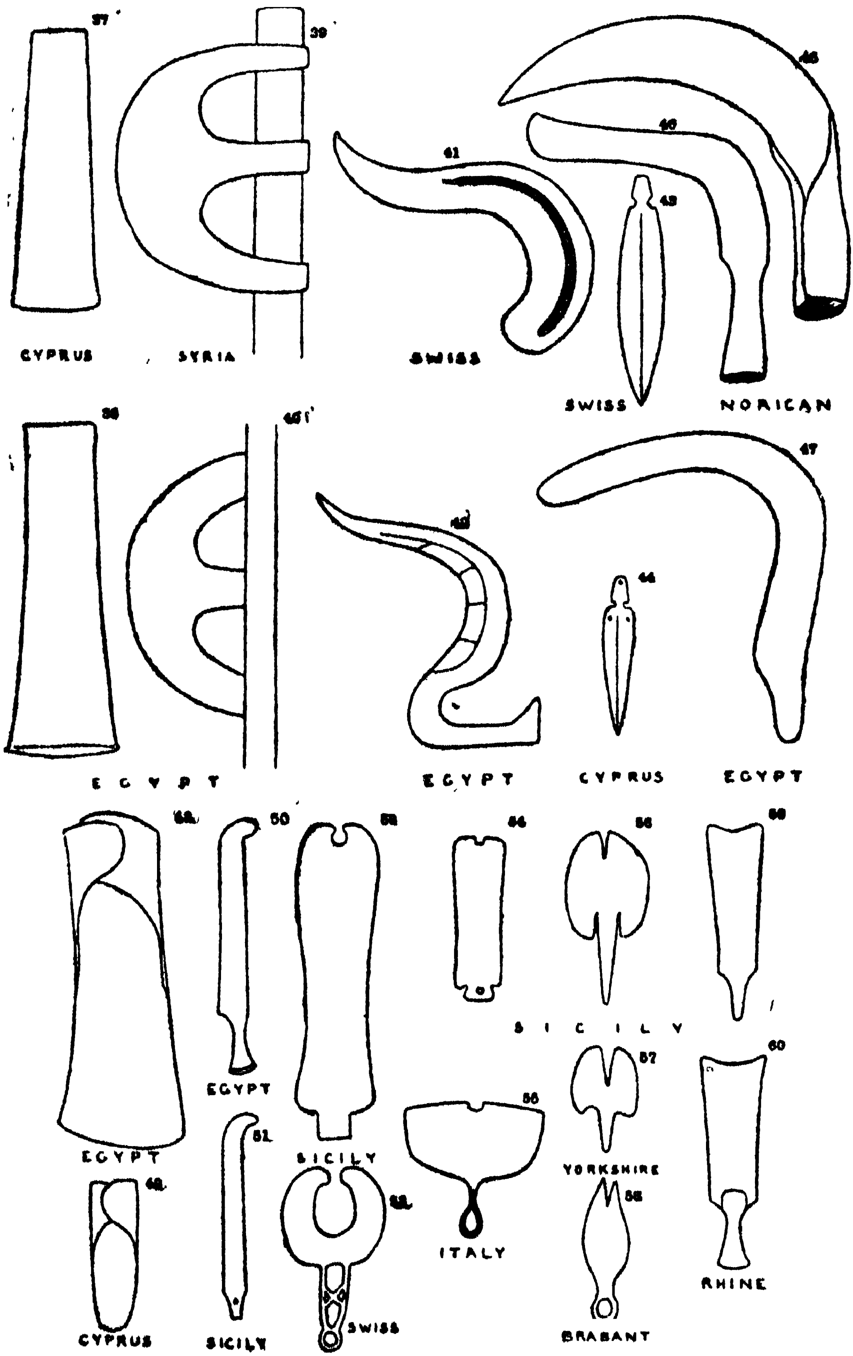
The principle of sockets for handles was well developed in Italy and spread elsewhere, for axes, hammers, and chisels, yet no Egyptian would make a socketed tool, and the only ones in Egypt were brought in by Greeks. The use of hammered sides to a blade, to form a flange for stiffening it, was of early

date in Syria, and general in the north. Yet it is rare, and probably foreign, in Egypt, and unknown in the Mediterranean. The girdle knife (fig. 19) is common in the West and in Asia; the flamboyant-blade hunting-knife (fig. 14) was usual in Italy, and spread into the north; the sword was the staple weapon in the North. Yet none of these were adopted by Egypt, and very few swords have been found there, nearly all foreign. In all these cases Egypt did not require a loan from the other lands.

This sharp separation between countries endured for thousands of years, while they were trading in food, materials, and manufacture continually. We can only conclude that each country already had, in these respects, what best suited it.

We now turn to the other historical point of view, the forms which are widely spread, because they were required. In Egypt at about 5,500 B.C. there suddenly appeared a very large wide-splayed adze (fig. 38), different from all that came before or developed later. The same large splayed adze (fig. 37) appears in Cyprus; it evidently came from there to Egypt, or both lands drew on a common source elsewhere. About 4,200 B.C. the axe with two large scollops in the back edge (fig. 40), leaving three points of attachment, suddenly appears in Egypt; a thousand years later it is far more advanced in Syria (fig. 39) than in Egypt, and it probably originated there, and spread also to Greece. About 3,000 B.C. a very strange drawing of a sickle appears in Egypt (fig. 42) unlike any other there; this is closely like a Swiss form (fig. 41). At the same time small daggers with notched tangs appear both in Switzerland (fig. 43) and in Cyprus (fig. 44). Here are links from the European copper age to the East. The same line of connection appears later, about 1,200 B.C., when the pruning-hook (figs. 45, 46) from Noricum (the modern mines of Styria) appears in Egypt (fig. 47); the rhombic arrow-head of Greece and Italy is found also in Egypt, the bronze hoe of Cyprus (fig. 49) and Egypt (fig. 48) spread northward in the Iron Age, and the European sword was rarely brought into Egypt.

An interesting confirmation of history is seen in the knives with straight parallel blades and turned-over ends. These are characteristic of the Siculi in Sicily (fig. 51), and just at the time when the Shakal people were attacking Egypt the same knife (fig. 50) is figured in an Egyptian tomb, and a specimen



37 to 51.—Forms of tools alike in East or West.
52 to 60.—Four variants of Sicilian razor, separately adopted in the North.

also has been found. This proves the connection between the Siculi and Egypt at the time.

A curious evidence of different trade routes is given by the razor. An unusual form in Sicily has a concave hollow or notch in the end (figs. 52, 54) which was reduced to a mere split (fig. 56), or a slight hollow (fig. 59). The notch form travelled into Italy (fig. 55), by the simple way across the strait. The concave hollow widened as a crescent travelled up to Switzerland (fig. 53) and Germany (fig. 60), probably by the Adriatic. The split form (fig. 56) travelled to Flanders (fig. 58) and England (fig. 57), probably by the Rhone. Here four different modifications branch from a type, and are carried by different routes to distant lands.

The triangular arrow-head is believed to have been started in South Russia. Thence it spread over Central Europe and Central Asia, and was taken by the Scythian migration into Syria about 600 B.C., and hence into Egypt.

Thus the spread of forms throughout the ancient world illustrates the movements of trade and of warfare, while the isolation of various types at the same time shows how efficient and self-supporting the ancient civilisations were in most requirements. The history of tools has yet to be studied by a far more complete collection of material, above all of specimens exactly dated from scientific excavations. It will certainly be, in the future, an important aid in tracing the growth and decay of civilisations, the natural history of man.

THE RELATIONSHIP OF THE MOST ANCIENT FLINT IMPLEMENTS TO THE LATER RIVER-DRIFT PALÆOLITHS

By J. REID MOIR, F.R.A.I.

THERE are very few serious prehistorians of the present day who believe that the earliest palæolithic implements of the river-drift represent man's first efforts to fashion flints to his needs.

If a typical example of these implements be examined, it will be recognised at once that the specimen owes its outline and form to a series of dexterous blows delivered by some one with a very definite idea of the kind of implement he wished to produce, and a thorough grasp of the art of flint flaking. It would seem to be contrary to reason and experience to suppose that some primitive being in the remote past should, without any prior knowledge of the flaking of flint, be able to produce such a specimen. And this opinion is strengthened if we try to conjecture what a present-day member of the human race would be able to accomplish in implement-making under similar circumstances.

Let us suppose that a person who had no knowledge of the fracture of flint, and had never seen or heard of such things as flint implements, was confronted with a block of flint and a hammer-stone. It may be supposed, also, that the present-day representative of mankind would possess a brain more alert and receptive than his ancient and untutored ancestor. And yet even with this very great advantage is it possible to believe that the modern person would be able to produce a symmetrical and well-flaked palæolith ? The answer to this question must, in the author's opinion, be a decided negative, and he considers that there is no reason to believe that any ancient and primitive member of the human race would, under the same circumstances of ignorance, be any more successful.

It seems then that the earliest palæolithic implements cannot represent man's first efforts in flint-flaking, but are, in all

probability, the outcome of long periods of time, during which a slow process of evolution in flint-implement making was in progress. And this seems a reasonable supposition. The flaking of flint is an art, one which might perhaps be termed an "industrial art," and we are all aware that most, if not all, art has slowly evolved, and is still evolving. It is no doubt somewhat difficult for us, who are familiar with all and every form of elaborately flaked flint implements, to realise that the person who made the first and most primitive edge-trimmed scraper or borer was, no doubt, regarded by the other members of the horde as an expert flaker of flint. He was as a matter of fact much more than that, he was a discoverer, a maker of new knowledge, which was to be of enormous value to his contemporaries and to those who lived after him. And so it was with all advances in implement making: they must have been epoch marking in the same way as great advances in various ways are at the present day. There seems little doubt from the scanty signs of improvement in the flint implements of any given geological horizon, that evolution in implement making proceeded very slowly. But this need not necessarily cause surprise when the lack of incentive to progress, and the general low standard of mental power of the primitive human beings are considered. In the present paper the author desires to draw attention to the apparent relationship of the most ancient and primitive edge-trimmed implements (generally known as "eoliths"), to the later and more highly evolved palæoliths such as are found usually in river-terrace gravels, etc. The author would like it to be understood that this supposed relationship extends only to the St. Acheul river-drift implements, and that he makes no claim, at present, to associate the later palæolithic specimens of the Moustier, Aurignac, Solutré, and Madeleine periods with the above-mentioned edge-trimmed eolithic flints.

It has generally been held that the beds containing the pre-palæolithic implements were separated by a considerable period of time from those in which the normal palæoliths occur; and this may or may not be true. But the author is concerned solely with the form and flaking of the various specimens described in this paper, and the somewhat complex geological problems involved in their provenance must be left to others for solution. After careful thought it has been

decided, for the sake of clearness, to illustrate this paper by means of slightly "idealised" drawings. These drawings however have been executed under the close supervision of the author, and are based upon a large series of actual implements. But before proceeding to describe these drawings it is necessary to write a few lines on the question of flint-flaking. To every one who has taken the trouble, or rather experienced the pleasure of flaking flint, it has become clear that to be able to detach flakes with precision, it is needful, whenever possible, to have a flat surface upon which to deliver the necessary blows. It is almost impossible to detach flakes from a rounded surface of flint owing to the fact that the hammer-stone cannot "get home," so to speak, but glances off ineffectively. There is no doubt that the necessity for a flat surface of flint in flaking was one of the first discoveries made by man in his earliest efforts to shape flints to his needs. The most ancient edge-trimmed stones are almost invariably of a tabular form, and it seems that these stones were selected because they afforded two naturally formed broad flat surfaces upon which to deliver blows with a hammer-stone. As time went on, as will be shown, it was found possible to produce the necessary flat surface by flaking, and this production of a "striking-platform" was, and always must be, one of the fundamental necessities in flint-implement making. In the author's opinion the rostro-carinate or "eagle's-beak" type of implement plays an important part in the evolution which it is the intention of this paper to describe.[1] It seems necessary, therefore, to give an accurate description and drawing of an ideal rostro-carinate implement, such as the ancient flakers of flint apparently had in their mind, but to which ideal they did not often attain.

The following description is copied from that given in Sir Ray Lankester's Memoir (*Phil. Trans. Roy. Soc.*, May 1912) and the accompanying drawing of an ideal rostro-carinate is also taken from the same publication.

A rostro-carinate is an implement with broad posterior region, narrowed anteriorly to a quasi-vertical cutting edge. This anterior narrow edge is strongly curved and gives the

[1] See *Journal of the Royal Anthropological Institute*, vol. xlvi. January–June 1916. These particular specimens were first found by the author in 1909 in the detritus-bed below the Pliocene Red Crag of Suffolk, and have been described fully by Sir Ray Lankester and by himself.

implement the form of the beak of an accipitrine bird. The form of this region of the implement may also be compared to that of the prow of a boat (the boat being turned keel upwards). If the implement is held with the prow or beak to the front, there are observed an upper or dorsal plane, a lower or ventral plane, a right lateral and left lateral surface, a posterior surface or stern, and an anterior surface, narrowed to the form of a keel and ending in a beak (hence the term

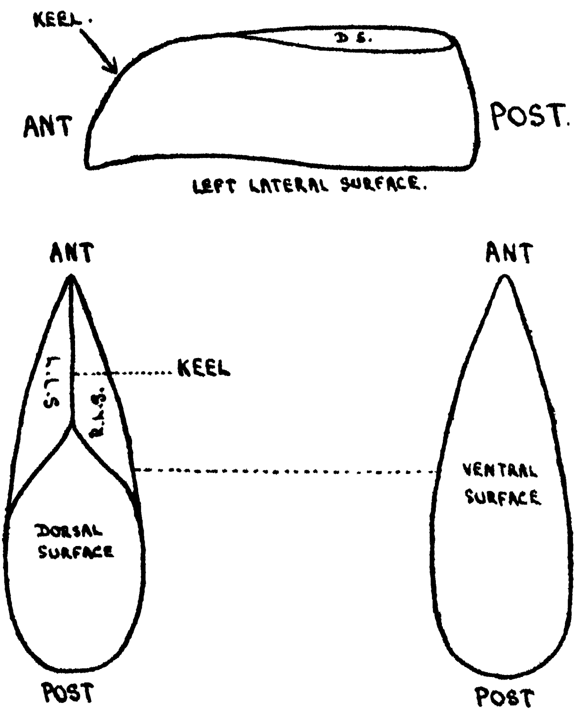


Fig. A.

"rostro-carinate") as a consequence of the oblique direction and convergence of the lateral surfaces, which approach one another so as to leave only a narrow keel-like ridge between them (see fig. A). It is proposed to indicate in each text figure the anterior (ANT) and posterior (POST) region of each specimen portrayed. The upper, dorsal surface, and the lower, ventral surface, will be indicated by the letters D.S. and V.S. respectively, while the left lateral surface (L.L.S.) and the right

lateral surface (R.L.S.) will also be delineated. A sectional drawing of each implement will also be given.

The " keel " of the specimens exhibiting this feature will be indicated clearly, and the author thinks that the drawings will be readily understood by the reader. The arrows marked in on the flake-areas of the specimens indicate the direction of the blows which removed the flakes.

Fig. 1.—The most primitive type of flint implement known is here represented. It is simply a more or less tabular piece of

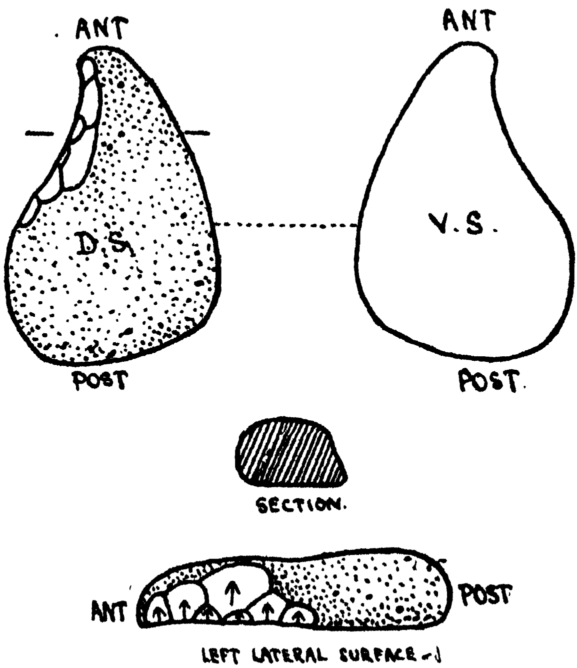


FIG. 1.

flint, the dorsal surface of which exhibits unflaked cortex, while the ventral surface shows the hard interior of the flint which has become exposed owing to a clean thermal fracture. On one side of this flat thermal fracture surface blows have been delivered and flakes removed, so that a hollow has been produced in the edge of the stone, which encroaches on the dorsal surface. It will be noticed that all the arrows marked in on the flake areas of this hollow, which forms part of the left lateral surface of the implement, point away from

the flat ventral surface, demonstrating that the maker of the implement delivered all his blows upon this flat surface. And any one experienced in the flaking of flint would follow his example, as the more uneven dorsal surface would afford a much less satisfactory striking-platform upon which to deliver flake-removing blows with precision. The specimen is D-shaped in section. Such an implement would be of service for scraping purposes, and the type is frequently met with

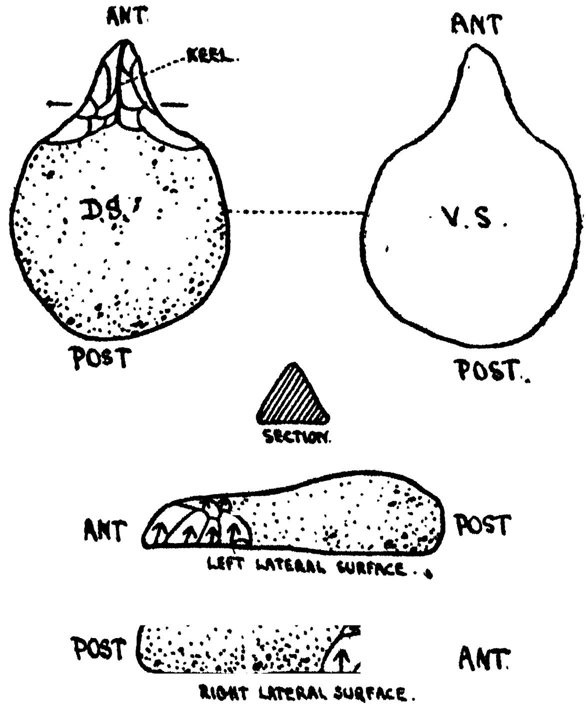


FIG. 2.

in the high plateau gravels of Kent, and in other ancient deposits of pre-palæolithic age.

Fig. 2.—Represents another very primitive type of flint implement often found in association with the form represented in fig. 1. This specimen (fig. 2), however, exhibits an advance on the first described implement. It will be seen that another hollow has been flaked in the side of the stone opposite to that shown in fig. 1, and that the specimen has now assumed a definite pointed form. The production of this extra hollow

would have provided the ancient flint flaker with two scraping edges instead of one, and it is the author's opinion that this was the result which he wished to attain. The pointed form was simply the inevitable result of the production of the two opposing hollows. But there was also another inevitable result which, apparently, had a great effect in the evolution of flint implements. As the respective fracture-surfaces of the two hollows encroached upon the dorsal surface of the specimen, and finally met, a ridge or gable was formed (marked KEEL in drawing). It does not appear probable that the formation of this keel was the object of the ancient workman, but that it was the inevitable outcome of the production of the two hollows, as any one can easily prove by flaking a flint to the form of the specimen represented in fig. 2. But it was not long before the possibilities of this sharp keel being used as a cutting-edge, and its superiority as such over the cutting-edges in use previously, were recognised, and from then onwards the efforts of these early flint flakers appear to have been directed to the production of such "keels" or cutting edges.

The right lateral and left lateral views of the specimen show that, as in the implement represented in fig. 1, all the blows forming the two hollows were delivered on the flat ventral surface. A glance at either of these lateral views will show also that the anterior region has already assumed in profile the appearance of a beak, and is prophetic of the later rostro-carinate specimens. This beak-like appearance is again attributable to the production of the two opposing hollows, and is the almost inevitable result of such production, as can easily be tested by any one desirous of doing so.

The section of the specimen is triangular, the base of the triangle representing the ventral surface, the two sides the right lateral and left lateral surfaces respectively, while the apex represents the keel or gable formed by the convergence of the flake areas, forming the two opposing hollows. It will be noticed that both the implements represented in figs. 1 and 2 are fashioned from pieces of tabular flint which provided naturally formed striking-platforms for the delivery of flake-removing blows.

Fig. 3.—Represents a rostro-carinate or eagle's-beak implement such as is found in the detritus-bed below the Pliocene Red Crag of Suffolk. It appears that at the time when these imple-

ments were fashioned tabular flint was not obtainable, as th author has not yet found a sub-crag rostro-carinate made from a piece of tabular flint, and the extensive diggings which hav

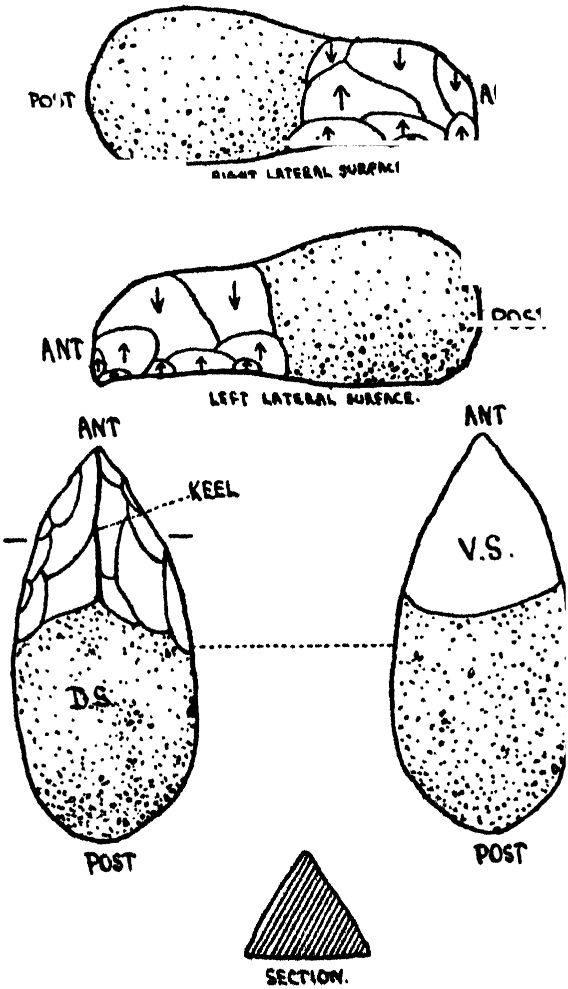


Fig. 3

been conducted have shown that this particular kind of flint is hardly ever found in the detritus-bed.

The pre-Crag people, however, had an abundance of flint of very fine quality, in the form of nodules, with which to work, but the more or less rounded surfaces of nodules did not afford

a satisfactory striking-platform, and so they had to learn to provide themselves by flaking with a flat surface upon which blows could be struck with precision. There is no doubt that the sub-Crag rostro-carinate implement, though generally much larger and of a more imposing appearance than the primitive implement represented in fig. 2, is nevertheless made on almost the same lines. The ventral surface of the rostro-carinate formed by the removal of a large flake from the original flint nodule represents the natural flat surface of tabular flint, and in both cases blows were delivered on each side of this flat surface. But whereas it seems that in the case of the primitive implements represented in fig. 2 the result aimed at was the production of two scraping hollows, in the rostro-carinate the keel or gable seems to have been the desired object.

It will be noticed that there is a slight difference in the method of production of the rostro-carinate from that of the implement represented in fig. 2. In the latter all the blows were delivered on the flat ventral surface, and while this is the case with most of the blows which went to form the keel of the rostro-carinate, yet one or two were delivered on the dorsal surface of the specimen. The author has found by many experiments in flint-making that in forming the keel it is sometimes necessary to deliver some blows on the dorsal surface. The section of the rostro-carinate implement is triangular as in the case of fig. 2.

Fig. 4.—It has been noticed, from an examination of a number of rostro-carinate specimens found in deposits of less antiquity than the detritus-bed below the Pliocene Red Crag, but more ancient than the river valley gravels, etc., that the ventral surface, or " striking-platform " as it might be termed, was gradually extended further backwards towards the posterior region of the implement.

This extension of the ventral surface made possible the corresponding extension of the keel, until an implement was produced having a cutting edge extending the whole length of its dorsal surface. Such a specimen is illustrated in fig. 4. The section of this implement is still triangular, and the majority of the blows forming the keel were delivered upon the flat ventral surface. Several specimens of this type have been found from time to time in river-gravels associated with early palæolithic

implements, and long before rostro-carinates were discovered were collected and preserved as "side-choppers." But there can be little doubt that such specimens are simply rostro-carinate implements in a high state of development, and moreover their method of manufacture is fundamentally the same as that followed in the production of the more primitive imple-

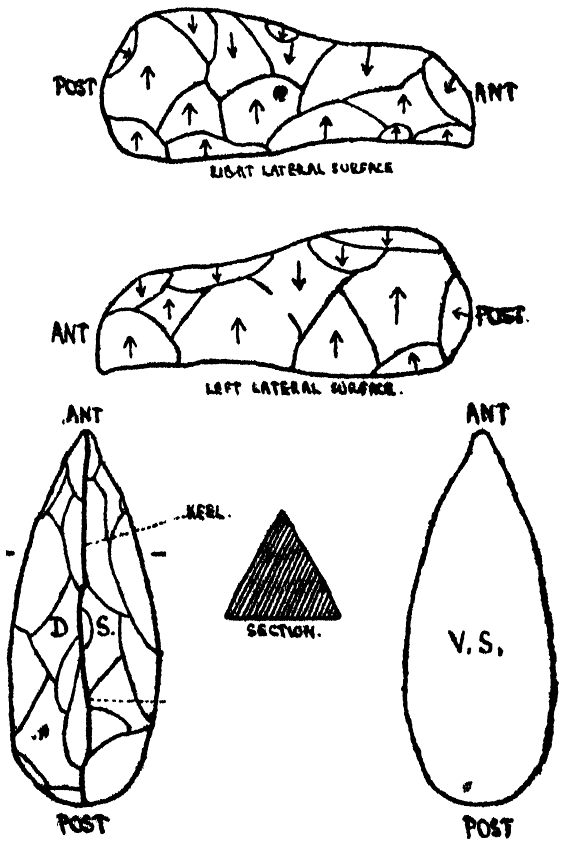


FIG. 4.

ments illustrated in figs. 2 and 3. This fact can be easily corroborated by any one prepared to experiment in flint flaking on his own account.

Fig. 5.—It seems that the palæolithic workmen having realised the advantage of a cutting-edge extending the whole length of the dorsal surface of their implements, realised also that it would be a great advantage to have at their disposal

another and opposite cutting-edge. We have seen that so far in implement-making the flat ventral surface had been an absolute necessity for the production of the required cutting edges. On this ventral surface or striking-platform the majority of the blows had been delivered, and it was the presence of this same flat surface which made it possible for the palæolithic

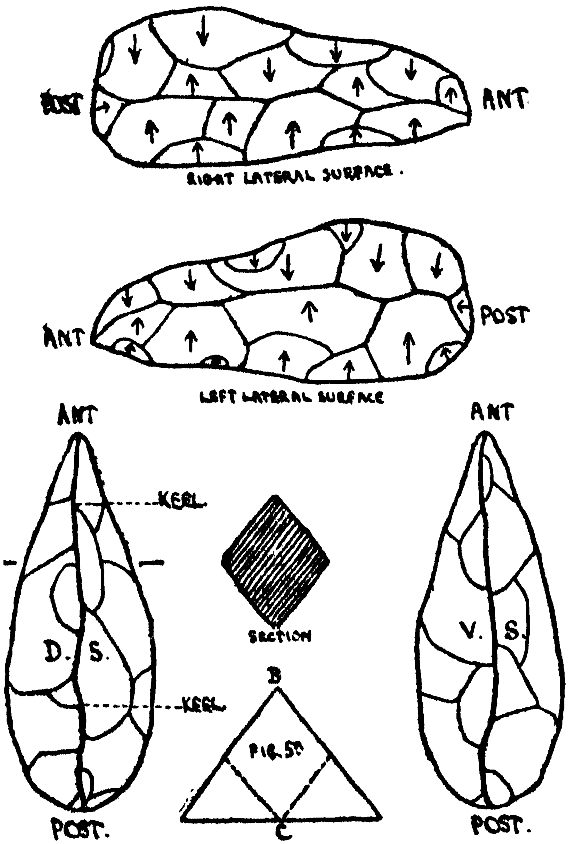


FIG. 5.

workman to develop his implements still further. If a number of the earliest palæoliths are examined it will be noticed that they are roughly rhomboidal in section, and thus differ from the triangular form of the implements represented in figs. 2 and 3. Fig. 5*a* shows how, in all probability, this change from the triangular to the rhomboidal form was brought about. The two areas indicated by crosses and limited by dotted lines were

removed by blows delivered principally upon the flat ventral surface, or base of the triangle, and a cutting edge (*C*) formed in opposition to that at *B*, which, as we have seen, was the keel of the more primitive type of implements.

The author has been able to examine a large number of early palæolithic implements in various public and private collections and has recognised many specimens which, though exhibiting the two cutting-edges of the normal palæolith, nevertheless show in their profile a marked resemblance to the profile of the rostro-carinate implements (fig. 5). That is to say that one edge of these early palæoliths is markedly curved towards the anterior region of the implement, while the other edge is much straighter.[1] He has noticed also that many of these specimens exhibit the remains of either the dorsal or ventral surfaces of the rostro-carinate form. Such a resemblance would be expected if the palæoliths were evolved from the rostro-carinates, and it is very significant that such forms should occur so freely in the earliest palæoliths which are nearest in point of time to the rostro-carinates.

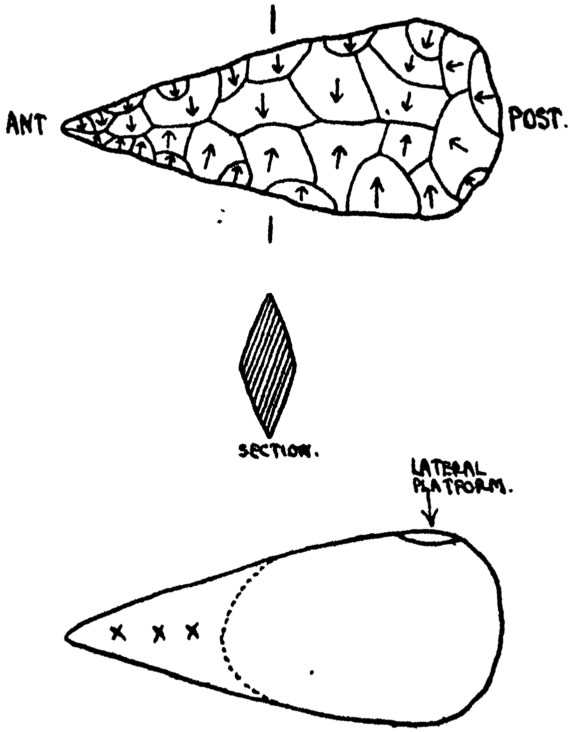
The author has experimented extensively in the flaking of flint, and has himself produced flint implements of early palæolithic type by following the procedure outlined above, and he has found that in several cases the outline of the rostro-carinate form has been preserved. He has found also that the remains of either the dorsal or ventral surfaces of the rostro-carinate are sometimes left at the butt-end or posterior region of the implement. It has been the custom to figure and regard palæolithic implements with their points uppermost, and in consequence the remains of the dorsal or ventral surfaces have been spoken of as "lateral platforms." But if these specimens are regarded with the point to the left, or the right as the case may be, these platforms are no longer lateral, but dorsal or ventral, and their true significance can be recognised.

Fig. 6.—This drawing represents a highly evolved palæolithic implement in which both edges are symmetrical, and the likeness to the ancestral rostro-carinate form has almost disappeared.

Fig. 7.—Represents an ovate palæolithic implement. These

[1] The downward curvature of one edge represents the curving of the keel towards the anterior region, while the straighter edge represents the flat ventral surface of the rostro-carinate form.

implements, which were made on precisely the same plan as the pointed examples, owe their ovoid shape to the substitution of a rounded cutting edge for the pointed end, and their evolution may be due to the breaking off of the area indicated by crosses, and the re-chipping of the broken end into a curve instead of a point. Ovate palæoliths often exhibit a "lateral platform," which, as has been shown, is probably the remains of either the dorsal or ventral surfaces of the rostro-carinate implement.



FIGS. 6 and 7.

The chief points of this paper may be summarised as follows:

(1) The most primitive implement known is a tabular piece of flint with a hollow flaked out of one of its edges.

(2) The next stage is represented by a similar piece of tabular flint in which two opposing hollows have been fashioned in its edges. The flake areas of these two hollows have converged and formed a keel or gable, and have also inevitably produced a beak-like profile at the anterior region of the implement.

(3) This keel or gable and the beak-like profile are still more marked in the sub-Red Crag rostro-carinate, and in the implements of this type found in deposits intermediate in age between the sub-Crag detritus-bed and the river valley gravels, the flat ventral surface and the keel are gradually extended further back towards the posterior region.

(4) This extension of the keel culminates in the production of the early palæolithic side-chopper in which a cutting-edge extends continuously from the anterior to the posterior region.

(5) The triangular section of the pointed eolithic and rostro-carinate implements is transformed in the earliest palæoliths into a section which is roughly rhomboidal. This change was in all probability brought about by the removal by flaking of each side of the flat ventral surface of the rostro-carinate form, so that a thin cutting-edge was left.

(6) These earliest palæolithic implements often exhibit a marked resemblance in their profile to the rostro-carinate form, and the remains of the dorsal or ventral surface of this latter type are often left at the butt-end of the implements. The remains of these surfaces have been called erroneously "lateral platforms."

(7) The most highly evolved palæoliths are those with straight symmetrical cutting edges, in which the rostro-carinate-like profile has almost disappeared.

It will thus be seen that the author is of the opinion that the most primitive "eolithic" implement is linked up with and related to the most symmetrical and perfect palæolith of the river-drift deposits. It is also his opinion, as a practical flaker of flint, that there is no other way of making the implements figured, except that described in this paper. But he does not claim infallibility, and it may be that some other investigator may be able to demonstrate a more accurate and better way.

NOTE.—In the article on "The Oldest Flint Implements" in SCIENCE PROGRESS for January 1917, p. 435, line 20, for [illegible] in. *read* 1[illegible] in.

POPULAR SCIENCE

THE ERUPTION OF SAKURA-JIMA ON JANUARY 12, 1914

By CHARLES DAVISON, Sc.D., F.G.S.

ON January 12, 1914, a violent eruption occurred in the volcano of Sakura-jima near the southern extremity of Japan. The eruption is remarkable for the vast amount of material ejected within a brief interval of time, for the extraordinary crustal movements within and near the volcano, for the close connection of the eruption with a strong tectonic earthquake, and for the anomalous forms of the areas within which the loud detonations were heard. The various phenomena of the eruption have been examined by Prof. F. Omori, the well-known director of the Seismological Institute at Tokyo. The memoirs[1] in which his investigations are described are among the most important of recent contributions to our knowledge of volcanic action, and are of sufficient importance to justify the somewhat lengthy abstract which is contained in the following pages.

Topography.—From the south coast of Kyushu, the most southerly of the main islands of the Japanese empire, the Bay of Kagoshima runs in a northerly direction for more than forty miles inland (fig. 1). Sakura-jima, until 1914 an island but now a peninsula, rises from the northern portion of the bay. It was separated from the main island by channels, that on the west side varying in width from 1½ to 2½ miles and in depth from 38 to 77 fathoms, that on the east side (the Straits of Seto) being less than half a mile wide and 45 fathoms in maximum depth. In the northern portion of the bay, which is surrounded as a rule by a steep coast, the depth of water is nearly uniform, ranging from about 70 to 107 fathoms. It is a basin similar, perhaps, in origin to the deep lakes which lie behind the coastal volcanoes of Usu-san and Tarumai-san in the northern islands of Hokkaido.

[1] "The Sakura-jima Eruptions and Earthquakes," *Bulletin of the Imperial Earthquake Investigation Committee* (Tokyo), vol. viii. pp. 1–34 (1914), 36–179, 181–321 (1916).

Sakura-jima is roughly elliptical in form, six miles long from east to west and five miles wide. Near the centre of the island are two lofty peaks, the Minami-dake or south crater, and the Kita-dake or north crater. These peaks, which are respectively 3,509 and 3,719 ft. in height, are connected by a slightly depressed ridge rather more than a mile in length. In the south-east of the island there is a flat ring-shaped parasitic

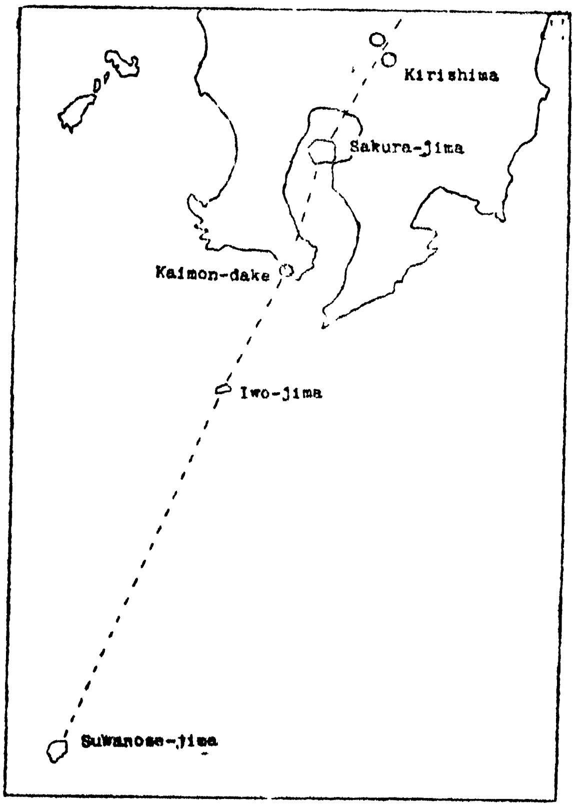


FIG. 1.—Map of the volcanic chain of South Kyushu.

cone, the Nabe-yama, 1,087 ft. high. On the west side, the most conspicuous crater is that of Hikinohira, 1,814 ft. in height. The total volume of the island above sea-level is about $6\frac{1}{4}$ cubic miles.

Epochs of Volcanic Activity in Japan.—The four large islands of the Japanese empire, Hokkaido, Honshu, Shikoku, and Kyushu, lie along a great arc, the convex side of which

slopes steeply into one of the deep basins of the Pacific. Crossing this arc almost transversely, there are two great volcanic zones, that of Fuji in the main island of Honshu, which meets the arc near its centre, and that of the island of Kyushu, which diverges from the arc at its southern end.

It is remarkable how closely the volcanic actions in these two zones are connected. The epoch of most violent eruptions in Japanese history was that of the 14½ years from 1777 to 1792, marked by the outbursts of O-shima in 1777, Sakura-jima in 1779, Aogo-shima in 1780, Asama-yama in 1783, and Unsen-dake in 1792. Of these volcanoes, the second and fifth are in Kyushu, the other three in the Fuji zone; yet, separated as they are by a distance of about five hundred miles, these different volcanoes were thrown, one after another within fifteen years, into great eruption.

For 130 years, the volcanoes O-shima, Sakura-jima, and Asama-yama have remained almost quiescent, and then again they broke into nearly simultaneous activity. From 1908, and especially from 1913, the explosions of Asama-yama have been numerous and violent. In 1912 there were magnificent outbursts from O-shima. In Kirishima, a volcano not far to the north of Sakura-jima, there were strong explosions on November 8 and December 9, 1913, and January 8, 1914. And these were followed four days later by the great eruption of Sakura-jima. During the six years from 1909 to 1914, Prof. Omori enumerates 194 eruptions from eleven different volcanoes. In the last year, 1914, as many as seven volcanoes were in action.

Previous Eruptions of Sakura-jima.—The record of the Sakura-jima eruptions dates from the year 1468, and from that time until 1914 includes 26 eruptions. Prof. Omori remarks that there are, in these four and a half centuries, three principal epochs of activity. The first extends over the eight years 1468 to 1476 and includes five eruptions, of which those of 1471, 1475, and 1476 were large outbursts. Then, after the lapse of nearly two centuries, there succeeded a series of eight small eruptions, spread over a century and a quarter (1642 to 1766). These may be regarded as merely preliminary to the second epoch of activity consisting of the great eruption of 1779, which began on November 9 and, after a few days of excessive violence, subsided into a series of minor outbursts lasting during the next ten months. The last nineteen years of the

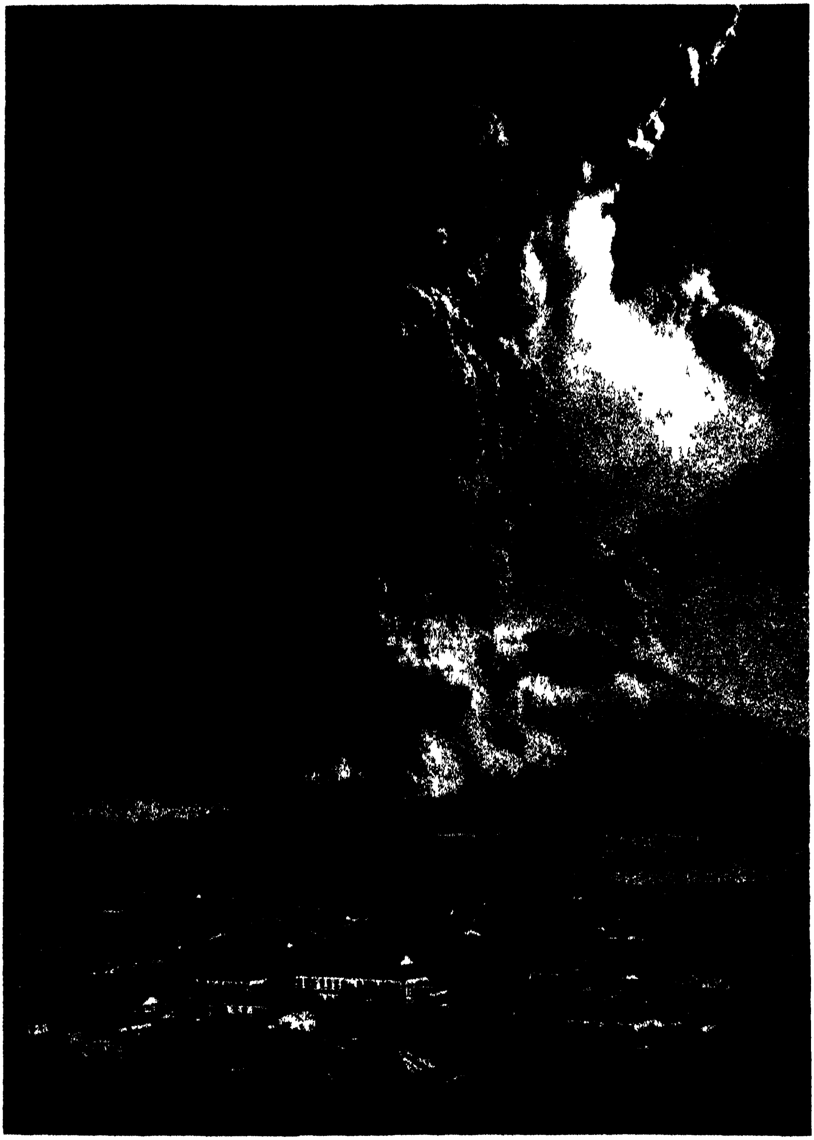
second epoch were marked by ten small eruptions, and were followed by a period of inactivity of 115 years, during which there was only one small outburst in the year 1860. This lasted until the opening in January 1914 of the third epoch of eruptive violence.

In each of these great epochs, there occurred great flows of lava from a number of craterlets arranged along nearly opposite flanks of the mountain. The positions of the 1914 craterlets are shown by round spots in fig. 2. The areas covered by the corresponding lava streams are represented by the shaded portions of the map, the lavas of the first epoch by vertical shading, in the second by horizontal, and in the third by diagonal shading. It will be noticed that, in the first epoch, the outflow took place on the E.N.E. and S.W. flanks of the volcano; in the second, on the N.N.E. and S. flanks; and, in the third, on the E.S.E. and W.N.W. sides. In the first epoch, the first outflow occurred in 1471 on the E.N.E. side, followed by two others, a few years later, in 1475 and 1476, in the south-west. In the second and third epochs, the outflows took place almost simultaneously on opposite sides, the time-intervals between them being only a few seconds and a couple of hours. The total areas covered by the lava-streams above sea-level are respectively $2\frac{1}{2}$, $2\frac{1}{4}$, and $6\frac{1}{8}$ square miles.

Premonitory Signs.—The earliest heralds of the coming eruption consisted, as usual, of a series of slight earthquake-shocks. On January 11, about 3.41 a.m., people in Kagoshima were roused from sleep by an earthquake evidently proceeding from Sakura-jima. From this time onwards, the Gray-Milne seismograph at the Kagoshima observatory recorded many shocks, which rapidly increased in frequency, the average hourly number being 4·1 from 3 a.m. to 11 a.m. on January 11; 12·4 from 11 a.m. to 8 p.m.; and 19·5 from 8 p.m. on the 11th to 10 a.m. on the 12th. The total number of earthquakes registered during these 32 hours was 418.

In Sakura-jima itself, the earthquakes must have been many times more numerous. At the village of Saido, on the north-west coast of the island, 66 earthquakes were felt during a single hour, from noon to 1 p.m. on January 11. In the same time, only nine tremors were registered at Kagoshima.

A day or so later, there were other symptoms of the approaching outburst. Early in the morning of January 12, hot-springs



Initial phase of the eruption, as seen from Kagoshima

issued copiously at several places along the southern coast of the island; while, somewhat later, cold springs on the opposite side greatly increased their flow. Lastly, at about 8 a.m. on January 12, a column of white smoke suddenly shot up in the form of a pine-tree from the top of the Minami-dake or south crater.[1]

The Eruption.—The first outburst took place on the west side of Sakura-jima at about 10 a.m. on January 12, from a point at an altitude of about 1,650 feet. This was followed ten minutes later by another from the south shoulder of the Nabe-yama on the east side of the island. The outbursts, though at first unaccompanied by special detonations, then rapidly increased in strength. At 10.30 a.m., ashes and lava fragments were projected, and at 11 a.m. the column of black smoke rose to a height of about 10,000 feet. At 2.30 p.m., the whole island was enveloped in black and white smoke, the eruptive noises became more and more intense, until at 3.30 p.m. the explosive detonations began to be heard. About this time, the smoke column had reached a height of more than 20,000 feet above sea-level.

At 6.30 p.m. an earthquake occurred of far greater severity than any of the local shocks that had been felt hitherto. After this earthquake, the volcanic noises and the projection of red-hot lava-masses became distinctly stronger, and the explosive stage reached its full intensity between 11 p.m. and 5 or 6 a.m. on the 13th, the maximum occurring at about 1 a.m. on the 13th. During this interval, the detonations appeared in Kagoshima like those due to the quick firing of artillery and were so loud that persons had to fill their ears with cotton. After 10 a.m. on the 13th, the explosions, though occurring almost continually, were greatly reduced in violence.

On the succeeding evening (January 13), at about 8.15 to 8.30, the volcano presented a magnificent aspect to the observers in Kagoshima. The burning materials were shot with brilliant display of lightning in a high pillar, converting the

[1] Owing to the promptitude of the authorities and to their recognition of the early symptoms, the whole of the inhabitants (more than 23,000 in number), with the exception of three officials who, stayed too long in the course of their duty, were safely removed from the island. This action stands in noteworthy contrast to that of the authorities in Luzon (Philippine Islands), where the early warnings were neglected, and in consequence more than 1,100 lives were lost in the eruption of Taal Volcano on January 30, 1911.

whole mountain-slope over Yokoyama into one continuous zone of red-hot masses from top to base. This was due to the first outflow of the lava-streams.

On January 25 the eruptions on the west side of the island came practically to an end. Those on the east side continued for some time longer, and, even as late as the following August, there were occasionally strong outbursts.

The positions of the craterlets in action during this eruption

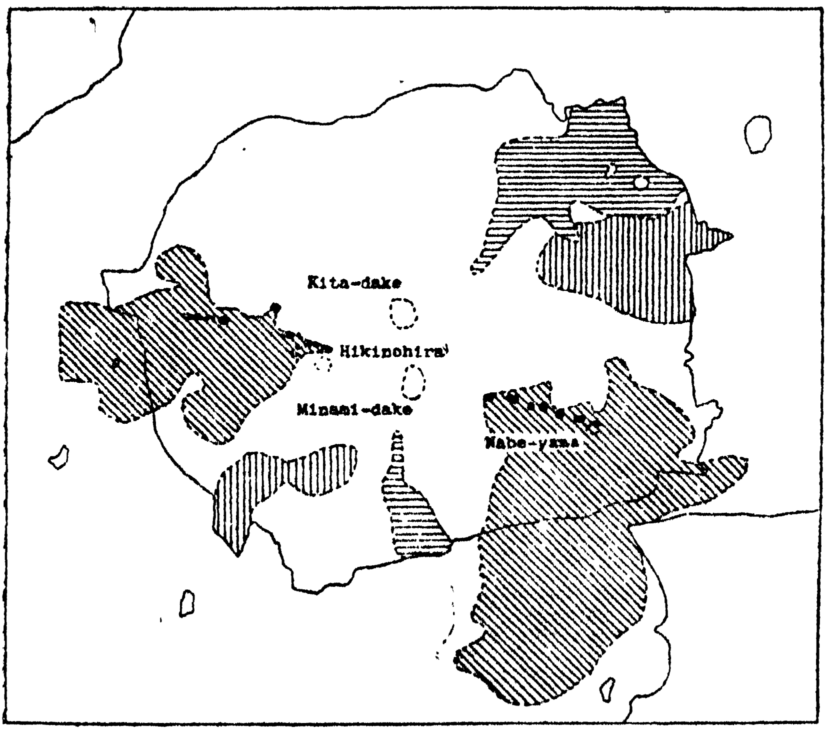


FIG. 2.—Sakura-jima and its recent lava-streams.

are shown in fig 2. They lie along a line running from E.S.E. to W.N.W. through the Minami-dake or south crater. This line, however, is not absolutely a new one, for it is almost identical with the much older line passing through Nabe-yama, Hikinoshira, and the islet of Karasu-jima.

Lava-Streams.—Lava was ejected from seven principal craterlets on the west side of the mountain and from eight on the east side, the altitudes of the highest being about 1,640 and 1,300 ft. respectively.

On the west side, the main branch of the lava-stream, which had a maximum thickness of about 200 ft., buried the villages of Yokoyama and Akamizu. This branch reached the sea on the morning of January 16, having travelled at an average rate of 2½ ft. per minute. It then continued its progress into the sea, enveloping on the 28th inst. the islet of Karasu-jima, which is 65 ft. in height, and finally stopping after an advance of three miles from the source and about seven-eighths of a mile into the sea. As the depth of the channel which separates Sakura-jima from the Kagoshima coast is usually less than 24 fathoms, there is no marked submarine extension of the lava on this side. The total area of the westerly stream (see fig. 2) is about 3¼ square miles, and its volume about ·08 of a cubic mile.

The lava streams which flowed from the eastern craterlets reached the sea on January 16, having buried in their course the villages of Seto, Waki, and Arimuza, on the south-east coast. A small hill on this coast, 406 ft. in height, though not covered by the lava at the end of January, was found entirely buried at the beginning of April. The lava-streams advanced rapidly into the deep Seto Straits, which, though originally nearly half a mile wide, were reduced in width to about twenty yards on January 24 and finally blocked up five days later, the island of Sakura-jima being thus converted into a peninsula. At the beginning of April, the lava-mass filling the straits was bulged up into hills 160 ft. and more in height.

As the sea off the south-east coast of Sakura-jima attains a depth of from 70 to 90 fathoms, the width of this lava-stream above water is everywhere less than half a mile, but the submarine extension, as shown by the soundings, is considerable, amounting in one place to as much as two miles from the coast-line. The total area of this lava-stream is six square miles, and its volume ·29 of a cubic mile. Thus, the whole volume of lava ejected is ·37 of a cubic mile or about one-seventeenth of the volume of the mountain.

Eruption-Blasts.—From the west side of the island, the plateau of Hakamagoshi projects towards the city of Kagoshima. When visited one week after the eruption, unmistakable signs of the generation of volcanic blasts were observed on and around the plateau. The school-house of the village of Hakamagoshi was entirely swept away. On a farm near the

top of the plateau, large mandarin-orange trees were uprooted and carried westwards up a slope. The blasts were, however, directed principally against the north-east corner of the plateau and the neighbouring village of Koiki. The destruction here was general, the tree-trunks being overthrown or broken in directions varying between lines which, when produced backwards, pass through the highest and lowest craterlets of the western series.

On the eastern side, no distinct trace of the blast could be detected. Here, however, the craterlets are formed on ridges. On the opposite side, they lie for the most part along the trough of a V-shaped radial valley, which probably facilitated the partial westward concentration of the explosive effects.

Precipitation of Ashes.—Over the Japanese empire, westerly winds usually prevail in the upper region of the atmosphere. They are no doubt accountable for the abnormal precipitation of ashes in an easterly direction from the Japanese volcanoes. Thus, in the recent eruption, ashes were carried to a distance of 765 miles towards the north-east and only nine miles in the opposite direction. Similarly, the layer of ashes reached a depth of 3 ft. to a distance of fourteen miles on the east side of the volcano and to somewhat less than two miles on the west. At Kagoshima, the depth was less than one-eighth of an inch. On the low ground on the eastern side of Sakura-jima, ashes and pumice were piled to a depth of 7 ft., and in one valley to a depth of more than 13 ft. The total volume of ashes is estimated by Prof. Omori to amount to ·15 of a cubic mile. Adding this to the amount of lava ejected, namely ·37 of a cubic mile, the total volume of material ejected is thus slightly more than half a cubic mile, or about one-twelfth of the volume of the whole mountain.

Earthquakes of January 12, etc.—At 6.30 p.m. on January 12, an earthquake occurred differing widely from the local shocks which so frequently precede and accompany volanic eruptions. It coincided, as regards time, with a marked increase in the eruptive activity of Sakura-jima, and its epicentre, though not exactly known, cannot have been far distant from the island. On these accounts, it may be regarded as a volcanic earthquake. But, while the typical volcanic earthquake is of brief duration and is strong with a limited area, and rarely recorded by distant seismographs, the Kagoshima earthquake was of

Houses in the eastern part of Sakurajima buried in ashes

Mandarin orange tree uprooted and carried by the eruption blasts from the village below to the plateau of Hakamagoshi

considerable strength and duration, and was registered as a great earthquake in European observatories. In Kagoshima, walls were overthrown and nineteen persons killed. It is evident, as Prof. Omori remarks, that the earthquake must have originated at a great depth, and must have been caused by stresses acting along the whole volcanic chain in southern Kyushu, rather than by the local operations within Sakura-jima.

About one or one and a half hours after the earthquake, a small seismic sea-wave or *tsunami* swept over the low-lying portion of Kagoshima. It was at first supposed to be a result of the earthquake, but the time-interval between the shock and the arrival of the sea-wave would imply too great a distance for the origin of the earthquake. The day, moreover, was that of full moon, and the tide was high at the time, the level of the water in Kagoshima harbour being only about half a yard below the top of the quay wall. There can be little doubt that Prof. Omori is correct in assigning the origin of the sea-wave to a sudden small depression in the bed of Kagoshima Bay.

Besides the great earthquake, numerous tremors accompanied the eruption. Prof. Omori registered these small movements from January 16 onwards by means of a tremor-recorder magnifying about 200 times. He distinguishes two kinds of eruptions according as they were, or were not, accompanied by loud detonations. The former were represented by small seismic movements with a total range of about $\frac{1}{250}$ of an inch, the latter by larger movements with about ten times that range. In his opinion, the loud detonations are produced by the sudden rupturing of the partially solidified lava-bottoms of the craterlets and, though alarming, are merely connected with superficial actions; while the other eruptions are more important phenomena and consist in powerful projections of ashes and gases along pre-existing and deeply-seated channels.

Relations between the Eruptions of the South Kyushu Volcanic Chain.—The eruption of Sakura-jima occurred in close connection with other outbursts in the volcanic chain of South Kyushu. It is worthy of notice that these outbursts occurred in succession from north to south, and all within the first quarter of 1914. Thus, on January 8 there was a strong explosion from Kirishima (fig. 1); on January 12, the great eruption of Sakura-jima; on February 13, the Iwo-jima was in eruption and this outburst was accompanied by a strong earthquake closely

resembling that of Kagoshima on January 12; and, lastly, on March 21, eruptions occurred in Suwanose-jima. These four volcanoes, together with Kaimon-dake, lie nearly along a straight line, the distances between successive summits from north to south being 28, 29, 31, and 91 miles. And this straight line is nearly at right angles to the line of recent craterlets in Sakura-jima.

Displacements of the Ground.—Some of the most novel, as well as the most remarkable, of Prof. Omori's observations are those which relate to the more or less lasting movements of the ground. The small tsunami or sea-wave probably pointed to some displacement of the sea-bed. It was noticed also soon after the eruption that the sea-level had undergone a change, a distinct elevation, with regard to the surrounding coast of Kagoshima Bay. For instance, at the spring tide of March 13, the low districts at the south end of Kagoshima were covered with sea-water at high tide. Along the north-western and northern parts of the bay, the relative rise was still greater, embankments and stone walls were damaged at several places, and extensive rice-fields behind them were devastated. Prof. Omori estimates the maximum rise at about 3 ft. at Kagoshima and at about 3½ ft. along the northern shore of Kagoshima Bay. The greatest change was that observed in Sakura-jima, on the north coast of which the relative rise of the sea was not less than 6½ ft.

These changes of elevation, however, were by no means permanent. They reached their maximum about the end of 1914. After this, a decrease set in, and Prof. Omori estimates that, if it should continue according to the law which governed its rate in the early stages, the sea may be only about an inch above its original level after the lapse of forty years.

As to the cause of the change of level, there can be little doubt. It can hardly be due, as has been suggested, to the heating of the waters of the Bay or to the blocking of the Straits of Seto. Prof. Omori claims that the only efficient cause is the actual subsidence of the surrounding land. That some subsidence might be expected to take place is obvious from the vast withdrawal of material, more than half a cubic mile, in the form of lava and ashes.

A more precise conception of the magnitude of the crust-displacements is afforded by a renewal of various lines of level-

ling in the district surrounding Sakura-jima. A series of levels was carried out by the Military Survey from December 1891 to January 1895. This was repeated at various times between July 1914 and May 1915. The heights of the bench-marks along the railway-lines near Kagoshima Bay were also redetermined in April and May 1915, the first series of levels having been made between 1897 and 1913.

The results of the re-surveys are shown graphically in Fig. 3.

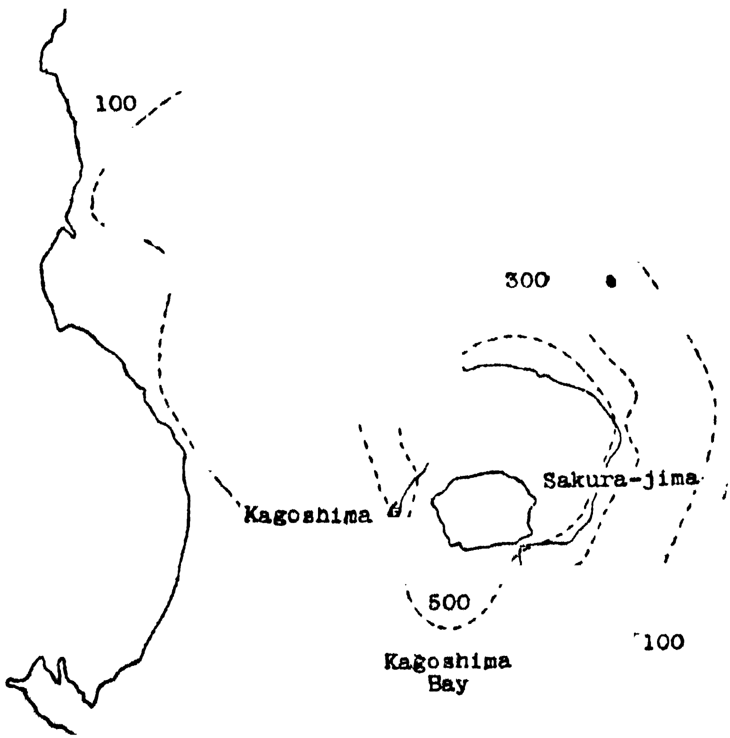


FIG. 3.—Vertical crust-displacements in the district surrounding Sakura-jima.

On this map are shown the curves of depression of 100, 300 and 500 mm. The two last curves, it will be noticed, surround the northern end of Kagoshima Bay. The axes of the curves intersect in a point lying a short distance to the north of Sakura-jima, and this point Prof. Omori regards as indicating the site of greatest depression of the ground, and as probably coincident with the principal centre of the underground lava reservoir. The latter, as he points out, may extend under the

area occupied by the northern part of Kagoshima Bay and the surrounding land, where the depression of the ground was more or less marked, an irregular circle about 28 miles in diameter. It would seem, he adds, that the particular portion of the lava reservoir which supplies magma to the active volcanoes of Sakura-jima and Kirishima is situated, not under the volcanoes themselves, but under the region between them.

The volume of the basin formed by this subsidence is of some

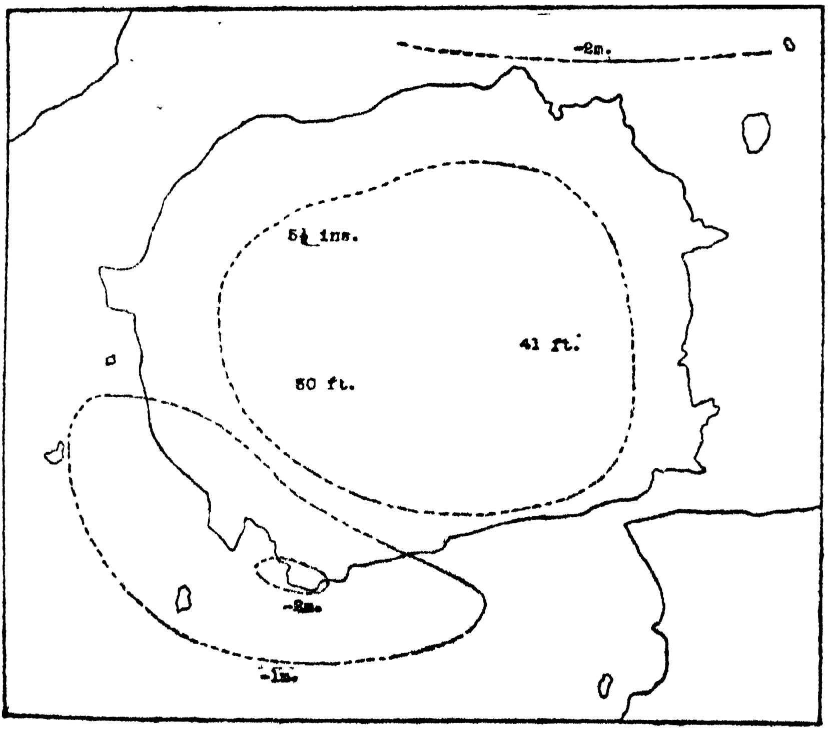


FIG. 4.—Vertical crust-displacements in Sakura-jima.

interest. Prof. Omori estimates the volume of depression within the 300 mm. curve as one-fifth of a cubic mile, and of that within the 100 mm. curve as one-eighth of a cubic mile, the total being in any case not less than one-half the combined volumes of lava and ashes ejected.

Within Sakura-jima itself, the triangulation surveys of 1898 and 1914 reveal results of no less importance, as regards both vertical and horizontal movements. The former are represented graphically in fig. 4. In the coast districts, the

movement is invariably one of depression by amounts ranging from about 1 ft. to 5½ ft. At three places on higher ground, the movement, however, is in the opposite direction. At Kamano (147 ft. in height) the measured rise is 5½ in.; at the top of Hikinohira (2,178 ft.), it amounts to 30 ft.; and at the top of Gongen-yama (1,339 ft.) near Nabe-yama, to 41 ft. Even making every allowance for the depth of ashes at these places, it is evident that the rise near the centre of the island must have been considerable.

The horizontal movements, if somewhat less in amount, are still of great importance. For instance, near the south-west coast, the displacements of three points amount to 6 ft. 8 in., 10 ft. 1 in., and 11 ft. 11 in., all to the south or a few degrees west of south. Two points close to the north-west coast were shifted 3 ft. 6 in. and 14 ft. 10 in. (the latter being the greatest horizontal displacement observed) in a nearly north direction, while a point in the extreme north of the island was moved 8 ft. 7 in. towards the north-east. The north and south portions of Sakura-jima thus appear to have been displaced outwards in opposite directions. Again, along the west coast of Kagoshima Bay, in the immediate neighbourhood of Sakura-jima, displacements of 1 ft. 10 in., 2 ft. 2 in., 3 ft. 1 in., and 2 ft. 8 in. were observed. These vary slightly in direction, but the important point about them is that all four directions, together with those in the north of Sakura-jima, converge towards an elliptical area, the position of which agrees roughly with the area of greatest depression.

Sound-Phenomena.—The sounds which accompanied the eruption may be divided into three groups: (i) the early sounds heard during the morning and afternoon of January 12; (ii) the strong detonative sounds from 6.30 p.m. on the 12th to 6 a.m. on the 13th; and (iii) the weaker sounds of the after-explosions which occurred during the ten days following the great eruption.

Observers of the gun-firing in France and Flanders have shown on numerous occasions how strangely the sound-waves are propagated by the atmosphere. The sounds of very distant guns may be heard with clearness, while those of equally large guns close at hand are quite inaudible. When the places of observation are mapped they are found to lie within two detached areas, one surrounding the source of sound, the other

separated from it by a silent zone which may be as much as fifty miles in width.

Prof. Omori has shown that the sounds of the Sakura-jima explosions were transmitted in the same abnormal manner. The detonations were of course too numerous and occurred too closely together to be traced over the country individually. But there was no difficulty in considering the propagation of the three groups of sounds. Though these groups differ greatly in strength, the detonations of the second group being by far the loudest, it is remarkable that the areas over which they were heard are practically alike in form and do not vary greatly in their dimensions. Each group of sounds, for instance, was heard within two detached areas. One of the areas includes Sakura-jima near its western margin, and in each case extends a considerable distance to the east. Though the prevailing winds in the upper atmosphere are from the west, the second sound area lies to the north of the former but is elongated in the east-west direction.

The area containing Sakura-jima varies but slightly in magnitude, the average distance of the boundary from the centre of the volcano being 69, 71, and 62 miles for the three groups of sounds. For the detached area, the average distance of the inner boundary is 89, 94, and 108 miles, and that of the outer boundary for the second and third groups 134 and 140 miles. The average distances from the volcano of the central line of the detached area are 121, 110, and 122 miles.

As might be expected, the width of the silent zone varies with the strength of the sounds It is estimated at 25 to 30 miles for the strong detonations and about 67 miles for the weak after-explosions. In both, however, the average distance from the volcano of the central line of the silent zone is about 75 miles.[1]

The extreme distance to which any detonation was heard is 310 miles towards the north-east, though the air-vibrations were strong enough to shake houses and doors to a distance of 364 miles in the same direction.

[1] Prof. Omori does not give any explanation of the anomalous forms of the Sakura-jima sound-areas. The theory of the detached areas is considered by Mr. S. Fujiwhara in a memoir, "On the Abnormal Propagation of Sound-waves in the Atmosphere," second part, *Bulletin of the Central Meteorological Observatory of Japan*, vol. ii. 1916, No. 4, pp. 1–82. A summary of this important memoir is given in *Nature*, vol. xcviii. (1916), pp. 71–2.

NOTES

"Science Progress"

WITH the commencement of our twelfth Volume further changes, but this time only small ones, will be made in the arrangement of our sections: the Recent Advances will be given first place, and the Essay-Reviews will be put in small print between the Essays and the Reviews, as seems reasonable. The Recent Advances head our Contents because in that position they enable the busy reader to see at a glance the most important scientific work which has been done during the quarter, and he can then peruse at leisure the more specialised papers which follow under the heading of Articles. The Editor would like to take this opportunity publicly to thank the contributors of Recent Advances for their work, which requires constant attention to the literature of the subjects with which they deal and, often, much consideration in the choice of the matter which is to be referred to in the short space available. We hope to add later occasional Recent Advances on various branches of Applied Science; but the reader will understand that these subjects are too large to be dealt with in any single quarterly publication except in very brief abstract.

Emil von Behring, 1854—1917 (A. S. Leyton, M.D., F.R.C.P.)

It has been a striking and a fortunate circumstance that with each of the three remarkable therapeutic strides of the last century and a quarter, of vaccines, of sera, and of organic extracts, the first application of each new step has at the same time been the most successful. The prophylaxis of small-pox by calf vaccine, the treatment of diphtheria by antitoxic serum and the cure of myxodœma by preparations of the thyroid gland have never been equalled in their success by similar efforts in other diseases.

The foundation of the specific treatment of diphtheria was laid by Behring when, in 1890, together with Kitasato, he successfully immunised and treated animals for the infections

of diphtheria and tetanus. A little later he applied these results to human patients, and after 1894 the serum treatment of diphtheria became the routine remedy. Thus to Behring is due the first successful application of serum therapy to man. His also is the merit, of much practical importance, of discovering that the horse is the most suitable animal for the preparation of the antitoxic serum. More recently, in 1913, he introduced a prophylactic remedy consisting of a neutral or nearly neutral mixture of toxin and antitoxin, which is still undergoing the test of experience.

Before his epoch-making discovery Behring had worked on the bactericidal property of serum and from it deduced the ideas which formed the basis of the humoral theory of immunity which he was the earliest to express in the following words: The immunity of rabbits and mice which have been immunised against tetanus depends on the capability of the cell-free plasma to render harmless the toxic substance produced by the tetanus bacilli.

Less well known is Behring's work on disinfection and antiseptics. Some of his researches in this direction were fundamental and of great practical value. He recognised the importance of the chemical composition of the substance to be disinfected and especially the neutralising power of proteids on many disinfectants. His book on Infection and Disinfection in 1894 was probably the first systematic treatise on this subject.

Later on Behring turned his attention also to tuberculosis, and devised a method for the immunisation of young cattle by the intravenous injection of ground-up tubercle bacilli of human type. The method was certainly successful, but, to Behring's disappointment, seems never to have been taken up, although duly confirmed by several workers.

It is perhaps becoming more common to recognise merit during the life-time of the deserver, and Behring's is one of those cases. Reputation and wealth flowed to him as the discoverer of a remedy for diphtheria. The Paris Academy of Medicine and the Institute of France both awarded him their most distinguished prizes, and later he received also the Nobel prize.

Behring possessed in high degree the rare capacity to lay a solid theoretical foundation and to build thereon a useful practical superstructure.

Prof. Count Mörner

We regret much to have to record the death of Prof. Count Karl Axel Hampus Mörner, Rector of the Royal Karolinska Institute, Stockholm, and President of the Nobel Medical Committee, on the 30th March last. He qualified as a medical man in 1884, and died at the age of sixty-two years. As President of the Nobel Medical Committee it was his duty to deliver the introductory speeches on each successful candidate for the Nobel medical prize, and his addresses were always an important feature of the distinguished ceremony in Stockholm at which the Nobel Prizes are bestowed by the King of Sweden or his representative. He had a wide knowledge of medical science, as was necessary in a person of his position, and naturally kept in touch with all the newest developments of that science. He was very fond of England and the English, spoke our language very fairly well, and often visited England, always taking care to see different portions of the British Isles. He was one of the most distinguished foreigners at the celebration of the two hundred and fiftieth anniversary in 1912 of the founding of the Royal Society. In Stockholm he was very popular among the students, and indeed with all classes. He was gentle, genial, and kind to all, and of the best type of a nation which is notoriously itself of the best type in the world.

The Choice of our Rulers (Prof. J. Wertheimer, D.Sc.)

This Empire is very largely governed by men who enter the service of the State by means of the conjoint examination for first-class clerkships and the Civil Service of India. For the leading permanent officials in the great departments of State, as well as our Proconsuls in India, are ultimately selected mainly from the successful candidates.

What is the nature of this examination, and to what extent does it allow men trained in different ways to serve the State in these capacities?

The results of the examinations in the two years immediately preceding the war show that, except in rare instances, the key to success in these examinations is the study of Greek and Latin. Taking the first-class clerkships, in 1913 sixteen vacancies were announced and the men who gained the first sixteen places secured 19,320 marks for classics and classical history; classical

history is counted in with Greek and Latin because an examination of the papers shows that nearly all the questions could be answered by a candidate who had an adequate knowledge of Greek and Latin literature; indeed this literature is the main source from which a knowledge of the histories of ancient Greece and Rome can be obtained.

The same sixteen candidates secured 254 marks for French, Italian, and German combined, 3,491 for Natural Science, and 6,707 for Mathematics. So that Mathematics, Science, and Modern Languages together were unable to score much more than half the marks awarded to Classics.

It may be thought that 1913 was an exceptional year, but this is not the case, for in 1914 nine vacancies were announced, and the candidates who gained the first nine places secured 11,560 marks for classics and classical history, 738 marks for French, Italian, and German, 1,401 for Natural Science, and 3,901 for Mathematics.

It will be seen that these results do not differ essentially from those in the preceding year.

As a consequence of this the country has hitherto been ruled mainly by persons who have scarcely any acquaintance with scientific method, and who, as often as not, are unable to understand and consequently disinclined to accept, scientific advice. We all know to what sad results this led at the beginning of the war.

The largest endowments for the teaching of Latin and Greek are to be found in Oxford and Cambridge, and the undue allotment of marks to classics in the examinations in question means that first-class clerks, and, therefore, to a large extent, the leading permanent officials, must be chosen almost exclusively from the ancient Universities. Thus in 1913 and 1914 forty first-class clerks were appointed; thirty-five came from Oxford and Cambridge, one from the University of London, one from a Scotch University, two from Irish Universities, and *not a single one from all the nine provincial Universities of England and Wales combined!*

Of course, we should all be sorry if Oxford and Cambridge men were not largely employed in the service of the State, but it is absurd to suppose that all the provincial Universities together are incapable, in two years, of producing a single person worthy to serve the State in these responsible positions.

A Committee is dealing with this strange state of affairs; it may be that they will recommend drastic changes, but considerable driving power will be necessary to secure that the present unfair system is altered; for the vested interests concerned are numerous, powerful and tenacious.

May 4, 1917.

Types: Radicals and Tories

There is not perhaps in the whole world from San Francisco to Tokio quite such an unutterable ass as Mr. Suttonly Such, M.P. Some people might think that Colonel Hangitall, M.P., excels; but, though we should suffer both gladly, the latter is wanting in certain qualities which bring the former under the zoological definition—for example, he does not possess the characteristic *voice*. That is, it is the former who more nearly resembles the perfervid Scotchman of whom it was said "that he is never himself unless he gets upon his banks and brays." The Ultra-Tory is more passive. When he is not "shootin' and fishin'," all that one sees of him is the top of a bald head behind a paper at a club; and he generally does his pishing and pshawing to himself, except perhaps when he is taking an *apéritif* with congenial spirits just before lunch or dinner. He is indeed usually a quiet person—though not for any reason except that he is quite above arguing; and when he perceives a discussion imminent he retires quickly behind his own superiority. But the Ultra-Radical is active—very active. He is a vigorous old man with a ragged beard under his chin—because he does not know how to shave properly; and he is generally accompanied by his wife or daughter—homely persons who accept his opinions with conviction—or are indeed sometimes responsible for them without the Radical knowing it. He is moderately well-to-do, having succeeded in the perhaps dubious enterprises of his particular shop. His opinions are written upon his face. His decisive mouth and his grey eyes bring conviction. Of him it can be said that he has put away doubt from him for ever; for indeed there is nothing more detestable in our life than doubt of any kind—it is almost like having dirty cuffs and collars. In fact both he and the Ultra-Tory are Men of Principle as we defined it a little while ago; and their opinions were firmly established shortly after they left school. The Radical is of course a member of his local caucus,

which frequently succeeds in inflicting him upon Parliament. There he becomes a power in the land because he knows all about everything without question. He is ready to meet every argument in an instant. Where he does not hurl principles, he throws statistics; but his self-confidence generally conquers without a blow.

The Tory is just now in the ascendant because he can always say, "I told you so" regarding the German attack. But the Radical still carries off his amazing mistakes with the same old confidence. We have had the pleasure of listening to many explanations. The reason why the Radicals did not attend to the advice of Lord Roberts was that it was given in too haughty and military a tone; the country does not like the "confound-it-sir" attitude so familiar in soldiers. After all, it was fortunate that the country did not take Lord Roberts's advice, because, had it done so, the Germans would have attacked us immediately. How were we so blind as not to see the menace of the Germany Navy? Pish! The Germans were not building their navy to attack Britain, but to attack Brazil, and it was only Lord Roberts's folly that diverted their attention to this country. By whose folly was it that the Sinn Feiners rebelled? Why, the folly of the Tories, of course. Why did the Turks and Bulgarians come in against us? Because, of course, they had been goaded to desperation by the Harmsworth Press. But the last argument we have heard was the most amazing, namely that our armies have done magnificently because not a single one of them (except the one in Mesopotamia) has been wholly captured by the enemy, as happened to the French at Sedan! The way that the British have managed the war from the beginning has been fine. Prepared for everything, we struck at the right moment. Our so-called oligarchy reduced the army simply in order to show the world how splendidly our young untrained men would come forward in the hour of need. As for Zeppelins, let them do their worst!

Undoubtedly Mr. Suttonly Such is a strong man. He is a teetotaller, and generally an anti-vivisectionist, an anti-vaccinationist, a pacifist, a non-compulsionist, a free trader, and a free talker. Indeed the word "free" has for him the fascination that a candle has for the moth—it is the point which attracts all his convictions. Other words which he enjoys are "right" and "rights." He thinks that the

world is made to be free and that the only things which men have to consider are their rights. He has apparently never heard the saying attributed to Mazzini, that we have no rights but only duties ! Yet this is the case ; and the Radical makes the mistake of omitting the word *duty* from his vocabulary altogether. On the other hand, our friend Colonel Hangitall quite agrees that we have duties, but thinks that all our duties consist in keeping him in comfort. Mr. Suttonly Such (who, by the way, is the son of the eminent Theophrastus) really has much the same ideas, for he reads " right " to mean what is to benefit him. Between the two of them poor old England has had a big war foisted upon her, and has not found that it is being conducted quite satisfactorily up to the present.

Let it be clearly understood that we do not attach the zoological analogy to Mr. Such simply because his opinions may be wrong. This often happens, even to men of science. The mere wrongness of opinion does not give the peculiar qualities found in Sancho Panza's humble servant. To put the matter mathematically, we may say that the *coefficient of sweet reasonableness* (to use Matthew Arnold's adjective) is the ratio of knowledge to conviction, and that this ought to be large in every one, though perhaps not too large. The reciprocal of this may be called the *coefficient of asininity*, which is the ratio of conviction to knowledge ; and this is generally infinite in both the types referred to. The only difference is that Colonel Hangitall's coefficient is tacit, while that of Mr. Suttonly Such is blatant. The negative quantity annuls the positive one, and the result is that the consequent legislation is generally of zero value for the public.

The Song of the Ion

In a never-ending pilgrimage
  From earth's airy rim to her centre,
I pass and repass from age to age—
  No door so narrow I cannot enter.
The midmost fires around me glow,
  A maelstrom of molten ores ;
Unharmed and unhurt I come and go
  Through their imperceptible pores ;
And I rise and I rise, till under the skies
  That are dark with the Arctic snows,
In the crypt of a cromlech of towering ice
  Æon-long in a trance I repose.

I rove and I range o'er the moon-ridden deep,
That, as it tugs at its chain,
Hunches up like a wild beast its back in a heap;
Or immured in the granite's grain
I laugh in the lightning's face, ere I leap
Down the dangling ropes of the rain;
Through the whirling tide of the torrent I glide
To enrich the distant plain.

I live in the heart of the budding plant;
I join in the hymn of praise
That its every leaf and blossom chaunt
To the author of its days,
The Sun-god; I share in its passionate prayer
For consummate beauty and grace.

I lie on the lips of the lisping babe,
I nest in the breast of the sage,
And the thoughts that sweep through a conqueror's brain
And a kingdom's doom presage,
With their tingling message my being thrill,
Ere the world of its fate is aware.
And when life has fled, in his body dead
I lie quiet, and lying there
The stirrings and strife of a new-born life
Awakening around me I hear.

I steal and I slip through Death's ogre-like grip
That crunches all else to dust;
And I burn like a star to my brethren afar,
With a sheen that can never rust;
Though a grain of sand were a Universe
To my infinitesimal realm,
If my orbit should swerve but a hair's-breadth's curve,
Every planet must alter its helm.

Mountains may crumble and continents shrink,
And Ocean's bed run dry,
Planets be shattered, the sun go out,
But I, I can never die.
At one with Matter, that is flesh of my flesh,
With Energy, soul of my soul,
I unite with all, or slip through their mesh,
In my quest from pole to pole.

CLOUDESLEY BRERETON.

The British Science Guild

The Eleventh Annual Meeting of the British Science Guild was held at the Mansion House on April 30, the Lord Mayor presiding over a very large and distinguished audience.

Lord Sydenham, the new President of the Guild, delivered an address on "National Reconstruction," in which he referred to the lack of appreciation of the importance of science and scientific research which lay at the root of so many of

our present difficulties; and urged the application of science in its broadest sense to our national affairs in the future.

Mr. Fisher, Minister of Education, speaking on science in education and industry, said that while the practical teaching of science in our schools was quite as efficiently carried out as in France and Germany, we had not yet found a form of scientific instruction which would appeal to the general mass of children who were not destined to a scientific career. The application of science to industry was an urgent necessity, and a very satisfactory feature of the present situation was that there existed the Imperial Trust for Scientific and Industrial Research, a Committee armed with a large and liberal fund, and formed for the purpose of co-operating with industries and with associations of industries for the development of industrial research.

Mr. G. H. Wells, while condemning compulsory Greek and Latin for the sake of the language alone, said that the beauty, the wisdom, and the wonder of the Greek literature could not be denied; but the classical scholar should not be isolated from the scientist, as at present. There should be two distinct educational courses, the one leading up to the fullest and completest knowledge of Greek and Latin, and the other leading up to scientific studies; but between the two there should be a connecting link, that of history and philosophy in English.

Verbatim reports of the speeches have been published in the June number of the *Journal of the British Science Guild.*

A Science Guild for the Union of South Africa

We have received an eloquent article from a distinguished scientific Civil Servant in South Africa advocating the formation of a Science Guild for that Dominion, and quite agree with the writer's suggestion. The author begins:

"Many definitions of the meaning of Science can be given. The one that suits me to use at the moment is that Science seeks to discover the relation between cause and effect. In the inorganic world the scientist is already certain that like causes produce like effects, although the relation in any particular case may not yet be fully explained or even in its real essence may be undiscoverable. In the organic world the scientist, if not so positive, has a faith that like causes produce like effects. If he fails in his proof in individual cases, he appeals to the mass effect by means of statistics, for accidental deaths, births, suicides, etc., in the gross seem to follow certain if still obscure laws. The test of scientific knowledge is, in the end, its power of prediction. The scientist feels rather than asserts that if his mind could grasp all the causes he would be able to predict all the effects. The victories of Science in this regard are already striking and numerous, and the present rate of scientific progress is not only great but is accelerating rapidly.

"Many men of scientific eminence saw in the growth of Germany, both in the industrial and military senses, causes at work which must in their due time produce their effects. But they were talking to a generation which would not listen. The uneducated man or the man trained exclusively in classical learning has no real conception of causality—the world *he* lives in is rather one in which chance rules, or perhaps one in which instinct is valued more highly than reason.

"But the scientist is not blameless. Although some scientifically trained men raised their voices, the multitude of them acquiesced in a policy of *laissez-faire*, and joined the rabble in the scramble for loaves and fishes. Although they knew that Mammon was only an idol, they did not refuse to bend the knee with the crowd. For this state of affairs the watertight or even sectarian methods of scientists are greatly to blame. They banded themselves into specialised societies, each limited to the problems of some one branch of knowledge; or when a society took cognisance of all branches, it became either too exclusive—a scientific hierarchy or close corporation—or it became commonplace.

"The result was that as a body, especially within our Empire, scientists were

disunited and almost without any influence with the body politic. These remarks do not apply to German or rather Prussian scientists, who were attracted to and favoured by their Government, and who, if moderately competent, had no fear as to their livelihood. It is superficial criticism to remark on this that, if Prussian scientists have united with their Government and the Great War is partly owing to that co-operation, then disunion is preferable; for we have to face the fact that such a union, however maleficent, does make for strength—and economy."

The kind of Science Guild recommended by the author, however, has a very much more important object than has been contemplated elsewhere. In fact, the scheme includes an entirely new form of government by which a small House of Assembly would be elected as at present, and there would be a new kind of Senate of which the Members are elected by the various Guilds, or professional or industrial corporations, which make up the vital activities of the country; and many of these Members would be elected by the author's proposed Science Guild. The suggestion appears very worthy of consideration, as it would imply that a "House of Lords" would consist of men who are acquainted with the whole round of affairs, and not of heredity peers as in Britain, or decayed politicians as in other countries.

The Empire's Assets and how to use them

Mr. Alfred Bigland, M.P., read a paper on this subject before the Colonial Section of the Royal Society of Arts on February 27th in order to elucidate his views as to the best method of paying off the national war debt after the declaration of peace. He discarded the idea of increased direct taxation as a useless measure and one which would impoverish the country. In its place he wished to substitute schemes for industrial development on the lines laid down in the Manifesto of the Imperial Resources Development Committee. The central idea of these schemes was that individual enterprise should give place to state enterprise, thereby ensuring the necessary capital to create new industry on a large scale; and he showed that, by these means the state would in ten years recoup its losses during the war, and at the same time increase the prosperity of the country. The following are some of the sources of new wealth which were in his opinion at the command of the British Empire.

(1) Mr. Moreton's Frewen's scheme of an Empire Farm which proposes to develop millions of acres of "new wheat lands, favourably situated as to climate and relatively accessible from Hudson Bay" to be sold at considerably enhanced values after such development. By this means "the Empire would be made more nearly self-supporting in respect of wheat, and the manufactures of the home country would find a new market." To make this successful, import duties on wheat from countries outside the Empire would become necessary. (2) Further development of the British possessions in West Africa, especially in regard to palm-oil (an industry which was rapidly drifting entirely into German hands before the war) and cocoa. (3) Considerable extensions of the fisheries, those round Canada in particular.

He pointed out that the cause of labour would be furthered by such operations, as the State would be able to meet the needs of the individual worker better than a single employer or company can, and that steps would be taken to see that this part of the organisation was properly carried out. During the discussion that followed the lecture it was suggested that a development of the Indian railway system, now very inadequate, would raise the prosperity of that country, and, in consequence, its power of purchasing from Great Britain.

The Shakespeare Association

On May 3, 1917, a meeting was called at King's College, London, to promote the Institution of an annual "Shakespeare Day" in the schools The chair was taken by the Vice Chancellor of the University, Lt -Col Sir A Pearce Gould, K.C V.O., and His Excellency Dr W H Page, the United States Ambassador, was present The idea of the setting apart of one day in which to do homage to our greatest poet is to awaken in the school children an appreciation of his works, which is at present insufficiently developed—this appreciation to be the precursor of the institution of municipal theatres and finally the building of a National Theatre It was thought best that each school should be left free to celebrate the day after its own manner, although the acting by the school children of one of Shakespeare's plays was brought forward as possibly the best suggestion Another idea was that those being trained in trades, such as carpentry, drawing, and so forth, should be encouraged to make the scenery necessary for these productions It was hoped, however, that, at all costs, this annual celebration would not be allowed to degenerate into a dead ceremony but would be instinct with interest for the children and really answer its purpose of cultivating a love of Shakespeare Not the least important part of the meeting was the presentation to Dr Page, by Prof. Gollancz, of a rare 1632 folio of Shakespeare Dr Page, in his warm-hearted and dignified speech of thanks, expressed the hope that the United States would also adopt the "Shakespeare Day" in all its own schools

Pamphlets and Periodicals

The Philosophical Institute of Canterbury (New Zealand) has issued its Report for 1916 We note that "The Council has recognised the importance of furthering the national movement to advance scientific research and extend the application of scientific knowledge" A grant has already been made for this purpose.

The Memorandum on the Organisation of Scientific Research Institutions in the United States of America, by Gerald Lightfoot, M A , F S S , December 1916, printed and published for the Government of Australia by Albert J Mullett, gives an interesting and comprehensive report of what the United States is doing in the cause of scientific research. Much good work seems to have been done by the Universities, which are state-endowed This work is classified as follows · "(1) Pure scientific research, including that done (*a*) by members of the professorial staffs, and (*b*) by students to obtain higher degrees, and (2) research having an industrial objective, including (*a*) work carried out by the professors on their own initiative, (*b*) work carried out by the professors and paid for by industrial enterprises, and (*c*) work done (ordinarily by special research staffs) at the Agricultural and Engineering Experiment Stations." According to the experience of the Rockefeller Institute, New York, in problems of organisation "the best teachers are, undoubtedly, those who do research work, but it does not by any means follow that the best research men are those who also do instructional work. The question is largely one of temperament."

The Report on Tuberculosis, issued under the authority of the Minister for Trade and Customs of Australia, and printed and published for the Government of the Commonwealth of Australia by Albert J. Mullett, concludes that more co-ordinated investigation on the undetermined question regarding tuberculosis is required, and that fuller use of methods already determined should be adopted. The Report deserves study in this country.

The editor of *Scientia*, through the medium of **Nature** (January 25, 1917), informs this country that the "peaceful penetration" accomplished by Germany in pre-war days for her commerce has also been put in practice for her science. By flooding the market with scientific publications of her own productions, she has tried to create for herself a monopoly which reacts deleteriously on science itself—which should obviously acknowledge no nationality. The wrong motive in this desire of quantity versus quality has already borne fruit in the marked deterioration in the scientific value of these German publications. "To take from Germany its scientific hegemony, one of the most suitable, efficacious, and prompt means is, it seems, the creation in each of the principal branches of science of 'Archives,' 'Year-books,' and 'Journals' in general, which are international in so far as collaboration and content are concerned, but which are edited and published in the countries of the Entente. . . . The projected Entente publications must, then, in the first place, print less and select better. Then they must direct their attention to both synthesis and analysis. . . . They must take account even of those researches which come from isolated thinkers. . . . Lastly, they must put the various writings into their correct plane by publishing at length the most important ones, giving long summaries of those which are less important, and merely announcing the results of researches which are not too restricted or evidently unfruitful."

The Australian Manufacturer (December 23, 1916) comments on Britain's waste of coal in her industries, and hints that she is not justified in exporting about sixty-three millions of so precious a mineral in view of the fact that it may be exhausted in a few centuries. A suggestion is made for the substitution of electrical power, the gain from which would justify the expenditure of the initial outlay.

As phrases such as "The Freedom of the Seas" have a knack of being repeated by the general public, while they are really understood only by the few, an article bearing this title by Ramsay Muir, in **Scientia** (April 1917), is of value, for it explains the true significance of the phrase, traces its history, differentiates between the freedom of the seas in war and peace, and suggests that "nations will be ill-advised if they allow all their attention to be concentrated upon the comparatively minor question of the freedom of the seas in time of war and forget the vastly more important issue of the freedom of the seas in time of peace." "There are only two ways," he says, "in which the freedom of the seas can be permanently secured for the whole world. One is that it shall be under the guardianship of a world-government, or of a common authority respected and accepted by all States; and we are very far from that. The only other way is that naval supremacy shall be in the hands of a power which, in the first place, cannot hope for supremacy on land, and, in the second place, is secure against the danger of destruction by any land power however strong."

Mr. Dugald Clark, D.Sc., F.R.S., M.Inst.C.E., in his address, entitled **The Stability of Great Britain**, delivered at the opening meeting of the 163rd Session of the Royal Society of Arts, London, on November 15, 1916, says "Commerce is not a war. Some . . . imagine a prosperous Great Britain in a poverty-stricken world. . . . It will be found, on the most careless examination, that wealth increases simultaneously in industrial nations. . . . We must not even forget that a poverty-stricken Germany and Austria would react upon the whole world. Punish the Germans and Austrians by all means—they thoroughly deserve it—but do not imagine that by cutting those nations out of the world's commerce the other nations can be rendered more wealthy. . . . All their (Germany and

Austria's) powers of production will be required to restore the material damage they have wickedly done." He places himself also in opposition to the ideal of Australia and Canada of a self-contained Commonwealth, which Colonial ideal "arises from a false notion of what trade is." He closes with these words: "Britain's stability in the future, as in the past, will flow from continued honesty and fair play, and her material success from consideration of the interests of the whole world as well as her own."

The Man of Science in the Community of To-day is the subject of the Presidential Address of Prof. D. Fraser Harris, M.D., C.M., D.Sc., F.R.S.E., F.R.S.C., to the Nova Scotian Institute of Science, delivered November 13, 1916. It is the plea for the recognition of science by State and people with which every one is now familiar. The following excerpt embodies, however, a somewhat new suggestion: "Science, in short, must have a Department, a Government Office, before the public will fully accord it its place of honour. We may regret that this sort of thing has to be, but our regret will not change public opinion; and it appears to be part of the British Constitution that nothing can be done or should be done without a very large body of public opinion behind it. But the official recognition of science cannot wait until the public has seen fit to render science the homage it deserves. To begin at the top, let there be a Minister of Science and a Ministry of Science with just as much prestige accorded it as the War Office, the Foreign Office, or the Home Office. The duties of the Minister of Science would be primarily to foster science in every way possible, to foster its interests, to administer its affairs somewhat after the manner in which the Board of Trade looks after trade, the Department of Agriculture and Fisheries, agriculture and fisheries."

Collectors of coins will find special interest in Vol. II, Part I, of the **Transactions of the Yorkshire Numismatic Society**, which contains a well-illustrated article by its editor, Thomas Sheppard, M.Sc., F.G.S., on "Medals, Tokens, etc., issued in Connection with William Wilberforce and the Abolition of Slavery," and "The Circulated Copper and Bronze Pence of Queen Victoria, 1837–1901," by J. F. Musham, F.E.S. **The East Riding Antiquarian Society Transactions**, Vol. XXI, of which Mr. Sheppard is the Secretary, publishes articles on the restoration work done in St. Mary's Parish Church, Scarborough, and a short account of that church, which will form pleasant reading for most people as well as for antiquarians.

This and That (The Editor)

No event of great importance as regards the **Affairs of Science** has occurred during the quarter. Science in education continues to preoccupy many, and good speeches on the subject were delivered at the annual meeting of the British Science Guild (page 118) by Lord Sydenham, the new President, by Mr. H. A. L. Fisher, President of the Board of Education, and by Mr. H. G. Wells. Prof. D. Fraser Harris, M.D., gave an excellent address, partly on the subject, last November in his inaugural address as President of the Dalhousie University, Nova Scotia (copy recently to hand); and Sir Alfred Bourne, F.R.S., gave a similar one before the Indian Science Congress in January (*Nature*, March 22). The Cambridge University Press has recently published a book of essays, called *Science and the Nation*, all written by distinguished men of science and edited by the Master of Downing (Mr. A. C. Seward, F.R.S.), with an introduction by Lord Moulton; and this work was ably reviewed by Mr. Wells in *Nature* (April 19).

I quite agree with Mr Wells that little practical comes out of such essays. We all know that science is great; but the man of science—that is another matter. The neglect of science is due to the man of science himself, first because he does not know what he wants, secondly because he has no backbone, and thirdly because he is too jealous to follow leaders—witness the doctors and the Insurance Bill. The main function of the man of science is, not to teach or write text-books, but to make discovery. There is only one way to encourage science—to encourage discovery. There is only one way to encourage discovery—to pay for it. As a rule (in spite of claptrap to the contrary) smart men will not undertake the hazard of discovery-hunting unless they know that they will gain something by it if they succeed. At present the man of science loses even when successful—except in a very few lines. If he succeeds he gives the world a great gift, but in return he receives—say a professorship worth £600 a year, with the privilege of subscribing to a small pension due at the age of sixty or sixty five. The man of science therefore remains a helot all his life—and deserves to. The few academical prizes available are usually obtained by men of quite another type.

The national attitude in this matter was well illustrated at a meeting held at the Royal Society of Arts on March 27, apparently to stimulate the enlistment of doctors into the **Indian Medical Service**—Mr Austen Chamberlain, Secretary of State for India, presiding. It was suggested that doctors should go to India because of the unparalleled opportunities for medical research there. I can speak from experience as to this, because I entered the Indian Medical Service myself in 1881, and carried out investigations on malaria from 1890–99, mostly in my leisure and at my own cost. The subject was of some minor importance, because malaria has been officially estimated to kill at least 1,300,000 persons annually in India alone, and my researches have been supposed to be of value, because I obtained a Nobel Prize for them in 1902. Yet I have never received for them the smallest thanks, promotion, or reward from the Government of India.[1] Some years ago, in order to bring matters to a point, I asked it for a small increase of pension which is sometimes given for good service—it was refused. Since I left India, I have been ten times to the tropics on professional malaria work for the British Empire, but was paid only on three of these occasions, and then less than a surgeon or a barrister often charges for a single case; yet when I asked the Colonial Office, in consequence of a resolution of the British Science Guild, to pay me a small fee for expert advice on two committees, it refused me (see SCIENCE PROGRESS, July 1916); and when I petitioned Parliament for payment, the Chancellor of the Exchequer refused to deal with the petition, though it was perfectly sound and legal (see SCIENCE PROGRESS, October 1915). Fortunately for me such childish ingratitude is of little consequence to myself; but it is of consequence for science, for the suffering sick, and for the Empire which is incompetent enough to allow it. I fear that India will long have to go begging both for its doctors and for a knowledge of the best way to prevent or cure its diseases. And the cause of this is a perfectly natural one and obvious to all.

To see the matter in its true light we must remember that the Indian and the Home Governments already spend large sums annually in the form of pensions and additional pay to judges, civilians, and various types of politicians whose services to the State are not one-thousandth part of the value even of a minor scientific discovery—let us say in medicine. For example, the recently discovered method of treating dysentery by emetine is a far greater boon to the people of

[1] Nor have I ever been consulted by it regarding the application of my researches to the saving of life.

India, natives and Europeans, than anything ever done by the whole legion of viceroys, judges, members of council, commissioners, and so on. Yet these last people are bounteously fed by the State, while the true benefactor may, for all the State cares, go to the workhouse. The reason is that State-affairs are managed, not by men who have really done things, but by grammar-fed clerks and "examination-wallahs," who, in consequence of their defective education, are not capable of understanding the difference between great work and little work. I have never disguised my own opinion, based upon exceptional opportunities for judging, that British administration is frequently or generally bad. Its badness is due fundamentally to the fact that it commonly fills the highest offices with men who have never done anything, while it excludes the men who have. We have only a little time left for a root-and-branch reform in the matter.

Much has been written during the quarter upon this all-important subject of **Reconstruction** after the war—let us call it revolution. Lord Sydenham's address to the British Science Guild (*Nature*, May 3) is, I think, the best contribution to the discussion which we have yet had. But I would add this to it, that we must as a nation find a means for stimulating the highest work possible—that is, work in science, art, invention, exploration, thought, etc., which is generally profitable to the world but not to the workers. There is only one way to do this, and that is to assure the prospective worker that if he succeeds he and his family will receive adequate recompense from his countrymen for the time which he might have spent in a manner more fruitful for himself. I would suggest, in short, a national pension scheme for great work in lieu of the existing Civil List Pensions and the present wastage of funds in the form of subsidies for projects which never come to anything. But of this I have no space to write more at the moment.

The **Representation of the People Bill** now going through Parliament will probably amount to a revolution, and a welcome one. I can only say (on going to press) that I for one sincerely trust that the clauses for Proportional Representation will not be lost, because in fact they constitute the whole gist of the measure, and will enable men of deeds of all types largely to replace the caucus-politicians who have so misruled us in the past. By the present system of election to Parliament almost every man of capacity is excluded from that assembly unless he becomes, what few men of capacity can do, a partisan.

It is stated that Lord Rhondda, the President of the Local Government Board, proposes to construct a **Ministry of Health**—which has been advocated for half a century by sanitarians. The war will indeed have stimulated John Bull if it succeeds in making him pay attention to his back premises. We hope that the Bill will include much more stringent measures against sanitarily defective municipalities, and that means will be found for forcing India and the Colonies along similar lines. In my opinion disciplinary measures should be used to deal with municipalities the death-rates of which exceed a given figure, and which at present are allowed to lose their people with impunity.

The Royal Society has recently had under consideration the question of enlarging the statute under which it sometimes elects as Fellows men of eminence in affairs, such as Privy Councillors, who have assisted the general cause of science. If the Society is to be looked upon merely as a private scientific club it may, of course, do as it pleases in such matters; but if we are to consider it as the national academy of science, such points become of prime importance. Personally I do not think it is proper for any society to make elections with any ulterior motives of the kind mentioned. A suspicion of seeking for subterranean influence is not ennobling to an individual—and certainly not to a great society. Many

Fellows of the Royal Society would, I believe, be entirely in favour of the complete deletion of the statute which allows the election of any persons except on the grounds of distinguished scientific work.

Our *learned societies* in general should be reformed in several particulars—some of which have been suggested in back numbers of SCIENCE PROGRESS. Few of them really do much for science except provide media for publication for papers, with an occasional, but generally very scanty, discussion. The main fault appears to lie in the method of election of councils, which is not democratic enough. The method of previous nominations by councils generally has the practical effect of enabling the sitting council and the officers to select whoever they please to select, thus tending to make the societies into oligarchies which choose their own friends and often exclude the most vigorous and capable Fellows who criticise their doings.

Mr. William Heinemann issues an excellent little book with an introduction by Sir E. Ray Lankester called **Science and Education**, consisting of a number of lectures delivered years ago at the Royal Institution by Faraday, Tindall, Paget, and others. The lectures are still much to the point, but, in fact, few of the reforms advocated by the lecturers have yet been effected Sir Ray Lankester's Introduction is also much to the point.

Another book, **One Hundred Points in Food Economy** (G. Bell & Sons), by Mr. J. Grant Ramsay, F.R.E.S., Principal of the Institute of Hygiene, may be recommended in these times of voluntary rationing. Each of the hundred points is very well put, and may be remembered by housekeepers, though I am not sure that all housekeepers will agree with every point, at least if they are ladies. But the author is an expert on the subject.

Sir Arthur Newsholme, M.D., F.R.C.P., has recently written a fourth series of Reports (Local Government Board, Cd. 8496) on the very important subject of **Child Mortality in England and Wales.** It summarises the experience gained during the last four years, in which 575,078 deaths occurred under the age of five, or more than a quarter of all the deaths at all ages. Striking contrasts in the mortality have been found. Thus Shoreditch had 241 deaths per thousand births against 112 in Hampstead, while Burnley, Wigan, and Middlesbrough had over 250 deaths per thousand births, as against 96 in Ilford. Sir Arthur Newsholme's final chapter reviews the steps that should be taken for the diminution of this mortality.

Much concern has been manifested by men of science in the fact that the **Kew Bulletin** was suspended in consequence of its consumption of paper; and Sir William Beale and Sir William Cowan have asked questions on the matter in the House of Commons. The reply was that the suspension of the *Bulletin* was acquiesced in by the Director of the Royal Botanic Gardens. The matter lies within the office of the Controller of His Majesty's Stationery Office

ESSAYS

THE THEORY OF NUMBERS (L. J. Mordell, Birkbeck College, London)

THE Theory of Numbers is a subject which has aroused an extraordinary enthusiasm among many of the world's greatest mathematicians. Fermat, Euler, Lagrange, Legendre, Gauss, Cauchy, Jacobi, Eisenstein, Dirichlet, Kummer, Kronecker, H. J. S. Smith, Minkowski, Sylvester, Hermite, and Poincaré among others have been fascinated by this branch of mathematics. There seems to be some reason, therefore, for the statement which we have heard attributed to Prof. Klein, that "One cannot be a great mathematician unless he is acquainted with the Theory of Numbers."

English mathematicians, however, have shown but little interest in the Theory of Numbers, and excluding Prof. Smith, who was undoubtedly an arithmetician of the first rank, and Sylvester, they[1] have taken practically no part in its great progress during the last century.

This is due to a variety of causes. Foremost, perhaps, as it is one of the most abstruse of studies and as its conclusions do not have any practical importance, it does not tend to appeal to the practical genius of the English mathematician. This tendency is accentuated by the English educational system, which seems to regard the examination as its final aim · so that naturally the importance of a subject, and the attention paid to it, depend upon its utility from an examination point of view. One result of this is, that while the Englishman with any pretence to a mathematical training is thoroughly acquainted with numerous properties of conics, very frequently he is entirely ignorant even of the existence of some of the most interesting and wonderful developments of pure mathematics. Another result, let us hope it is a feature of the past, was that the interest and energy of many mathematicians were directed to research which was closely related to questions of the examination type; or, as the late Prof. H. J. Smith put it, to finding new expressions for the foci of a conic. This also accounts to some extent for the production in England of so many elementary mathematical text-books which, no doubt, are useful for the purpose for which they were written, while really advanced mathematical books are still so few in number that the very advanced student has to rely to a considerable extent upon works published abroad.

A further reason for the neglect of the Higher Arithmetic in England is to be found in the Fellowship system of the ancient English universities. Under this system the young mathematician is often expected to produce important research within a few years of taking his degree. This is certainly no incentive to the pursuit of a study in which research is difficult, and in which there may be little to show in print as a result of many years' work. It can scarcely be expected that the young mathematician will jeopardise his chances of substantial recognition at

[1] This does not apply to the Theory of Partitions, which, however, is really a branch of Combinatory Analysis. Cf. the introduction to Major MacMahon's *Combinatory Analysis*, vol. i.

a period of his life when it is most useful, unless somehow he has become fascinated by the Higher Arithmetic, and is resolved that he shall not be deterred by reason of material considerations.

The difficulty of securing a suitable introduction to the subject is a further hindrance to its study. In the first place, there are no lectures upon it at most British universities. In the second place, while there is no lack of introductory text-books, a number of them either pay too much attention to details which cannot be appreciated by the beginner, or limit their contents to the barest elements of the subjects, giving not the slightest indication of its great extent or any mention of many of its most beautiful and interesting results, a large number of which could easily be mentioned in connection with many topics.

Many of these reasons may also explain statements made and questions frequently asked by English mathematicians. Thus Prof. Burnside in his presidential address to the London Mathematical Society in 1908, deals with the fact that the Theory of Groups attracts far more interest abroad than in England. In a similar address in 1912, Prof. Baker asks, "Why is it so often the case that the early history in England of a department of Pure Mathematics is a history of importation?"

Gauss, to whose supreme genius is due the modern aspect of the Higher Arithmetic, said, "Mathematics is the Queen of the Sciences, and the Theory of Numbers is the Queen of Mathematics." In the introduction to Eisenstein's *Mathematische Abhandlungen*, to quote H. J. S. Smith,[1] Gauss wrote: "The Higher Arithmetic presents us with an inexhaustible storehouse of interesting truths—of truths, too, which are not isolated, but stand in the closest relation to one another, and between which, with each successive advance of the science, we continually discover new and sometimes wholly unexpected points of contact. A great part of the theories of Arithmetic derive an additional charm from the peculiarity that we easily arrive by induction at important propositions, which have the stamp of simplicity upon them, but the demonstration of which lies so deep as not to be discovered until after many fruitless efforts; and even then it is obtained by some tedious and artificial process, while the simpler methods of proof long remain hidden from us."

The Theory of Numbers is unrivalled for the number and variety of its results and for the beauty and wealth of its demonstrations. What reader cannot but be impressed by such gems as, for example, Eisenstein's uniform demonstrations of the laws of quadratic, cubic and bi-quadratic reciprocity; Jacobi's arithmetic investigation, as simplified by Dirichlet, of the representations of a number as a sum of four squares; or Tchébicheff's proof of Bertrand's postulate that at least one prime number exists between the limits x and $2x - 2$ if x is greater than $7/2$?

The best illustrations that the proofs of many simple results involve most abstruse and complicated considerations are, perhaps, Eisenstein's results on the representations of a number as a sum of five squares; and Kummer's results on the general law of reciprocity and Fermat's Last Theorem, one of which may be stated as follows: If n is an odd prime which is not a divisor of the numerator of one of the first $\frac{1}{2}(n-3)$ Bernouilli's numbers, the equation $x^n + y^n + z^n = 0$ does not admit of rational values for x, y and z apart from trivial solutions for which one of the unknowns is zero; though 250 years ago Fermat asserted that he had discovered a truly wonderful proof for all integer values of n greater than 2, but

[1] *Collected Works*, vol. ii. p. 169. I have much pleasure in taking this opportunity of acknowledging my great indebtedness to Prof. Smith's writings.

that the margin of his book was too small to contain it.[1] The enunciation of Eisenstein's results occupied part of one page of Crelle's *Journal*, but their demonstration by Smith and Minkowski required a small volume This proof, as well as the proof of Kummer's results, are examples of some of the most delicate and intricate demonstrations to be found in the whole range of mathematical analysis.

As the integers are the fundamental elements of the Theory of Numbers, it is not surprising that questions are easily suggested and theorems discovered inductively, though it does seem strange that their demonstrations should so frequently be very difficult and indeed occasionally baffle all mathematical analysis. The law of quadratic reciprocity, which is one of the most important and beautiful theorems, was discovered by Euler nearly half a century before the first rigid demonstration was given by Gauss in complete ignorance of the work of his predecessors Kummer discovered the general law of reciprocity about ten years before he was able to give a proof of it. Stern found by induction that the value of x in the equation (p is a prime) $p = 8n + 3 = x^2 + 2y^2$ could be found from a simple congruence, though it remained for Eisenstein to demonstrate and generalise this result. Waring's conjecture in 1787 that every positive integer could be expressed as a sum of m positive n^{th} powers, where m is independent of the integer, was not proved until 1909 by Hilbert. Goldbach's Theorem, that every even number can be expressed as the sum of two primes, has neither been proved nor disproved. Similarly, while our readers will have no difficulty in finding a few solutions in integers of the indeterminate equation of $y^2 = x^3 + 17$, the general solution is still unknown; although Fermat stated that he had discovered an entirely new method—sane pulcherrima et subtilissima—which enabled him to solve such questions in integers.[2]

In 1890, at the meeting of the British Association, Dr. Glaisher said: "I am sure that no subject loses more than mathematics by any attempt to dissociate it from its history." This applies with great force to the Theory of Numbers, as can be seen from any history of mathematics, but more especially from the many volumes by Bachman and from the original memoirs. The Higher Arithmetic seems to include most of the romance of mathematics. No other study requires such steadfast devotion or gives more pleasure to its investigators. As Gauss wrote to Sophie Germain, "The enchanting beauties of this sublime study are revealed in their full charm only to those who have the courage to pursue it!" and we are reminded of the folk-tales, current amongst all peoples, of the Prince Charming who can assume his proper form as a handsome prince only because of the devotedness of the faithful heroine.

The original memoirs frequently suggest the toilsome labours of a mountaineering party attempting to ascend hitherto unexplored summits. We realise at once that the authors are grappling with extraordinary difficulties. Their paths abound with innumerable obstacles, and new methods must be devised to overcome them. Progress may be slow, and it may be many years before their efforts are crowned with success and an imperishable monument erected as a testimony to their genius

Who can fail to sympathise with Eisenstein when we read his account of his efforts to find the number of classes of ternary quadratics (during which he developed a tolerably complete account of the arithmetic properties of the ternary quadratic and very likely of the general quadratic), and to imagine his joy, when, after ten years of arduous labours, as he himself tell us, the goal was in sight? It

[1] Cajori's *History of Mathematics*, p. 179. For a full account see Ball's *Mathematical Recreations*, p. 40, or his *History of Mathematics*, p. 295.

[2] Ball, *Mathematical Recreations*, p. 43.

is sad to think that his devotion to his favourite study contributed to his early death at the age of twenty-nine, and was, perhaps, the greatest loss the Higher Arithmetic has ever sustained. Similarly we read that Kronecker in 1858 writes to Dirichlet that he requires a long time for the development of his ideas upon the wonderful and intimate relation connecting elliptic function and the Theory of Numbers, and it is only in 1885 that his results are perfected.

The humorous element is not lacking since the establishment in 1908 of the Wolfskell prize of about £5,000 for the first demonstration of Fermat's Last Theorem. The opportunity of gaining immortal renown (not to mention the £5,000) has attracted the attention of numerous aspirants, whose chief qualification, in many cases, seems to be their ignorance of elementary mathematics.

The Higher Arithmetic presents many pitfalls even for the experienced investigator, and, as might be expected, the amateurs miss few opportunities of making errors of every conceivable kind. We read with amusement of one author who has been convinced for some time that the application of elementary algebra can be greatly extended and supports his view by giving a proof of Fermat's Last Theorem, which he considers is quite original (no one can doubt this) and very fascinating. The author of another of the so called solutions quotes innumerable learned references; but he makes mistakes of such a kind that one is firmly convinced that his references were inserted unread and at random.

English mathematicians are apt to forget that many of the developments of modern mathematics had their origin in investigations arising from the Theory of Numbers. The Higher Arithmetic throws out its tentacles in all directions, and, in making a systematic study of the subject, the reader notices at once that all the resources of pure mathematics are being drawn upon. The characteristic properties of an invariant and a contravariant were introduced by Gauss in his arithmetical investigations on the binary and ternary quadratics; and it is not unlikely that he was led by other arithmetic investigations to his discovery of the elliptic functions twenty-five years before their discovery by Abel and Jacobi. The first covariant, the Hessian of the binary cubic, was discovered by Eisenstein in his researches on the arithmetic theory of the binary cubic. And it was in applying the elliptic functions to the Theory of Numbers that he discovered the Weierstrassian elliptic functions many years before their introduction by Weierstrass.

The modern developments of doubly periodic functions of the second and third kinds are due to Hermite's efforts in proving Kronecker's Class Relation Formulæ. These formulæ also proved a powerful stimulus to the Study of Groups, the Theory of Functions, Modular Functions, and Automorphic Functions. As a further illustration we may call attention to Poincaré's review of a large number of his published articles on Differential Equations, Theory of Functions, and the Theory of Numbers. He emphasises the fact that he did not pursue his researches on these three subjects separately, but that the results obtained in the different subjects threw light on each other; and that his work in each of them was greatly assisted by his work in the others.

The Theory of Dirichlet's series, which is attracting some attention at the present time, arose from Dirichlet's famous investigations many years ago, when he proved that every arithmetical progression, whose first term has no common factor with the common difference, contained an infinite number of primes, and also found the number of classes of binary quadratics with a given determinant.

Finally, the analytical Theory of Numbers, which is exercising a powerful attraction for many mathematicians now-a-days, is simply the study of functions

arising in the Theory of Numbers, and in particular from Riemann's celebrated researches on Prime Numbers.

The present age may see, or perhaps is seeing, a revival in Great Britain of interest in such parts of mathematics as have marked or are likely to mark a definite advance of the science. This would be greatly facilitated if lecturers were to lecture more frequently upon other than the ordinary examination subjects. The Universities might make it easier for qualified members of the public to attend such lectures and might, perhaps, confer upon them some sign of recognition after a definite period of work. It is no easy matter to decide upon what the recognition should depend, but written examinations in very advanced work are by no means a test of proficiency. Proficiency is really indicated by a general knowledge of the principles of an investigation and not necessarily by the ability to write out in three hours the details, which one is often only too pleased to take for granted when reading books or journals.

At present most of the advanced students in the Universities are those who have taken the usual undergraduate course. It would contribute greatly to the progress of the science in Britain and of the Universities if some attempt were made to attract those students who find their requirements satisfied only at continental Universities.

MR. J. H. GURNEY'S SOLUTION OF QUARTIC EQUATIONS

(A. S. Percival, M.A., M.B. Camb.)

WHEN the coefficients of the Quartic are numerical, Horner's tedious method is commonly used. For some little time I have been replacing the Quartic by two quadratic factors. This can be done in several ways, and I have been corresponding with Mr. Gurney about the simplest method of forming these factors. He finally has given this method, which I feel convinced is the speediest way of finding all the four roots of the equation.

Let the Quartic be of the form

$$x^4 + Ax^3 + Bx^2 + Cx + D = 0 \text{ or } F(x).$$

If $A \neq 0$, remove the second term, synthetically dividing by $-\frac{A}{4}$. In other words transform $F(x)$ into $f(y)$ by putting $x = y - \frac{A}{4}$; we then have

(1) $$f(y) = y^4 + Qy^2 + Ry + S = 0.$$

(2) Write down the auxiliary cubic $\phi(z^2)$, where

$$\phi(z^2) = z^6 + 2Qz^4 + (Q^2 - 4S)z^2 - R^2 = 0.$$

This cubic must have one positive root at least. Call it a^2.

Then $$\phi(z^2) \equiv (z^2 - a^2)(z^4 + pz^2 + q).$$

(3) It will then be found that

$$f(y) \equiv \left\{y^2 + ay + \frac{1}{4}(a^2 + p - 2\sqrt{q})\right\}\left\{y^2 - ay + \frac{1}{4}(a^2 + p + 2\sqrt{q})\right\}$$

and hence the four roots of the Quartic are found by solving these two quadratics.

It should be noted that the positive values of $\sqrt{a^2}$ and $\sqrt{q}$ must be taken Further that

(1) If the cubic has 3 real positive roots, the quartic has 4 real roots.

(2) If the cubic has 1 real positive root and 2 unreal roots, the quartic has 2 real and 2 unreal roots.

(3) If the cubic has 1 real positive root and 2 real negative roots,
the quartic has either 2 real and 2 unreal roots, or 4 unreal roots.

If a^2 be incommensurable, a result can be obtained which will be accurate to any number of decimal places required. In the second example, $a^2 = 2$, so a is a surd.

Ex. (1) $$x^4 - 25x^2 + 60x - 36 = 0 = f(x).$$

As the term involving x^3 is absent, we can immediately write down the auxiliary cubic $\phi(z^2)$.

$$z^6 - 50z^4 + 769z^2 - 3600 = 0.$$
$$\text{or } (z^2 - 5^2)(z^4 - 25z^2 + 144).$$

The cubic has here 3 real positive roots, the quartic must by (1) have 4 real roots.

$$f(x) \equiv \left\{x^2 + 5x + \frac{1}{4}(25 - 25 - 2\sqrt{144})\right\}\left\{x^2 - 5x + \frac{1}{4}(25 - 25 + 2\sqrt{144})\right\}$$
$$\therefore x = -2{\cdot}5 \pm \sqrt{6{\cdot}25 + 6} \text{ or } 2{\cdot}5 \pm \sqrt{6{\cdot}25 - 6}.$$
$$\therefore x_1 = 1\,;\ x_2 = -6\,;\ x_3 = 3\,;\ x_4 = 2.$$

Ex. (2) $$x^4 + 4x - 1 = 0 = f(x)$$

Auxiliary cubic $\phi(z^2) = z^6 + 4z^2 - 16 = 0.$

$$\phi(z^2) \equiv (z^2 - 2)(z^4 + 2z^2 + 8).$$
$$f(x) \equiv (x^2 + \sqrt{2}\,x + 1 - \sqrt{2})(x^2 - \sqrt{2}\,x + 1 + \sqrt{2}).$$
$$\therefore x = -\frac{1}{2}\sqrt{2} \pm \sqrt{\sqrt{2} - \frac{1}{2}} \text{ or } \frac{1}{2}\sqrt{2} \pm \sqrt{-\sqrt{2} - \frac{1}{2}}.$$

The 2 real roots are consequently ·249038376 . . . and $-$ 1·663251938 . . .
and the 2 unreal roots are ·707106781 . . . $\pm \sqrt{-1{\cdot}914213562\ldots}$

SOME OF THE EVOLUTIONARY CONSEQUENCES OF WAR

(Ronald Campbell Macfie, M.A., M.B., C.M., LL.D.)

THOUGH war has been in the world since the time of the trilobites, and though its importance in the evolution of animal types has long been a cardinal article in the creed of biologists; yet the sociological and biological significance of human warfare with reference to the evolution of man's body and mind has never been quite adequately studied, either from the standpoint of sociology or of biology.

In view of the universality of war, there naturally arises the question: What physical and spiritual types of Man does war select, and what types does it eliminate? No question, indeed, would seem more to invite and more to merit thorough investigation. But the problem is not only difficult: it is readily obscured by associated sentiments and passions, and, so far, it has been more often exploited by military or pacific partisans than elucidated by patient and impartial investigators. Even the wary spirit of scientific inquiry seems readily corrupted by the emotional psychology of war, and even in scientific papers we find doubtful data leading to undoubting generalisations, and strong prejudices drawing, from very weak premises, very wild conclusions.

Thus, we find competent biologists, such as the eminent pacifist Prof. David Starr Jordan, stating, with reference to the dysgenics of war, that war caused degeneracy in the Romans, and that the Napoleonic wars lopped inches off the stature of the Frenchmen.

Yet both statements, though widely current, have never been proved, and are probably erroneous.

With regard to the Romans. Dr. Otto Seeck, who has made a careful investigation of the subject, comes to the conclusion that the decline of Rome was largely due to political murders and to voluntary enlistment; and such a view is quite as plausible as the view Prof. Jordan prefers. But it might be equally well contended that the fall of Rome was due to malaria, or too much eating, or too many hot baths. We do not know, and we can speculate as much as we please. But we must not use speculations as corner-stones in any scientific theory of the consequences of war.

With regard to the lopping of the French. It is by no means certain that the modern Frenchman is shorter than the Frenchman of Napoleon's time, and even granting that he is shorter, it would be very difficult to prove that the shortening is the result of war. Further, even if we could prove both propositions, we should still be unwarranted in formulating a general law; for the Teutons, who have possibly suffered more from war than any other race in Europe, are a tall race; and the Montenegrins, who have been decimated by war for centuries, are much above ordinary stature; rendering it difficult to believe that they have been pruned by battle.

In old-time wars, when men fought hand to hand with their foes, it is quite possible that the tall strong men, with weight and reach, killed the small weak men—though, even then, brains were of some survival value, and the "race was not always to the swift and the battle to the strong"—and that, therefore, old-time wars led, in some degree, to an increase in the average stature of fighting nations. But it is much less likely that recent wars, waged with modern firearms, picked off the tall so much more frequently than the short as to lead to a permanent reduction in the average height of the belligerent nations. It must be remembered *that the selective agent in most recent wars has been bacteria rather than bullets*, and we have no reason to think that the tall succumbed more readily to disease than the very short.

In any case, it would be extremely difficult to make any permanent alteration in the average stature of any nation of pure or well-mixed race by any process of lethal selection. Variations of stature in the members of any race are, as we now know, mainly a matter of nurture—a matter of mother's milk, oatmeal, fresh air, and so on—and a tall man's progeny and a short man's progeny tend respectively to go up and down to the average height of the race; or, as the biometricians put it, "to revert to the mean." Johannsen, in very interesting experiments, has shown that large pea plants and small pea plants grown from peas of the same pea-pod have equal potentialities; and the individuals, big and small, of a nation are, so to speak, from the same pea-pod, or of the same stock.

A nation like the French, it is true, consists of three distinct races: in the north, the Scandinavian, tall and blonde; in the middle, the Slavonic, medium-sized and dark; and in the south, the Mediterranean, dark and short; and, if so be, a large proportion of the two first races happened to be killed off, there might be a permanent reduction in the height of Frenchmen, but even then the women of Scandinavian and Slavonic stock might be able to perpetuate the physique of their races. Further, any statistics that might happen to show diminution in height of a nation after war must be interpreted with caution, since in many cases the diminution may be due to the poverty and underfeeding that so often follow war.

This slight discussion of these comparatively simple questions may serve to show how carefully we must go when we consider the eugenics and dysgenics of war.

Sweeping statements with regard to the selective consequences of war in

general cannot be made. In every war there is a complicated interplay of conflicting factors; and in each war the factors vary in weight and in direction, so that each war, and almost each battle, will have its own special consequences. A war waged under modern conditions, with machine-guns, and poison-gas, and serums, must be very different in eugenic character and consequences from a war waged with assegais and arrows. A war, again, involving a whole nation must differ greatly in its evolutionary results—social and biological—from a war fought by a few mercenary troops.

Let us, then, consider a special case on its own merits. Let us consider the probable evolutionary effect of the present European war on the biological characters of the English, French, Italian, and Teutonic peoples. (In this inquiry we disregard Russia, for a great part of her huge and very heterogeneous population have been unaffected and will be unaffected by the conflict that is now raging, while the smaller nations pay their own particular tribute to Mars and would require separate consideration.)

In our inquiry we must, in the first place, ask whether the preliminary medical selection of recruits is of evolutionary value.

The nations we have named have sent almost every *fit* man within certain age-limits to fight, and almost every *unfit* man within these ages has been left behind; and a cry goes up from the pacifist and the quasi-scientific and even from the scientific that since the fit go to be killed and since the unfit remain at home to procreate their kind, this preliminary sifting with the temporary procreative advantages it gives to the unfit must, in itself, have evil racial consequences.

It is very doubtful, however, whether this preliminary medical selection can have any important or permanent effects on the future physical fitness of the fighting peoples. Since I myself have examined and selected some thousands of recruits, I have some special knowledge of the nature of the medical selection, and I would draw attention to the following facts, which seem to have been rather ignored.

The great majority of men rejected, are rejected on account of short-sight, rupture, flat feet, varicose veins, heart disease.

Now, short-sight is very often a product of bad domiciliary conditions; rupture is very often due to accident; flat feet and varicose veins are often the result of too much standing; heart disease is very often caused by rheumatic fever. Most of these defects and diseases are acquired, have no effect on the racial value of the individual—since acquired characters are not immediately transmitted—and are not likely to affect his offspring. Even the men who are rejected for deficient physique are not likely to depress unduly the average physique of coming generations; they are a very small fraction of the total male population; many of them had finished their paternal career before the war; and, in any case, most of them are victims of environment, and their offspring under good conditions will tend, as we have already pointed out, to return to the average physique of the race to which they belong. I doubt, indeed, whether, taking all things together, the average enlisted man has three per cent. more racial value than the average unenlisted person.

I cannot, therefore, quite agree with Dr. Abraham Jacobi when he asserts that "the unfit fathers produce unfit children"; I see little reason to fear that the race will suffer from the procreative advantages of the unfit left at home, especially since such advantages have been probably nullified to a great extent by an epidemic of soldier weddings.

Now, let us look at selection in the army by war itself. Armies are not com-

posed, as popular writers assert, of "the flower of the land": they contain men of all sorts and sizes, sons of Anakim, and bantams—men of 50-inch chests, and men of 32-inch chests—magnificent specimens of humanity, and very feeble creatures—men of keen sight, and men of impaired vision.

The question is: Does modern war select the best or worst of these? and if the best, will the net result be serious racial deterioration?

That is a difficult question, and I should not care to answer it dogmatically. But, considering the nature of modern warfare, the impartiality of machine-guns, the wholesale massacre of shrapnel, it seems very probable that death is indiscriminate in his harvest. It is no longer a matter of individual courage and initiative; it is no longer a matter of hand-to-hand combat, where the strong or cunning man survives; it is no longer a matter of disease *versus* constitution; it is no longer a case of battle in the open where the bigger men are the better targets; it is a case of blind, indiscriminate slaughter.

On the dysgenic side we might point out that the best regiments have, in most cases, been given the most dangerous tasks; but whether this selection would be sufficiently stringent to have much effect on the race as a whole must be doubtful, especially in view of the fact that many more are wounded and captured than killed. And even if—as we question—modern warfare *do* chiefly kill off the bigger and the stronger men, so also do many industrial occupations. The average physique of many great industrial centres is much below that of the general population, and the discrepancy is not wholly nurtural. Weaving machines involving sedentary work, bad air, and meagre diet, eliminate the big man much more discriminately than shooting machines: for the big man requires more air and food than he can get. In the United States alone the yearly toll of poverty and preventable diseases amounts to 250,000 dead, and 4,700,000 wounded; and it has been said that the net result of the American steel industry is the manufacture of millionaires and the slaughter of babies. The slaying may in some cases be a eugenic process—though in many industries, as we have said, the most fit to survive are certainly not those of best physique—but the wounding is probably much more dysgenic than the wounding of war. So that we reach the curious paradox that war is eugenic in so far as it takes men from the dysgenic industries of peace.

Even apart from industrial selection, physique, *qua physique*, has no particular survival value on the battle-fields of peace. Money is one of the most important weapons of the armies of peace; and selection by gold is at least as dysgenic as selection by lead or steel. A puny millionaire is more likely to survive and propagate his stock than an impecunious Hercules. When we think, too, of the deep-reaching and wide-reaching dysgenic effects of drunkenness and certain racial diseases, we find it difficult to attach great importance to any possible dysgenic selection by war.

On the eugenic side may be counted the good and sufficient food, the open-air life, and the physical training that soldiers enjoy. These tend to improve the soldiers' general health and to diminish their vulnerability to tuberculosis and some other diseases, and may have some actual racial results, since war, through higher wages, with less overcrowding and more food, will also tend to improve the health of the women and the children of the lower classes. (This does not apply to Germany, which is probably suffering from insufficient food.) Such improvement in health, however, will be probably quite nullified by greater prevalence of drunkenness, nerve diseases, and vice diseases, and by the greater poverty and destitution that will follow the war.

It is obviously very difficult to estimate the net result of such conflicting factors as we have mentioned; but, altogether, and giving due and full weight to the considerations, that it is only a *part* of the male population (the part between the ages of 19 and 45) who are subject to the direct selection of war; that many of these leave children; that many skilled workmen of war age are shielded in war factories; that all females are unselected by war; that variations in physique, even if selected, are often only nurtural, and that in any case all stocks remain well represented in the survivors—taking everything together, and giving due weight to these special considerations, I think we might be justified in concluding that the present war is unlikely to have any important eugenic or dysgenic effects on the three nations we have under view.

But one very interesting and important eugenic action—an action that has been hitherto strangely overlooked—the war will have.

It will lead to a much more stringent selection of women by men.

If men and women are about equal in numbers there is some assortment, but little selection. If there are more men than women there is a selection of men by women. If there are more women than men there will be a selection of women by men. And the greater the disparity in numbers the more stringent will be the selection.

In the case of the four nations under review, there has always been a deficiency of adult males with corresponding selection of females; but, after the war, the deficiency will be much greater, and will lead—especially in view of a probably reduced marriage rate—to a much more stringent selection.

How this fact, and the interest and importance of this fact, have so long escaped scientific notice, it is difficult to understand; but there it is, an unquestionable evolutionary factor.

Such increased stringency, too, of selection of women must in some degree follow every war, so that it is a general law; and it is quite possible that in it may be found the main evolutionary significance and use of war.

I do not for a moment agree with Prof. David Starr Jordan when he asserts that race progress finds its cause in selection only: I believe that there is a *vis a tergo* driving life along certain progressive lines quite apart from selection. But selection plays its part, and no one can doubt the evolutionary value of sexual selection; and war, in this way we have indicated, greatly increases the potency of such selection. If, in the nations we are considering, five million men are killed, that means some millions of women denied motherhood, and a more stringent selection of those chosen to bear children. It is not men the bullets select, but women. War slays men blindly and indiscriminately; there is no racial selection there: the real racial selection is the selection of women made by the eyes and the hearts of the men who survive the war.

Now, on what lines does this selection proceed, and what are its main biological results?

When men have an opportunity to select wives, or when in their hands the choice chiefly lies, there are certain physical characters they usually seek; and these are health, physique, and beauty: for Nature has wisely arranged that men should be attracted by characters that imply capacity for motherhood.

Since health, physique, and beauty are transmitted, it follows that this matrimonial selection favours the evolution of these qualities, and will probably more than compensate for any possible reverse selection by the chances of war.

The upper-class Turks and the upper-class English, who for generations have had the opportunity and have taken the opportunity of selecting healthy and

beautiful women, are distinguished for their health, beauty, and physique; and the fine physique and fine features of the Albanians and Montenegrins may perhaps be explained by similar sexual selection.

Anyhow, this selection does take place: it would seem to be the only important evolutionary factor in war so far as the physical characters of the present belligerents are concerned, and to make for health, physique, and beauty.

Health and physique, however, are characters with spiritual consequences, for it is well known that for a *mens sana* in *corpus sanum* is required. It is very possible, too, that the selection will have more specific results than was summarised in the phrase *mens sana*.

Men select, as we have said, not only health and physique, but also that subtle something called womanly beauty. Now, sense of beauty and craving for beauty are obscure instincts; we do not understand their meaning; but they are also very real and very strong and universal passions, and we cannot doubt that they are factors in the upward moral and biological progress of man, even though we may not subscribe to the dictum: "'Tis eternal law that first in beauty will be first in might."

Of course, there is beauty *and* beauty: the ideal of the Hottentot can hardly be said to make for progress of any kind, and the ideal of the Turk is perhaps largely to blame for the apathy and stupidity of that nation; but I think it will be found that civilised man is inclined more and more to choose such types of female beauty as are correlated with a beautiful mind and with the more feminine virtues of sympathy, unselfishness, gentleness, motherliness. Every war, therefore, will result in a selection that will do something to set up evolutionary tendencies opposite to its own brutal, truculent, anti-social spirit.

Verily it is a fool-proof world!

It is interesting to note, *en passant*, that selection of this nature also makes for the differentiation of nations; for each nation has its own taste in beauty, and this taste, no doubt, has some survival value.

Wisely did Socrates identify the beautiful and the useful, and wisely does William Watson sing:

> "Beauty, the Vision whereunto
> In joy, with pantings from afar,
> Through sound and odour, form and hue,
> And mind, and clay, and worm, and star,
> Now touching goal, now backward hurled
> Toils the indomitable world."

To sum up, then, any influences of the European war on the racial evolution of English, French, and German nations are probably very unimportant, save the racial results produced by the more stringent selection of women which will follow the war as a result of the decimation of men.

"There's a Divinity that shapes our ends, rough-hew them how we will!"

THOUGHTS ON MODERN LITERARY CRITICISM (the Editor).

IN the previous number of SCIENCE PROGRESS I quoted the saying of Mr. Theodore Maynard that "for the last twenty years or more poetry has been left by the English to languish in the dungeons of derision," and argued that, in spite of disclaimers, the statement was true. This contempt is of special interest in

view of the similar neglect of science, and I think that a principal cause of it is the poor quality of much of our modern literary criticism. Thus a professor of poetry at one of our universities, who is himself, *mirabile dictu*, one of our best poets, recently complained in conversation with me of the manner in which our leading review eulogises certain stuff and consigns good work to the kitchen-midden of its "books received." On another occasion I heard a distinguished artist maintain that reviewers of art are ignorant of the elements of it; and one of our greatest novelists who was present endorsed this opinion and described how he himself, when he was a young man who knew nothing of literature, used to "cut up" such men as Ruskin, Carlyle, and Tennyson in the pages of a sixpenny weekly. We know that Swift described the malignant deity called Criticism as living among the icy mountains of Nova Zembla on *half-devoured books*; that Pope tells us

> Let such teach others who themselves excell,
> And censure freely who have written well;

that he compared critics to apothecaries who,

> Bold in the practice of mistaken rules,
> Prescribe, apply, and call their masters fools;

and that Byron clinched the matter in his couplet,

> A man must serve his time to every trade
> Save censure—critics all are ready made.

But these strictures refer mostly to ignorant criticism; while it is the learned criticism which some think is more dangerous to poetry—precisely because it is learned.

For example, every modern work must necessarily touch older works at some point or other, and the book-sated reviewer observes the similitudes, loathes the familiar flavours, and often overlooks the merit of the whole—which is the main thing. Weary of Parnassian simplicity and sameness—for there is really only one beauty—he strives to supplant them by an idol of his making which he thinks the world also should admire. This may be some particular god of his own, but is more generally the image called Originality, or Novelty, or Eccentricity—what you will. To his palled palate, beauty herself is not enough and he seeks the stimulus of some new quaintness which no one has seen before—and which therefore cannot really be beauty; just as the modern philosopher is not satisfied with the problems, difficult enough, of the universe, but must go a-ghost-hunting as well. Hence it often follows that for the modern reviewer (who is frequently learned) the new aspirant for poetic fare must be eccentric in something—phrases, style, subject, or metre. Without this he is a mere copyist, echo, or plagiarist. If he is musical he imitates Swinburne; if harsh, Browning; if choice, Tennyson; if epithetical, Shakespeare; if idealistic, Shelley; if classical, Milton; and so on. Now we see from all the great masters that the first law of all the arts is to keep the feet planted upon the commonplace, and that the second law is to imitate all previous great masters! Probably all the Greek sculptures were largely copied from earlier ones; as cathedrals from cathedrals, epics from epics, and sonatas from sonatas. The modern reviewer would be horrified to hear it maintained that an artist should plagiarise without scruple if the æsthetic rule of the best possible is to be obeyed. Yet whole passages of *Julius Cæsar* (for instance) are taken bodily from Plutarch,

and every good poet has been justly accused by every bad critic of theft. Art is built upon art as coral upon coral and mountain upon mountain:

> For poems are not made but grow; and like
> The crystal spikelets of the ocean caves,
> Accumulate for ever.

Art is the creation of perfections—which become immortal because they are perfections, not because they are eccentricities. Novelty in detail is generally bad art, and true originality must lie in the ultimate degree of perfection of the whole complex—quite another matter and a difficult task indeed.

Mr. Edmund Gosse says sarcastically in the satirical preface to his admirable poems—which he tells us are unknown to readers of the present day—that "there is nothing in which fashion alters so rapidly as it does in poetry." But of course poetry is the same to-day as it was in the time of Job and Homer, and it is only the false gods which are decked in new-fashioned robes. Hence the follies of impressionists, cubists, topicalists, localists, and the rest of them. Art has arts, but no tricks; flavours, but no sauces. True art is great precisely because she is above fashion. Wordsworth protested against the classical form, and Burns is admired for his "local colour"; but the only things worth reading in them are the classical and the universal.

I saw the other day a review applauding what calls itself Imagist Poetry, one merit of which would appear to consist in leaving out the rhythm of verse; and the reviewer seemed to think that there was something new and therefore admirable in this trick. But of course poetry without rhythm is always not only possible but, with certain lofty substance, may be of the very highest; as in the Bible, and in Hugo's prose, where the lines are broken to mark, not the time-bars, but the cadence of sense and sound. It is, however, a different matter with other substance, for which rhythm and rhyme are the water of crystallisation of the thoughts, without which they fall into dust; and a cut diamond is always more beautiful than a lump of carbon. One might as well claim merit for painting pictures without colour.

The same critic then proceeds to contemn the use of certain words like *glamour* because they are supposed to be poetical, and also certain forms of verse because "they are filled with ghosts, Pan and Proserpine, and Paolo and Francesca, and all sorts of old unhappy things that infest the poets' mind and will out in his poetry." The truth is that such names are symbols of whole groups of ideas which the poet uses just as the algebraist uses his x, y and z. In *Pan* we see instantly the mystery of forest and field, and in *Proserpine* the innocence and beauty of the spring flowers. The words are as vital as *mountain*, *river*, and *heavens*, but they present, not only visions of nature, but often also entire tracts of human experience—so that, for instance, the mere words *Paolo* and *Francesca* give us a complete poignant drama in one flash. True poetry delights in such images just as true music loves certain old cadences. But as there are decadent algebraists who would try to teach us algebra without symbols, so there would seem to be critics who would have us forsake the great and ancient symbolic words which have enshrined certain thoughts in us ere the dawn of history.

Such reviewers would reduce us by successive deletions to the inane. The careful construction of speech is mere rhetoric, the crash of stormy words is bombast, images are fustian, passion is rhodomontade, old forms are eclectic, and, in short, the last thing to be used in art is art. Subject is of course quite unimportant, and all the poet must do is to write perfectly simply of simple things,

such as Mary peeling potatoes So the poet obeys, the critics praise him, and the world refuses to read him Or, if the poet disobeys, the critics shatter him—and, again, the world refuses to read him That is one reason why poetry languishes to-day in the dungeons of derision A man of wide taste once said to me in the Academy that the pictures there were the ghost-works of dead artists bitten by critics.

Under these influences modern poetry seems to have become debilitated by the absence of healthy effort along various lines of possible good work Thus, since Swinburne, there have been few attempts at new measures, and we are given nothing but the common jogtrot double-time and jiggitty jiggitty triple time, or the *vers libre* which Swinburne hated and which is really little more than the rhythmless stuff mentioned above Compare Horace with his various beautiful rhythms, built probably (I think) on the accentual (drum music) basis, with adherence to the laws of long and short for the purpose, not of measure, but of euphony. His particular rhythms are seldom suitable for English, but we shall easily find others that are, even besides those employed by Swinburne, Bridges, and, very occasionally, by others. We seem to have lost the sense of drum-music– a real music. Every one has heard what Europeans called the "howling dervishes" of Constantinople—who really performed a sacred oratorio of drum-music for which the European has no ear at all Every day, once in Egypt, an Arab passed my quarters playing on his cymbal an exquisite lyric of rhythm which my friends thought merely barbarous. But this attitude is due rather to inadvertence than nature, for I once heard the difficult choruses of Bridge's *Demeter* admirably rendered at an open-air performance under the guidance of Prof Oliver Elton Our reviewers and public seem, however, to take little interest in such matters, nor in the delicate beauty of the phone-music which must accompany the drum music—as in the ancient verse where each syllable is, so to speak, tasted by the poet and the reader; nor indeed in any of the more subtle fibres of the poet's art How complete is our loss may be judged from the really monstrous but common stage-habit of turning verse in recitations literally into prose—bad enough with dramatic blank verse but absolute murder with lyric verse, even in jogtrot or jiggitty-jiggitty rhythms The delusion appears to be that it is only the sense of the words that matters –at least to the audience. I have heard the same barbarism in French Really a horrible vandalism, but one of which the perpetrators appear to be quite unconscious

The critics delight in words of vague meaning such as *idealism*, *realism*, *individualism*, *values*, and so on, which they hurl like mire at the artists. One observes that they are particularly fond of depreciating work of the previous generation, so that *Victorian* has become as disgraceful to-day as *Georgian* was fifty years ago—in order to prove how much wiser they themselves have now become Each carries his own pocket-sextant with which he measures (from the plain) the altitude of the Parnassians and those climbing upward there. Those whom they dare not censure they depress by calling *minor poets*; and doubtless the greatest poets were all set in this category by most of their contemporaries—who could not see the summits beyond the shoulders of the hills. One can have little confidence in their judgments or in their neglect; and it is a sad thought that really great works may quite possibly often be lost in the flood of matter poured forth daily by the mud-crater of journalism. On the other hand, many of the most commended novels and verses of the day are nothing but another form of this same journalism, meant for the moment and writ for the hour. It is an unhappy reflection, but probably a true one, that literature swamps itself nowadays

by its own excessive output; the concentrated and purified jewel of a life-time's work is lost in that lake of slosh. It is also true that many writers reach fame by the mass rather than by the quality of their penmanship. But in charity we must remember that the duty of critics is, not to criticise, but to amuse. It is impossible to criticise poetry. We can only recognise it.

What, however, most depresses modern poetry is perhaps the puny nature of the themes required by the critics—petty personal reminiscences, baby-pictures, occasional reflections, namby-pamby moralising, old ballad-tales reset with new tricks, verses to Henry and Mary, to faith and constancy and death, to college tankards, to other poets, to politicians, to anti-vivisectors, and so on; and these, not crystallised as perfections, but thrown out anyhow with a chance good line or phrase somewhere. This is the ultimate fruit of the Wordsworthian revolution—of which the critics have been the demagogues and the result a decadent democracy of art living half alive in a land where no one works and it is "always afternoon." Massive inventions are impossible here, great groupings grotesque, constructions wearisome, satire is rudeness, wit buffoonery, drama an occasional wail, and the great movements of the world pass unrecorded. The theory of the simple tale simply told ends merely in gush of no interest to any one, where even those who do the gushing often have little experience of life and no philosophy to gush about. The ancients brought every subject within the domain of poetry; we now give her a single altar lit by the flame of "feeling" and make her a goddess of gush. Where do we find living history, or philosophy, or even politics, or indeed any discussion of anything interesting in our verse of to-day? And the greatest of possible dramas, the drama of science, has scarcely yet been even attempted.[1]

Our charge against writers, reviewers, and public alike comes to this, that what they generally take to be poetry is, even in the intention and throw of it, too trivial to be poetry: an affair merely of verbal prettinesses or curiosities; written merely for the sake of writing, not because there is something to say which must be said. Indeed, it is generally evident that behind the polished phrases there is really nothing at all to say—no blood-won experience either of the inquiry which yields truth, the pain which yields happiness, or the horror which yields beauty. The writers have often seen nothing, done nothing, lived nothing. They deal in words not things, and address critics not humanity. Their art is particular not constructive—not a shining crystal to be held and turned in the hands of Time, but a glittering sand which runs through his fingers. And the reviewers are of the same order—blind, I should say, to all but the word-granules of that which they analyse: so much so that I have never yet seen a review even of the *Iliad* or of one of Shakespeare's great philosophies.

The ancients were wiser than we are, and Lucretius for one put all the science of his day into music. For poetry is not merely a temporary heat of the heart, but a making of monuments of and for the spirit of humanity. The harp of the Muse has many strings—nay every possible string; but the modern reviewer would

[1] One of the friends quoted at the beginning of this article said to me that "not even yet does the world understand the full meaning of science." For instance, a Sunday paper, in reviewing Mr. Masefield's exquisite sonnets on beauty (SCIENCE PROGRESS, January 1917), remarks that "the scientific trimmings of so many of them spoil the sonnets, just as similar embellishments have spoiled some of Mr. Masefield's prose"! Can British unintellectualism go further? The same number of the paper blames a judge for punishing a boy for stealing a rabbit, because the boy said he "did *so* want a rabbit." Science disfigures poetry and theft is justified by desire.

teach the literary man to become a poet by plucking for ever at only one of them, the string of sentimentality of which the world wearies, or of eccentricity which rouses because it jars. The poet is not the literary man. His quality is something beside and above his craft. He is not a purveyor of prettiness. His matter must matter, his words must be deeds, and his ink his own blood. Poetry is an essence thrice distilled, of theme, expression, and music: it is no occasional flower, nor wandering strain, nor the pallid altar-fume of a cult of cloistered critics. Poetry is the inscription of all experience, the tablet of the heart, the record of things seen, the song of the thing done, the breath of action climbed to the summit, thought on the peak, philosophy in music more divine, the perfected utterance of humanity. On the peak, I say; for it is only there that poetry is heard.

ESSAY-REVIEWS

A GREAT PHILOSOPHER, by CHARLES A. MERCIER, M.D., F.R.C.P.: on **Herbert Spencer,** by HUGH ELLIOT. [Pp. viii + 330.] (London: Constable & Co, 1917. Price 6*s*. net.)

THE first three-fourths of the nineteenth century was an age of great men. For the last forty years the general level of intelligence has been higher than in the previous seventy or eighty years, but there has been a remarkable dearth of great men—of men who stand a head and shoulders above their contemporaries, and are recognised by them, as well as by posterity, it may be enthusiastically, it may be grudgingly, to be Great Men. In the nineteenth century there were great men in almost every region of human endeavour Cavour, Bismarck, Lincoln, and Gladstone were great statesmen · Castelar, Gambetta, Bright, Gladstone, and Macaulay were great orators; Napoleon and Wellington were great soldiers; Macaulay, Ranke, Freeman, and Stubbs were great historians; Mendelssohn, Verdi, and Wagner were great musicians; Garibaldi and Gordon were great heroes; Faraday, Darwin, Pasteur, Virchow, and Kelvin were great men of science; and Herbert Spencer was a great philosopher.

Herbert Spencer was undoubtedly a great philosopher. At no time in history were men less swayed by custom and tradition than they are now; but at no time were they more swayed by fashion. We hold our opinions not singly, not individually, but in the mass, along with other people; and the present fashion is to depreciate Herbert Spencer, and to speak of him as a shallow, limited, mistaken man of one idea, whose one idea has been falsified by subsequent discoveries. Nothing is so easy as to gain a cheap reputation for modernity and superiority by sneering at Herbert Spencer and deriding him; and the man who carps and sneers at Herbert Spencer risks nothing, for he is in the fashion, and every one is willing to applaud him and agree with him. It is mainly in this, his own country, that Herbert Spencer is thus depreciated and undervalued, for the English genius is before all things inductive and practical, and Englishmen have an intense suspicion and dislike of what they call theoretical speculations, that is to say, of deductive reasoning that seems to have no immediate bearing upon practice, and of wide and comprehensive principles. Such reasonings and such principles are highly valued by the Latin races, but here they are looked upon with suspicion and distrust when they are not regarded with mere indifference; and yet the two greatest generalisations that have yet been arrived at by deductive reasoning have been discovered by Englishmen, the one by Newton, and the other by Herbert Spencer.

For Herbert Spencer did for co-ordinations in time what Newton did for co-ordinations in space. He gathered them up into a single vast generalisation, and expressed them in a single formula. Whether his formula was right or wrong we need not now inquire. Whether it is the best possible, whether it is at all points unimpugnable, are minor considerations. Merely to arrive at such a formula, merely to state in intelligible terms such a formula and to support it by

evidence that establishes a *primâ facie* case for examination, is a gigantic achievement; and this Spencer undoubtedly did. He did this, and he did much more. He not only formulated his theory, but he compelled the most practical nation on earth to pay attention to it. He spread the knowledge of it and the recognition of it over the whole civilised world. He so brought it home and hammered it into the minds of men that at the present day, though it is the fashion to sneer at Herbert Spencer, yet we all speak and think in terms of his great theory of Evolution. He has modified permanently and for ever the mode of thought of all mankind. Never again shall we think of the universe or of any part of it in merely statical terms. Never again shall we think of it or any part of it as maintaining its state unchanged. Spencer has taught us the truth and the meaning of the hitherto meaningless dictum of Pythagoras: Πάντα ῥεῖ. Everything changes. Change is the law of nature. Pythagoras saw this dimly and expressed it vaguely. Spencer has taught us to see it clearly, and has expressed precisely not only that everything changes, but also how it changes: in what direction, in obedience to what law, in what succession, the changes take place. Whether he was right or wrong in this particular or in that is of minor importance. What is of major importance is that he showed us that all changes take place according to law, and according to the same law; that underlying all the superficial differences of all changes in all things there is the same fundamental law. This is a very great achievement. It is questionable whether it is not the greatest achievement of its kind to which the human mind has ever attained; and this alone establishes Spencer's indefensible right to the title of a great man. We all know Macaulay's famous simile of the child that, raised upon his father's shoulders, cries "How much taller am I than papa?" Thus it is with the little men who now belittle Herbert Spencer, to whom they owe unwittingly the chief part of their mental equipment.

Of all the men that have been selected by the editor of the series of books treating of Makers of the Nineteenth Century, none has a better title to a place in the series than Herbert Spencer; and he is singularly happy in his biographer. Mr. Hugh Elliot has performed his task admirably; with a thorough knowledge of his subject, with a broad and firm grasp of principles; with great critical acumen; and with a power of skilful and clear exposition in excellent English that is almost as rare in biography as it is in philosophy and in science. He is neither blind idolater nor a carping critic. He fully appreciates Spencer's greatness, l yet fully recognises his limitations. He expounds Spencer's doctrines clearly, ırately, and fairly, and criticises them with great acuteness. He is able to 'n that, though some of them may have been deduced from false premisses, not follow that they are wrong; the fact being that Spencer, like all e reasoners, first jumped to his conclusions by a kind of intuition, and ıtly sought for them reasons that were as likely to be wrong as right, and did not display the method by which his conclusions were arrived at. node of arriving at his conclusions first and seeking reasons for them hat is at the root of Spencer's dogmatism. For he was a dogmatist, testy dogmatist, though he did his best to hide his dogmatism under ısonableness. Nothing would have horrified and disgusted him more are him to a classical schoolmaster; and yet his convictions and those l schoolmaster are arrived at by the same process, and supported by ans. Each starts with a settled conviction, and each seeks to prop tion and account for it by reasons which he seeks after his conviction ıd pretends that they settled his conviction. The difference between

them is that the schoolmaster's conviction is almost universally accepted because it is instilled into children, and grows and strengthens with their growth; Spencer's doctrines were received with detestation because, appealing to men of mature age, they were opposed to the whole system of thought on which the English nation prides itself, the system of induction by accumulation of corroborating instances. Not that he did not accumulate instances. He collected and produced them in dazzling and overwhelming abundance; but it was evident that their collection and production were posterior, and not anterior, to the doctrines they illustrated, and therefore they were received with suspicion.

Mr. Elliot introduces his subject by drawing a very apt contrast between the teaching of Spencer and the teaching of the German school of philosophy, and shows how inevitably and necessarily the one tends to produce liberty and peace, the other, despotism and war. The latter is now admitted by every one, and might have been foretold even by those who knew nothing of German philosophy, if they had noticed the effect it had upon Carlyle, who introduced it into this country, and was saturated with it. Carlyle, though a Scotsman, though a member of the nation that is more passionately attached to personal freedom than any nation in the world except the Swiss, became, under the influence of German philosophy, a worshipper of brute force, of despotism and of war. Spencer was the great apostle of Freedom, and he recognised that freedom rests upon peace and needs peace, and produces peace. At the present time it is the free nations that have been attacked and driven to war by the despotically governed nations, and the one despotically governed nation that joined the free nations in enforcing peace by war has been compelled to throw off its despotic government and join the League of Peace, which is also the League of Freedom. Spencer's advocacy of peace through freedom and of freedom through peace has been first derided and then neglected; but the triumph of its adversaries has brought its inevitable result in war, and has compelled all the free nations of the world to acknowledge the truth of the first half of his doctrine, that peace can only be preserved by freedom. It remains to be seen whether, when peace comes, freedom will result. At present the omens are adverse, for never have free nations been governed with such despotism as now prevails. But then we must remember first, that these free nations are thus despotically governed with their own free consent; and second, that they are at war, and war always means despotism, just as despotism always means war. Shall we, when war is concluded, be more free or less free than before? That is the problem that is troubling thoughtful minds.

Next to the inculcation of the righteousness and desirability of freedom and of peace, the chief concern of Spencer's life was the inculcation of the doctrine of evolution; and in this second aim he has been far more successful than in the first. The doctrine of evolution is now universally accepted. It is not only accepted: it is become a truism. It is assumed as a matter of course in every department, not only of what is ordinarily called science, but in every region of thought and of life. Even politicians, even University authorities, the last people in the world to be influenced by new ideas, or by any ideas, are compelled to shape their course by implicit reference to the doctrine of evolution. Even those ignorant persons who look upon Spencer as an overthrown idol, as an expended force, as a misguided and over-rated spinner of flimsy hypotheses, yet utilise his doctrine in their daily work; and while they talk evolution, and think evolution, and base their practice on evolution, belittle the author of the doctrine of evolution. Many do not even call it evolution, and do not know that they owe it to Spencer. They call it Darwinism, and think they owe it to Darwin; but

Darwin did not originate the doctrine of evolution. Darwin did not publish his memorable paper at the Linnean Society until years after Spencer had published the gist of his doctrine of evolution, and Darwin never subscribed to the doctrine of evolution. It is true that his theory of the Origin of Species gave an immense corroboration to that doctrine, brought it into notice, and furnished it ultimately with a tremendous impetus; but neither Darwin himself, the Moses of the New Exodus, nor Huxley, its Aaron, was ever a whole-hearted follower of Spencer.

It is not too much to say that no one can understand the trend of modern thought who is ignorant of the works of Spencer, just as no one can understand mediæval thought and the renaissance who is ignorant of the works of Aristotle. The world is ruled neither by kings, nor by emperors, nor by ministers of State, no, nor by democracies. The world is ruled by ideas, and by the ideas of philosophers. In the last resort, it is the philosophers who rule the world; and in the front rank of philosophers will always stand the figure of Herbert Spencer.

PARASITES OF MEN, by ANNIE PORTER: on **The Animal Parasites of Man.** By H. B. FANTHAM, M.A., D.Sc., J. W. W. STEPHENS, M.D., D.P.H., and F. V. THEOBOLD, M.A., F.E.S., Hon. F.R.H.S. [Pp. xxxii + 900, with 423 illustrations.] (London: John Bale, Sons & Danielsson, Ltd., 1916. Price 45*s*. net.)

DURING the last few years the number of organisms known to produce disease in man and in other animals has increased enormously. The study of parasites has become more and more important from the standpoints of social and economic development. The ravages of disease due to minute animal parasites often transmitted by biting insects have been sufficient to retard colonisation and development of some of the most productive areas in the tropics, while in temperate climates development and productivity have been lessened by the attacks of malarial parasites, spirochætes of relapsing fever, worm complaints, or other maladies of animal origin. The necessity for a knowledge of the causal agents of many diseases practically unknown to England has been evidenced in the present war, where amœbiasis, spirochætoses, malarial fevers, flagellate diarrhœas or dysenteries, coccidiosis and bilharziasis, all have played a part in increasing mortality among the troops. Information regarding numerous animal excitants of disease can be obtained, but it is scattered, is frequently found in obscure or little-known publications, and in many cases is not easy of access either to the zoologist or the medical man desirous of information. The present volume is therefore welcome, since it contains an account of the many animal parasites of man found in the three great groups—Protozoa, Worms, and Arthropoda.

While the book is stated to be partly adapted from Braun's *Die Tierischen Parasiten des Menschen*, English edition, 1908, the adaptation has had to be carried out on so broad a scale that for practical purposes the work is a new one. The incorporation of much new matter, the exclusion of inaccuracies, and the making of numerous alterations were essential for accuracy. The Protozoa section, apart from a few historical references, has been written by Dr. Fantham, and must be regarded as new. The section on Worms by Dr. Stephens has been so remodelled that it, too, is practically new; while numerous additions have been made to the section on Arthropoda by Prof. Theobald. During the time that the work has been in the press, a considerable scientific output has occurred. In order to bring the work up-to-date, use has been made of Appendices, which are

well indexed and have cross references. The book is well illustrated, and a number of new figures are included. There are a copious index and a bibliography of some length. In the section on Protozoa, references to more recent work are given in footnotes, a scheme that could have been followed with advantage throughout the book. It is not always easy for one desiring to consult original authorities to find which author in a lengthy list is responsible for certain statements. The make-up of the book renders it easy of reading, and there is considerable differentiation of type, particularly in the first section.

Section I, PROTOZOA, pp. 1-210, with its appendix, pp. 733-52, by Dr. Fantham, gives a comprehensive account of the parasitic Protozoa, more especially of those affecting man directly, though careful attention is given to those organisms possessing potentialities for pathogenicity or capable of injuring man indirectly. The whole range of the subject is considered, which is necessary nowadays, when never was the need of a sound knowledge of comparative morphology more desirable. Care is taken throughout the section to describe fully the life-history and the morphology of each organism discussed, to indicate its mode of transmission, and to suggest prophylactic measures. Wherever controversial subjects are presented, both sides of the question are set forth, together with the references to the original authorities for the same.

Among the Sarcodina, there is a full and well-illustrated account of *Entamœba histolytica* in all its forms. *E. coli*, Noc's Entamœba, *E. gingivalis* (or *buccalis*) of the mouth, and other entamœbæ of less importance are discussed. Notes on the so-called "cultural amœbæ" of Williams and Calkins, an account of Paramœba and Craigia, now of increasing importance, with the cycle of Chlamydophrys are included. A careful study of the morphology of *Entamœba histolytica* should show the need for a knowledge of comparative morphology, and for the avoidance of dogmatic assertions concerning the same.

Among the Flagellata, the Polymastigina and Protomonadina, including the majority of the parasitic flagellates, receive most attention. The genera Trichomonas, Chilomastix (Tetramitus), and Giardia (Lamblia), all of topical interest, are described and illustrated. The latest information available is incorporated in the appendix. Stress is laid on the various modes of transmission of the flagellates associated with diarrhœa and dysentery, and the rôle of rodents, insects, and water set forth. Among the Protomonadina, members of the Cercomonadidæ, Bodonidæ, and Trypanosomidæ infect man. The account of *Prowazekia urinaria* is clear and well illustrated. The Trypanosomidæ, including the four great genera Trypanosoma, Crithidia, Herpetomonas, and Leishmania, are all dealt with in detail. *Trypanosoma gambiense*, *T. rhodesiense*, *T. nigeriense* and *T. cruzi*, all found in man, are described as to their morphology and life-history in both the vertebrate and invertebrate hosts, while the problem of reservoir animals receives adequate treatment. There is a useful summary of the animal, serological, trypanolytic, and cross-immunity reactions of *Trypanosoma gambiense* and *T. rhodesiense*. Various animal trypanosomes that may occasionally be found in man, or that affect him by destroying cattle, etc., are also described. There is an interesting account of the power of adaptation of trypanosomes, of their variations in virulence, and of development of drug-resistance among them—very suggestive topics.

Much interest has centred around the Leishmania problem. The species differentiated are described, the question of the insect transmitters and the various views thereon are set forth, and an account is given briefly of the recent work on induced herpetomoniasis through insect flagellates. The latter subject has probably far-reaching developments.

Spirochætes and Treponemata are well treated. Their general morphology and life-history are detailed, while attention is drawn to the modes of transmission of *Spirochæta duttoni*, *S. recurrentis*, and *S. bronchialis*. A useful summary of Noguchi's results on the cultivation of various spirochætes and treponemes is incorporated, with references to the originals.

The great group of the Sporozoa is entirely parasitic. It includes very diversified organisms. A brief account of the Gregarines is followed by a detailed one of the Coccidiidea, two genera of which, Eimeria and Isospora, are found in man, though they are better known as parasites of rodents, carnivores, and birds. The Hæmosporidia are well described and illustrated, details of the recent results on the development of Hæmogregarines, cultures of human malarial parasites and Piroplasma, as well as the morphology and developmental cycle of these organisms being indicated. There is also a note on Paraplasma, thought by Seidelin to be the cause of yellow fever, while an inserted slip records the Government report against his view.

Rhinosporidium and Sarcocystis among the Neosporidia infect man, and an account of each is presented in a clear and concise manner. The Ciliata associated with dysentery also are described and illustrated, while a comprehensive yet concise summary of the various views held as to the nature, characteristics, and life-histories of the problematic bodies, the Chlamydozoa, concludes the section.

It may be mentioned that the appendix contains not only notes on recent researches, but also the formulæ for some of the most useful media for the cultivation of protozoa and notes on certain useful fixatives and stains.

Section II., HELMINTHES, by Dr. Stephens, is subdivided into three parts dealing with the Trematodes, Cestodes, and Nematodes. A general account of the anatomy and life-history of each group precedes the accounts of the different genera and species. While due attention is given to the anatomy of the various worms, there is also a constant striving after classification which obtrudes itself upon the reader. Classification can only mark the state of our knowledge for the time being, and there are no arbitrary boundary lines in nature. Again, classification based on incomplete knowledge can only be temporary and subject to much revision as more information comes to hand. The "Worms" are particularly liable to such revision inasmuch as parts only of the life-cycle of many of them have been determined.

The Trematode parasites of man are described in detail. Many are only occasional parasites in man; thus *Watsonius watsonius* has been recorded but once from man, the patient being a negro from German West Africa who died in Northern Nigeria. Gastrodiscus occurs in 5 per cent. of the pigs in Annam, and is sometimes found in man. The life-history of the liver fluke is detailed, and twenty-eight cases have been recorded from man. The malady termed "halzoun" in N. Lebanon, due to a fluke acquired by eating raw infected livers, especially of goats, is more common. The genus Fasciolopsis seems widely distributed in parts of China. When the genus Paragonimus is described, the new classification of the species based on the structure of the spines due to Ward and Hirsch is incorporated. Opisthorchis, Paropisthorchis, and Amphimerus are noted, and attention given to Clonorchis, valuable work on which has been done by Japanese investigators. *Clonorchis sinensis* and *C. endemicus* are still retained as distinct species, though it has been pointed out that the distinctions between them are artificial. As with other zoological groups, size and distribution have sometimes been regarded as specific characters among worms, and such is not the case here.

There is a lengthy account of the genus Schistosoma and of its distribution and pathogenic action on man An inserted slip records the characteristics of adult *S. hæmotobium* and *S mansoni* as set forth by the recent Bilharzia Commission in Egypt There is also a good résumé of the life history of *S japonicum*, the first of these organisms to be well investigated, Japanese scientists again being the pioneers.

The Cestoda are treated on the same plan as the Trematoda The introductory portion contains anatomical details and an account of the general lines of development of the group Some of the new diagrams illustrating developmental stages (*e g*, cysticercus, cysticercoids) are crude in execution, and contrast therefore with the majority Among the more interesting genera described is Dibothriocephalus, the "broad tapeworm" of man, French Switzerland and the Baltic Provinces of Russia being the distribution centres for Europe. It is frequent in Germany, particularly in East Prussia Man acquires the parasite by ingesting the developmental plerocercoid stage with raw or imperfectly cooked fish. *Sparganum proliferum* is of interest as it forms capsules beneath the skin An account is given of Dipylidium and Hymenolepis, parasitic in dogs, cats, and rodents, the transmission being effected by the agency of fleas in some cases The various species of Tænia are described, and statistics given as to the frequency of Tænia infection in man and pigs in Germany.

The Nematodes are a huge group of parasites, many of which occur only occasionally in man Others unfortunately are well known The life-cycle of most of these parasites is described and illustrated The guinea-worm has long been known. The efforts of the Americans to eradicate Ancylostoma and Necator, responsible for so much feebleness among certain sections of the public, have made them known The life histories of the various worms, and the resemblances between certain of their immature stages and those of other, relatively harmless, young or adult worms, are set forth Similarly information respecting filariasis and trichinous conditions, infections with whipworms and strongyles, with Ascaris and other threadworms can be found. The section closes with some notes on the preservation of worms and their eggs

Section III., ARTHROPODA, by Prof Theobald, deals with the two parasitic groups of these organisms, the Arachnoidea and the Insecta. There is a useful series of diagnostic tables given in this section

Two orders of Arachnoidea are considered, the Acarina and the Linguatulidæ. Certain genera of the Acarina receive a good deal of attention Many are parasites of man when he is available as host, though they may spend the greater part of their lives on domestic or wild animals, or on plants. Trombidium in its larval forms is common in barley fields, producing autumn erythema in men whom they attack. Small mammals, such as hares and moles, are the normal hosts, but horses and cattle may be attacked Pediculoides infest cereals and the insect pests found in cereals. They may attack men handling corn. Other mites lead endoparasitic existences in the tissues and air sacs of birds, a few having been suspected of living endoparasitically in man. Certain Gamasidæ may attack man. Thus, the "red fowl mite," Dermanyssus, can attack men who have to enter dirty poultry houses, or attend to infected pigeon lofts. These parasites are described more particularly with respect to their habits, rather than to their structure.

The Ixodidæ are more important to man economically, since certain of them are concerned in the propagation of relapsing fever in man and red water, malignant jaundice, and heart water in cattle, sheep, and dogs. A generalised

life-history of a tick is given, but it is suggested that in a future edition there should be the life-history of a one-host, two-host, and three-host tick set forth. The classification used is based on that of Neumann, and a more recent one, such as that of Nuttall and Warburton, might replace it. Descriptions are given of various members of the genera Ixodes, Amblyomma, Hyalomma, Hæmaphysalis, Dermacentor, and Rhipicephalus. The connection of *Hyalomma egyptium* with piroplasmosis transmission in cattle in Egypt and the Sudan might have been mentioned, as well as the association of this malady with other ticks. With regard to remarks made in connection with *Argas persicus*, it has been proved that spirochætosis is transmitted by it, and the granule phase of spirochætes (which are not spirilla) is accepted by the great majority of workers on the subject. Also, *Ornithodorus moubata* transmits *Spirochæta duttoni*.

An interesting account in general terms is given of the mites that cause local maladies known according to the trades affected, as copra itch, grocer's itch, and coolie itch, while there is a good account of Sarcoptes causing itch in man, with a list of the forms that can be transmitted from domestic animals to man. Short accounts of Linguatula and Porocephalus are also given.

The account of the Insects is introduced by general statements on the structure of insects. Members of the orders Rhynchota, Coleoptera, and Diptera chiefly are parasitic in or on man. The Rhynchota include the lice and bugs. The account of the body lice is somewhat short, considering the importance of these insects in transmitting spirochætosis and typhus fever, neither of which, unfortunately, is mentioned. Further information regarding the life-cycles of these insects should be incorporated when opportunity arises.

The part on Insecta has been brought more up-to-date than that on Arachnoidea, and the author is at his best when dealing with the various mosquitoes that may be injurious to man. Rothschild's classification of fleas, with short notes on the same, is given. The part dealing with mosquitoes opens with an interesting general statement regarding their anatomy and biology. The Culicidæ are considered at length, including as they do Anophelines that carry malaria, Culex associated with Filaria and Stegomyia transmitting yellow fever. There is a good comparative account of the habitat, larval habits and food of the mosquitoes, with clear diagrams of the parts of the flies, and of the scales found on the adult insects. A detailed table of classification of the Culicidæ will be of service to the systematist and to the sanitarian who has to determine mosquitoes that may have disease-producing propensities. A similar table of the species of Stegomyia is given. Various other flies such as Simulium, Chironomids, and owl midges, ncluding Phlebotomus, are described, and then the various Muscidæ associated with different forms of myiasis are discussed. There is a short note on the cutaneous Oestridæ also.

Good up-to-date accounts of the Tabanidæ and Glossinæ are given, together with a table of species modified from Austen. The section concludes with short notices of other groups of parasitic Diptera and an addendum wherein there is a brief record of some recent work on lice prevention.

From the foregoing outline, it will be seen that the contents of the volume are comprehensive, covering a wide field. It certainly should be most valuable to the student and should stimulate research into the many problems of parasitology that still need solution.

TEMPERAMENTS, by THE EDITOR: on **Human Temperaments, Studies in Character**, by CHAS. MERCIER, M.D., F.R.C.P. [Pp. 91.] (The Scientific Press Ltd., 1916. Price 1*s*. 3*d*. net.).

THERE are only three witty men in England, and Dr. Charles Mercier is one of them; but his wit is perhaps broader based on wisdom than is the wit of the others—though they all make the ultimate appeal to philosophy which distinguishes wit from buffoonery. For the display of this faculty, a more admirable subject than human temperaments cannot be imagined. It is a subject capable of classical treatment, and, frankly, we should like to have seen it treated in verse, like the verse of our forefathers now so much disused in an age of daffodils, lambs, and gush—like the verse of Pope whom the author contemns. But after all, nearly the whole of history and literary criticism consists of studies of human temperaments; and what is an essay on Alexander or Virgil but such? Dr. Mercier's little book is if anything too small for his theme, and we should have liked to have seen his deft arrows shot into many other types. Perhaps he will undertake further archery later; but, as it is, he gives us a pretty spectacle; and the reader, whoever he may be, will feel the sharp but not unpleasant pangs in every page—not unpleasant, because he is sure to receive a healing balm in the next one, since the author is physician as well as wit.

The first essay is on the artistic temperament. Witty and true as it is, it seems to be wrong in one respect, and that is in its title—for, should it not have been named the vain temperament? The author distinguishes between the artistic temperament and the temperament of the artist—a much better thing; but of the former, not the artistry but the vanity is the keynote, and the artistry is pretended only to indulge the vanity. "In the extreme instances of this temperament, even the ordinary obligations of morality are not acknowledged as binding or applicable to themselves, though they are quick to resent any relaxation of these rules by which they may suffer. Such persons will rob, and forge, and swindle without any acknowledgment, without, it seems, any realisation, that they are doing wrong." The most typical case of this is the man who caused the present war, who will go down to history as the Judas of the whole human race, having betrayed the god in all of us to death. Such people are "Facile, plausible, and unblushing liars, and display little shame or embarrassment when their lies are exposed"—*vide* the German press. True; but we still say that the burden is placed on the back of the wrong ass, and that the man's vanity but not his æsthetic sense is to blame.

Cleverness and capability are well contrasted in the next essay. After all, as we may put it briefly, cleverness always implies something bad. "The clever man is fertile in devising new ways of meeting circumstances . . . but he does not go to the heart of the matter . . . he makes a brilliant display but he loses his cause . . . if he is a man of science, he is fertile in hypotheses which he does not trouble to verify. As a surgeon, he devises new and ingenious operations, which he executes with deftness and dexterity, for diseases that could be cured without operation . . . the clever man has a good verbal memory; the capable man has a good business memory." All this is true, but nevertheless the clever man often ends by reaching such a wide survey of truth (often from bitter experiences) as the capable man never attains to. Nature is most wonderful in this, that she frequently obtains the best ultimate results by the worst tools, so that vain men have often done much for the world—and the author has remarked this. The essay on the Faddist is live truth. "The negative faddist is anti-alcoholic, anti-carnivorous, anti-tobacconist, anti-vaccinationist, anti-vivisectionist, anti-

patriotist, anti-bellumist. He is anti-restraintist, and would abolish the imprisonment of criminals, and the confinement of lunatics. If criminals must be imprisoned, in spite of his efforts to prevent it, they must at any rate be pampered. They must be allowed to have pianos and newspapers, beer and skittles. He is anti-scientist or anti-Christian; he is an astrologer, a neoteric Buddhist, a flat-earth crank, a spiritualist, a telepathist; in short, the faddist holds any opinion and advocates any cause of action you please so long as it is opposed to the general sense of the community."

SCIENCE PROGRESS analysed this type in an article entitled "Irrationalism" (July, 1914). "Anything that runs contrary to generally received opinion appeals to him with irresistible force, and he is ready to adopt it without any consideration of the evidence: in fact, he is constitutionally incapable of weighing evidence, of suspending his judgment, or of entertaining doubts. . . . The faddist has a very remarkable conscience. To any infraction of the fad his conscience is morbidly sensitive: it quivers with hyperæsthesia; but in the treatment of the adversaries of his fad it is shockingly callous. Rather than permit the slightest infraction of his fad he is willing to sacrifice the lives of his wife and children, and even, within limits, his own comfort. One distinguished anti-alcoholic faddist electrified the world by proclaiming that if his wife were dying and a drop of alcohol would save her life, he would not allow her to have it. He did not make the same promise for himself, if he should be in the same desperate condition. . . . In support of his fad, the faddist will conscientiously lie, perjure himself, slander and traduce his antagonists with eagerness and relish. . . . He condemns with ferocity the 'vivisection' of a rat, even though the vivisection may be but the infliction of a scratch or a prick; but he glories in practising moral vivisection on his opponent. He is a sentimentalist and a humanitarian, and has all the cruelty of these characters." We thank Dr. Mercier for this last epigram, and commend it to the legions of such humbugs in Britain. "It is largely because faddism is the occupation of the idle that it has pretty well disappeared since the war broke out"; but we still see it in the innumerable old ladies who drag about horrid little curs to defile our streets, and who still subscribe to the anti-vivisection societies.

The religious temperament is also well dealt with This word religion has come to mean something quite different from what it meant originally. It really means the sense of duty instilled into men by the untold ages of their development; it has come to mean belief in various hypotheses. We resist this false definition; for duty is greater than any belief, and God, however we may define Him, has said so. The author distinguishes between the religious temperaments which consist in self-sacrifice and vicarious sacrifice respectively. "Those examples of the religious temperament that tend to self-sacrifice provide us with some of the most beautiful and admirable specimens of human character, just as those that tend to vicarious sacrifice provide us with some of the worst." That is why the Emperor William, who has the latter religious temperament, demands the sacrifice of his subjects. Dr. Mercier speaks God's truth when he says that "The monk and the nun who renounce the pleasures and comforts of what they are pleased to call the world, and who renounce them, not in order to be of service to their fellow men and women, but in order to shirk the burden and the battle of life, and to secure for themselves a better future in the world to come, do not command our admiration or our sympathy. They are engaged in a commercial transaction, which they believe will turn out to their profit, and we admire them no more than we admire the trader who embarks on a speculation for the sake of the return that it will bring; or if we do, the admiration is rather for their

astuteness than for their virtue" Religion and morality are indeed very different things according to the new definition of the former word; and frankly we do not at all admire the religious temperament as given in such false terms Those who adopt the policy of believing where they cannot prove are defaulters before Heaven and will never earn its approval, however vividly they may dream they deserve it

Perhaps we like least the two concluding essays. Surely the definition of the man of action is the definition given by the lady novelist—the strong silent man with a square jaw, but Dr Mercier appears to accept it as being valid Now we have seen many such, but they are rather men of inaction than of action, are scarcely ever to be trusted with any work because they seldom ever possess an idea, and commonly end by becoming what are called on the stage "heavy fathers." Surely the author is scarcely justified in claiming men like Alexander, Cortez, Raleigh, Napoleon, and others to be of this type, and what about Nelson and Frederick? Conversely we have seen many men of action, travellers, soldiers, hunters, and they are generally of quite a different type The nervous temperament does almost everything in the world, and one of the hardest workers and most assiduous explorers we ever met was as frail as a shadow and as sensitive as a case of shell shock And, with all due respect to Dr. Mercier, we must protest against his supposition, regarding which he can have no real knowledge, that Copernicus and Galileo, Clarke Maxwell, Faraday, Mendel, and Pasteur undertook their researches without any forethought of the practical use to which those researches might ultimately be put We do not believe it for a moment When these men spent their lives each in finding out the key to a single problem, they did so knowing that when the box was opened its invaluable contents would become accessible to mankind Was not this matter discussed in the last January number of SCIENCE PROGRESS? "A vivid but rather confused recognition of these truths leads the philosopher to look askance upon researches that have a direct utilitarian object, a feeling that finds expression in the toast reputed to have been proposed at a dinner of the Royal Society 'Here's to the latest scientific discovery, and may it never be of the slightest use to any one!'" We can quite understand that this toast was proposed at a dinner of the Royal Society; but not that it represents the feeling of genuine investigators

Well, we shall all be the better for reading Dr Mercier's book—it will ferret out our faults and justify our virtues Perhaps only Homer, Shakespeare, Cervantes, or Velasquez could have written or painted the absolute books on the theme—and indeed they have done so indirectly. But what they presented in combination, our author has, at least partly, analysed in a fine crucible of observation and humour.

REVIEWS

GENERAL

Alfred Russel Wallace. Letters and Reminiscences. By JAMES MARCHANT. In 2 vols. [Pp. vol i, xi + 320, and vol. ii, 291, with 2 photogravures and 8 half-tone plates] (London: Cassell & Co., 1916. Price 25*s*. net.)

FEW tasks are so exacting for a writer of the twentieth century as that of forming a just estimate of those who were most associated with the progress of thought in the nineteenth century. In our earlier years we were, perhaps, ardent disciples—an emotional halo surrounded the master; and our intellectual agreement was weighted and filled out by a mass of sentiment of the same order as, though doubtless of much lower intensity than religion. As time goes on, and we learn to think for ourselves and study the subsequent progress of science, our intellectual agreement begins to evaporate; we find ourselves at many points in opposition, and begin to criticise with the vigour which comes from an intimate acquaintance with the master's work and its weaker points. We have assimilated the main principles, they are now as much part of our minds as they were of his; they have become part of our ordinary stock-in-trade, and cease to excite our special interest or attention. But the points on which we differ stand out with unnatural prominence before our attention. Yet, although our intellectual agreement may yield to considerations of experience and logic, the emotional halo is not so easily dissipated. We are apt still to *feel* more strongly about him than about our contemporaries; and when that feeling is allied to antagonism, we come to under-estimate his value, until there arises a new generation which can view him impartially, and refer him to his proper place in the history of thought.

These remarks are prompted by the first reflection of the present reviewer on taking up the Life of Wallace: "What on earth was it that the nineteenth century saw in him?" A kindly old man, of conspicuous sincerity, and the most admirable moral qualities; but as *we* saw him, with scarcely more scientific judgment or philosophic spirit than the average man of business walking down the street. But was he not the co-discoverer of Natural Selection, and was not Natural Selection one of the greatest discoveries of the last century? Replying first to the latter part of the question, *many* people now think that it was of greater value in stimulating scientific interest than by its literal acceptance. They say indeed that to a great extent Natural Selection is untrue as a complete description of the workings of nature; and that in so far as it is true, it is an obvious platitude as though we were to announce from a pulpit that when a strong man fights a weak man, it will in the long run be the strong man that wins.

However, platitudes have the advantage that even the public find them hard to deny. Organic evolution was formulated in one shape or another long before Wallace was born or any plausible agency could be named for it. But the public (scientific as well as vulgar) refused to believe in evolution because they could not see how it worked, and because there were no references to it in the Bible. What was wanted to secure their conviction was a plausible account of its working; and, if possible, an account of such transparent simplicity that not even expositions by

Bishops could render it opaque. Just such a plausible account was found in Natural Selection. The clergy had previously had no particular difficulty in convincing their flocks that evolution, being wicked, could not be true. But when they went on to affirm that, in a struggle for existence, the weak were more likely to survive than the strong, they overtaxed the credulity of the proletariat, and during the latter half of the century were compelled to nourish themselves on leaks, to the great entertainment and satisfaction of their opponents.

It was not till the proletariat were wholly convinced of Natural Selection, nor till the clergy had at length perceived in it the finger of God, that men of science began to think there was something wrong about it. The Mendelian school arose, protesting that Natural Selection was greatly overdone; that at the best it was only a minor factor in organic evolution. And they suggested another factor which, not being an obvious truism, the public were unable to understand. It was pointed out, moreover, that Natural Selection involved an uncritical acceptance of the teleological theories current in the early part of the last century—theories, namely, which assumed that every tissue and organ had some special *use* to the organism or to man—theories which never considered the possibility of *useless* and *irrelevant* organs or tissues. Natural selection was based, therefore, on the popular belief in the universal utility of all parts of an organism; and little effort appears to have been made to investigate what foundations in fact this belief might possess. An illustration from the present work will indicate the difficulties into which evolutionists were thrown by overlooking this fundamental point. Wallace was asked to explain why a wagtail wags its tail. As an extreme Natural Selectionist he was obliged to postulate, not merely some utility for this insignificant action, but some utility of vital importance for the existence of the species. No alternative even entered his mind. Accordingly he explained to his correspondent that it was "quite easy": a hungry hawk pouncing upon a wagtail, wagging its tail, would have its attention so transfixed by the motion of that appendage as to mistake the tail for the entire bird. It would thus miss the bird and carry off nothing more substantial than a feather from its tail. *Ergo* the wagtail escapes, and wagtails in general survive and multiply. Now we should be very glad to know of some positive evidence in favour of a theory which credits a hawk with a degree of stupidity and myopia altogether incompatible with its own survival and multiplication. If I were desirous of shooting a dog, I should not be deluded into aiming at its tail, merely because that appendage was in motion; yet Wallace's explanation assumes that, if my survival depended on shooting dogs, I should so invariably be misled by their tails, that the canine species would rapidly prosper and multiply owing to my delusion. Far more natural is it to suppose that tail-wagging is a mere outlet of energy released on the sudden cessation of flight, and possessing no evolutionary significance; in the same way that a grouse or a pheasant is apt to crow on settling, without any suggestion of ventriloquist powers for misleading simple-minded hawks.

However this may be, it is clear that Natural Selection fitted well into the prepossessions of the times which discovered it. *If* it should hereafter be established that Natural Selection is of minor importance as an agency in evolution, then the main value of its discovery will have been its propagandist aspect. On account of its being a truism and being teleological, and being so simple that even a clergyman could understand it, it was capable of hoisting the whole theory of evolution into popular approval. The future historian of science will then, perhaps, regard it as the great scientific hoax of the nineteenth century, and will find some pleasant scope for satire in the circumstance that the public could not

be induced to believe in a true theory until it was presented to them securely established on a false basis. But whether false or sound, Natural Selection has done its work well. The theory of evolution is at length too deeply ingrained in our thought ever to be uprooted, even though the original basis of it may happen to be cut away.

As long as biologists are divided on the doctrines mentioned above, it will be impossible to form a just estimate of the position in science of Alfred Russel Wallace. He was unquestionably a very eminent naturalist of enterprising disposition; but, like another eminent naturalist—Fabre—more fitted for the collection of vast multitudes of very interesting facts than for their philosophic co-ordination into general principles. The two aptitudes seldom go together. Those whose minds can spread over a vast collocation of facts do not easily penetrate their inwardness. It speaks much for his mental vigour that he *did* perceive the theory of Natural Selection, and almost by chance bounded into fame through the favourable orientation of public attention, due mainly to the work of Darwin and Huxley.

Though we may yet be unable to estimate truly the ultimate position of Wallace in biology, we are under no such disability with reference to his subsequent incursions into politics and spiritualism. Twice at least he was warned by Darwin to avoid politics. "I hope to Heaven that politics will not replace natural science." "I hope that you will not turn renegade to natural history." Yet his interest shifted almost entirely to politics, and, worse still, to spiritualism. It is unnecessary to comment on the absurdities in which he became implicated. Like a well-known living spiritualist, he seemed to anticipate an influential billet in the next world. Myers writes asking Wallace to put in a good word for him when he arrives in "the next world," "as you will have much influence there." They both seem to have regarded the Almighty as a sort of prime minister in heaven, accessible to backstair influence, which indeed it was rather essential to exercise, if one did not wish to be relegated to everlasting obscurity. Merit is to be modified by social position, etc., etc. Wallace is to be a "friend at court," and the unfortunate outsider, who has no friend at court, will escape the notice or favour of the Lord.

It may well be, however, that the accentuation of these primeval crudities is due more to the biographer than to Wallace himself. For, as a biography, the work cannot be compared to those either of Huxley or of Darwin. Mr. Marchant's capacity as a biographer is limited by the circumstance that he has no knowledge of science, and no power of literary expression. He speaks, for instance, of the "mutual exploration" of Wallace and Bates—a phrase which has a totally different meaning from that intended. He says that "the philosophy of Bergson is *spoken about* on the housetops"—a singularly desolate location in which to discuss it—for Mr. Marchant misses the point of the hackneyed simile which he probably intended to employ. It is more alarming when he informs us that "To Wallace pain was the birth-cry of a soul's advance." How can a poor reviewer adequately deal with such drivel in an important biography of the season! He can only suspect that Mr. Marchant has laboured to bring out all the weaker points of Wallace, and to suppress the stronger, for the benefit of his own peculiar opinions, which however are a subject of but little interest to most of his readers. In conclusion, we can do no more than affirm that this book leaves us pretty much where we were before in reaching a true estimate of the remarkable personality with which it deals.

HUGH ELLIOT.

PHILOSOPHY

(1) **Essays Towards a Theory of Knowledge.** By ALEXANDER PHILIP, F R S E. [Pp 126] (London. Routledge, 1915 Price 2s 6d net)

(2) **Theosophy and Modern Thought.** By C JINARĀJADĀSA, M.A. [Pp 171.] (Madras Theosophical Publishing House, 1915.)

IN the first named of these books, Mr. Philip makes an earnest attempt to deal with the problem of reality He does not bring any new scientific light on the subject he deals with it purely by the methods of metaphysics he has before him the same data as Hume, Kant, Schopenhauer, etc , and endeavours to extract the truth merely by reconsideration and reflection. He will, therefore, hardly be surprised to hear that we can in no way follow his conclusions, which are of a similar order to those of his predecessors, and equally destined to bankruptcy From time to time, in the course of this work, a more cogent criticism is justified What does Mr Philip mean by saying that Perfect Love can overcome Materiality? or that the "Love-divine" generated "the potent current of Life"? Such meaningless and mystical expressions will hardly create a favourable prejudice in the scientific reader

However that may be, the prejudice is not nearly so unfavourable as that created by a perusal of the second book under review. Mr Jinarajadāsa divides his book on Theosophy into four parts, the first of which professes to furnish an account of various modern theories of heredity From this we learn that Darwin based his theory of Natural Selection on the assumption that acquired characters are inherited We learn also that "the dethroning of Darwin" was in the first instance due to August Weismann, who showed "that Darwin's idea of how species arose is no longer tenable" Surely it is surprising that a gentleman who comes from the college of Prof Bateson should harbour a mistake that would reduce any intelligent baby to tears Those tears, however, would soon turn to screams on passing from Mr Jinarājadāsa's science to his theosophy We are there informed that every class, order, family, genus, species, etc , has a "group-soul," which appears to be a sort of monster ghost that infuses its substance into the various individuals making up the group Mr Jinarajadāsa then furnishes a brief account of the origin of life, which turns out after all to be quite a simple matter , for wherever a difficulty occurs, the author invents a spook to account for any process he cannot understand He then goes on to say that "within us [? Theosophists] is the Light of the World, but it is now covered over by our ignorance and delusion," with the latter part of which statement we are at least able to agree

It is curious to find an author in these days who entertains the superstition of reincarnation ; for Mr Jinarājadāsa makes naïve attempts to identify great men of modern times with certain well known men among the ancients He is under the impression that India is the spiritual home of philosophy and knowledge : ludicrous as such an opinion is, we gladly leave him in possession of Kant, Fichte, Hegel, and Schopenhauer, all of whom are stated to be Indians reborn. Goethe, Schiller, and others are renovated Greeks , while—strange coincidence—Francis Bacon is no other than Roger Bacon come to life again. Thus does Theosophy rashly hope to save its Bacon !

HUGH ELLIOT.

MATHEMATICS

A Course in Mathematical Analysis. By EDOUARD GOURSAT, Professor of Mathematics in the University of Paris. Translated by EARLE RAYMOND HEDRICK, Professor of Mathematics in the University of Missouri. Vol. I. [Pp. xviii + 548.] Vol. II, Part I. Functions of a Complex Variable. Translated by EARLE RAYMOND HEDRICK and OTTO DUNKEL, Instructor in Mathematics, the University of Missouri. [Pp. x + 260.] (Boston, New York, Chicago, and London: Ginn & Co. Price respectively 16*s.* and 11*s.* 6*d.*)

THIS is an excellent translation of one of the best of the French treatises on analysis. The original French edition was published in 1902, and the first volume of this translation was apparently published in 1904, although the title-page bears no date. The first part of the second volume is translated from the first half of the second volume of the second edition of Goursat's work, and was published in 1916. The first volume was not radically altered in the second French edition, so that the present English translation of that volume may be used conveniently as a companion to that of the second volume. A second part of the second volume will presumably contain the theory of differential equations.

The contents of the first volume concern derivatives and differentials; implicit functions, functional determinants, and change of variable; Taylor's series and maxima and minima; definite integrals; indefinite integrals; double integrals; multiple integrals and integration of total differentials; infinite series; power series and trigonometric series; plane curves; skew curves; and surfaces.

The first part of the second volume treats successively the general principles of the theory of analytic functions; power series with complex terms and elementary transcendental functions; conformal representation; the general theory of analytic functions according to Cauchy's method of complex integration, including a study of the periods of definite integrals; one-valued analytic functions treated according to the method of Weierstrass and Mittag-Leffler, and Weierstrass's elliptic functions; analytic continuation; and analytic functions of several variables.

The treatise differs from many of the French ones in laying great stress on what is usually referred to as the "intuitional" as opposed to the logical development of analysis. Thus the first volume contains a discussion of differentials as well as derivatives (pp. iii, 19), and a particularly welcome feature is the introduction of integrals by a short and almost historical account of the methods of quadrature used before the integral calculus was invented (pp. 134-40). The excellent collection of examples, which brings the book very close to those to which we have been accustomed in this country, should also be noticed. The translators have added several useful notes to both volumes. With regard to the second of the two volumes noticed here, attention should be drawn to that very characteristic part of Goursat's original work on the proof of Cauchy's theorem on complex integration (pp. 66-70). There is one point which it seems that we may criticise. On p. 139 we read that: "Long before Weierstrass's work, Cauchy had deduced from the theory of residues a method by which a function, analytic except for poles, may . . . be decomposed into a sum of an infinite number of rational terms." The object of the theorems of Weierstrass and Mittag-Leffler was actually to construct certain analytic functions; the object of Cauchy's process was to develop a function which was *assumed* to exist in a certain form.

PHILIP E. B. JOURDAIN

Differential and Integral Calculus. By CLYDE E. LOVE, Ph.D., Assistant Professor of Mathematics in the University of Michigan. [Pp. xviii + 344.] (New York: The Macmillan Co.; London: Macmillan and Co., Ltd., 1916. Price 9s. net.)

THIS text-book possesses some features of great value. Thus, the applications of the calculus to mechanics are dealt with very fairly thoroughly; there is some insistence on the importance of checking the results of exercises, either directly or by solving in more than one way; and a section on line integrals is included which is of interest and importance to students of the integral calculus. The chapters of the book deal with functions, limits, and continuity; the derivative; differentiation of algebraic functions; geometric applications; differentiation of transcendental functions; the differential; curvature; applications of the derivative in mechanics; curve tracing in Cartesian co-ordinates; curve tracing in polar co-ordinates; the indefinite integral; standard formulas of integration; integration of rational fractions; the definite integral; the definite integral as the limit of a sum; integral tables; improper integrals; centroids and moments of inertia; law of the mean and evaluation of limits; infinite series and Taylor's theorem; functions of several variables; envelopes and evolutes; multiple integrals; fluid pressure; differential equations of the first order; differential equations of higher order; and applications of differential equations in mechanics.

It will be seen that an integral is defined as the inverse of a differential before the integral is regarded as the limit of a sum, and a definite integral is (p. 143) defined first of all as the change of value in the integral between the limits of integration. It is stated that "the text is intended to contain a precise statement of the fundamental principle involved. . . . Wherever possible, except in the purely formal parts of the course, the summarising of the theory into rules or formulas which can be applied blindly has been avoided" (p. v). It does not seem that the frequent use of "It can be shown" (cf. pp. 56, 238, 296) and "It is evident" (p. 121; cf. pp. 148-9), quite without any further explanation, is good policy in teaching higher mathematics. Further, even if there is no harm in assuming fundamental points without attempting to prove them, it seems a mistake not to call attention to the fact that the points are unproved (cf. pp. 7, 212).

PHILIP E. B. JOURDAIN.

Functions of a Complex Variable. By THOMAS M. MACROBERT, M.A., B.Sc., Lecturer in Mathematics in the University of Glasgow. [Pp. xiv + 298.] (London: Macmillan & Co., Ltd., 1917. Price 12s. net.)

OF this somewhat comprehensive treatise the author says in the preface: "I have abstained from the use of strictly arithmetical methods, and have, while endeavouring to make the proofs sufficiently rigorous, based them chiefly on geometrical propositions." Thus the method is chiefly that of Cauchy with some of the additions made by Riemann and Weierstrass. Cauchy's fundamental theorem on complex integration is proved by the help of Green's theorem, and a slight modification of Goursat's proof is also given, which is credited (p. 54) to Knopp in his *Funktionentheorie*. The essential point in this proof is, however, not brought out, because the tacit supposition is made that the derivative is continuous. The exposition (p. 57) of the theory of "residues" seems to have some advantages from the teacher's point of view, and the expositions of the theorems of Mittag-Leffler (p. 105) and Weierstrass (p. 108) avoid the usual mistake of considering

them as means for developing a given function in certain forms. The rest of the book is devoted to the treatment of such functions as the Gamma function and the elliptic functions of Weierstrass and Jacobi, and a slightly new point of view in British and American treatises on the theory of functions is introduced by a treatment of linear differential equations of the second order from a modern point of view, including equations of Fuchsian type and the solution of differential equations by definite integrals. The book has certain advantages as a text-book.

PHILIP E. B. JOURDAIN.

Sur les Problèmes célèbres de la Géométrie élémentaire non résolubles avec la règle et le compas. By F. GOMES TEIXEIRA. [Pp. 132.] (Coïmbre: Imprimerie de l'Université, 1915.)

THIS exceedingly useful compilation is a very complete summary of the various solutions that have been given of the three celebrated problems: the duplication of the cube, the trisection or multisection of the angle, and the quadrature of the circle. The contributions of each mathematician are dealt with in separate sections and in an analytical manner, so that modern methods and notations are used throughout. Such a method certainly conveys a clear idea of the point at issue to modern students, and perhaps this indicates the object of the book. The problem and solutions of the duplication of the cube are traced from Hippocrates to Montucci (1869), those of the division of the angle are traced from Hippias to Kempe, and those of the quadrature of the circle from the approximations given in the Rhind papyrus to the expressions of Mansion (1910). A fourth chapter is on the impossibility of the resolution of the above problems by the ruler and compass, and is a very useful analytical treatment from a modern point of view.

PHILIP E. B. JOURDAIN.

Elementi della Teoria delle Equazioni Integrali Lineari. By GIULIO VIVANTI, Ordinary Professor at the Royal University of Pavia. [Pp. xvi + 398.] (Milano: Ulrico Hoepli, 1916. Price 4.50 lire.)

THIS is a triple volume in the well-known series of "Manuali Hoepli," and is as well printed and pleasant to handle as all the volumes of this series. It might well be questioned whether the pages are not rather too small considering the size of type and thickness of the book. It is not that the type is too large: it is of a very convenient size; but, especially in mathematical works, it helps our faculty of understanding to be able to take in a good deal with one glance of the eye, and not to have to turn over pages in the middle of an argument. As for the matter of the book, it forms a most valuable and complete summary of our present knowledge of the theory of integral equations. There is at the end (pp. 367-398) a bibliographical list of books and memoirs on the subject which seems to be very thoroughly done; a great deal of space is saved by omitting the full titles of the works listed, and giving instead a short indication of what the work is about. After some preliminary considerations on analytic functions, linear differential equations, and properties of determinants, the second chapter deals with the equations of Volterra and Fredholm. The third chapter is on relations between integral equations and self-adjoint linear differential equations of the second order, and the fourth chapter is on some applications to mathematical physics—theory of potential, vibrations of a cord and membrane, and the movement of heat.

PHILIP E. B. JOURDAIN.

A Comparative Study of the Early Treatises Introducing into Europe the Hindu Art of Reckoning. By SUZAN ROSE BENEDICT. [Pp. vi + 126] (Concord, N. H.: The Rumford Press, 1914.)

THIS work is a thesis submitted in partial fulfilment of the requirements for the degree of Doctor of Philosophy in the University of Michigan in 1914, and consists of a very laborious and useful contribution to our knowledge of the development of mathematics from the Dark Ages to the Renaissance. This development is not nearly so thoroughly treated in even the best histories of mathematics as it should be, and the most important contributions are in periodical literature Hence the need of such works as the present one. "As a first step," says Miss Benedict (p. 2), "it seemed advisable to find the arithmetics of this period which have been published, and to make a bibliography which may perhaps save valuable time for some later study. Accordingly, a systematic search has been made, and all the treatises found, unless dealing exclusively with abacus reckoning, have been noted. There is included in this bibliography, where it has been possible to find such information, the name and date of the treatise, a few words concerning the author, the library where the manuscript may be found, and the journal or monograph in which it is published There is added to this a brief account of the contents, the approximate length, and, in the case of Latin works, the words of the beginning and end." The bibliography (pp 4-22) extends from Brahmagupta to Peurbach. The scope of this work has had to be limited to a discussion of the fundamental operations upon integers, numeration, addition, subtraction, mediation, duplation, multiplication, and division (pp 23 116) It is, perhaps, unfortunate that German works should have modified the spelling of English so much that "Lewi" appears for "Levi" Miss Benedict finds (p 117) that "it is impossible to conclude that China contributed to the mathematical knowledge of India," that "the suggestion of Greek influence need hardly be considered," and that "among the Hindus, mathematics was developed not as a subject of value in itself, but as an aid to religious ceremonies or business transactions." I think that her second result may be doubted. PHILIP E. B. JOURDAIN.

Theory of Errors and Least Squares: A Text-book for College Students and Research Workers. By LE ROY D. WELD, M.S., Professor of Physics in Coe College. [Pp. xiv + 190.] (New York: The Macmillan Co.; London: Macmillan & Co., Ltd., 1916. Price 5*s*. 6*d*. net.)

THIS is a very simply written and practical text-book on the theory of the distribution of errors, which is of such great importance in all the natural sciences. The chapters deal with measurement, its nature and inaccuracies; the occurrence and general properties of errors; probabilities; the error equation and the principle of least squares, in which Gauss's deduction of the error equation is given in preference to that of Hagen; the adjustment of indirect observations, with illustrations from various sciences; empirical formulæ; weighted observations; precision and the probable error. An appendix contains supplementary notes chiefly of a mathematical nature, together with a collection of important definitions and theorems for convenient reference.

The reading of this book inspires us with the lively desire that at least the elementary questions of the theory of errors should be included in all elementary courses of mathematics. A good place for this theory would be just after where probabilities are treated in most books on what is called in schools "higher algebra." PHILIP E. B. JOURDAIN.

The Combination of Observations. By DAVID BRUNT, M.A., B.Sc. [Pp. x + 220.] (Cambridge: at the University Press, 1917. Price 8*s*. net.)

THIS volume supplies a long-felt want: there has hitherto been no book in English dealing entirely with the theory of errors and the method of least squares and treating the subject in a connected manner, although it has important applications in several branches of science. The author has wisely kept the size of the book within reasonable limits by excluding these particular applications of the theory. When the student has mastered this volume he will have obtained a sufficient groundwork to enable him to proceed to any special applications.

In the first seven chapters the nature of errors of observation is discussed, the law of errors is explained and various proofs given, the assumptions upon which the proofs rest being clearly stated; the cases of one or several unknowns are considered separately, and the principle of weighting observations explained. In the next two chapters the method of adjustment of conditioned observations and the guiding principles in the delicate matter of rejecting observations are briefly set forth. Alternatives to the normal law of errors, the principle of correlation, harmonic analysis from the standpoint of least squares and the methods of periodogram analysis are dealt with in subsequent chapters as fully as possible without unduly increasing the size of the book and sufficiently for the student to obtain a general understanding of these portions of the subject. Each chapter is followed by a few examples which have been carefully and well chosen from various branches of science.

A few misprints, such as "helmon" for "helium," have crept in, due doubtless to the final proofs having been read by the author whilst on active service, and which may be excused on that ground.

H. S. J.

PHYSICS

Eight Lectures on Theoretical Physics. Delivered at Columbia University in 1909. By MAX PLANCK. Translated by A P. WALLS. [Pp. ix + 130.] (New York: Columbia University Press, 1915.)

THESE lectures which Planck delivered as foreign lecturer at Columbia University in the spring of 1909 have for their object the presentation of a System of Theoretical Physics based on certain points of view which are explained in the first lecture. Whereas, undoubtedly, the materials of Physics are the specific perceptions of man as received through the channels of sense, yet all modern advance in the subject has been attained by a definite elimination from all physical ideas of the anthropomorphic elements, particularly those of specific sense perceptions. This may be seen, for instance, by consideration of such ideas as tone, colour, temperature, etc. Thus the definitions of these quantities are to-day not derived from perception through the corresponding senses, but tone and colour are defined through a vibration number or wave length, and temperature through a scale based on the second law of thermodynamics. The thesis on which Planck proceeds is questionable, but the exposition based on it is interesting, and gives an excellent idea of some of the problems now agitating the minds of physicists, in particular the conditions attending irreversible phenomena, the atomic theory of matter, Heat Radiation, Relativity, and the foundation of Dynamics on a single principle such as the Principle of Least Action. Much of the material of these lectures is to be found in the author's "Thermodynamics"

and "Heat Radiation," and in some respects his views on certain points have changed of late, but the lectures present in bold outline the most important advances in the present System of Physics.

J. RICE.

Exercises in Practical Physics. By A. SCHUSTER, Sc.D., Ph.D., and C. H. LEES, F.R.S., D.Sc. Fourth Edition revised. [Pp. x + 379.] (Cambridge: at the University Press. Price 7*s*. net.)

THE work, of which this is the fourth edition, already enjoys a well-deserved reputation among teachers of Physics for the clear descriptions which it contains of a large body of carefully selected experiments. The additions to the material of the first edition are as follows:

(1) A fuller account of the measurement of the magnifying power of a microscope.

(2) A description of an alternative form of water voltameter.

(3) Some remarks on the measurement of thermoelectric power.

(4) An appendix on the determination of dip.

The development of formulæ has been modified here and there, while an account of the preparation of a Weston Standard Cell replaces the account for a Clark Cell of the earlier edition.

Students who have already obtained an elementary knowledge of Physics will find in this book an excellent guide to the principles and methods of accurate measurement. The instructions are complete, and there is a useful appendix giving details as to design and dimensions of suitable apparatus.

J. RICE.

Introduction to Heat. By A. R. LAWS, B.Sc., and G. W. TODD, D.Sc. [Pp. x + 212, with 106 Diagrams.] (London: Mills & Boon. Price 2*s*. 6*d*.)

THIS little volume is designed as a text-book and a laboratory manual for beginners. The authors state that, as boys are gifted with a large amount of curiosity which they endeavour to satisfy, an appeal is constantly made in the book to ordinary experience, so that pupils may realise that there is a great deal to enquire about in the phenomena of everyday life; experiments are designed to elucidate these experiences, others are suggested with the object of inducing a frame of mind in which the pupil will "ask for more."

As far as the writer can judge, the authors should attain their object. The choice and arrangement of the subject matter is excellent. The explanations are lucid and ample. The diagrams—106 in number—are clearly and carefully drawn; while there are numerous problems and examples to test the student's progress.

J. RICE.

X-Rays. By CAPT. G. W. C. KAYE, M.A., D.Sc. [Pp. xxii + 285, with 114 diagrams and illustrations. Second, revised and enlarged, edition.] London: Longmans, Green & Co., 1917. Price 9*s*. net.)

THAT a second edition of this handbook should be called for so soon is not surprising in view of the vast increase in the number of X-ray operators called into being by the war, though it would probably have been required in any case; for, by adding an account of the Braggs' and of Moseley's experiments while the first edition was in course of preparation, Capt. Kaye made it indispensable to all workers in the new field. It still remains unchallenged as the best complete

account of the theory and technique of X-ray work. Very few investigations bearing on the theoretical aspects of the subject have been carried out since the previous edition was published, and from this point of view the changes are few and far between. There is now a full description of Rutherford's and Andrade's experiments on the reflexion of γ-rays; a short reference to the application of the quantum theory to the nature of the rays, and an account of Barnes's, Rutherford's, and Richardson's work on the absorption of X-rays from a Coolidge tube by aluminium. On the technical side, however, there are notable additions in the descriptions of new forms of apparatus, and an entirely new chapter on X-ray equipment and technique written by Mr. W. F. Higgins, who is responsible for much of the X-ray equipment at the National Physical Laboratory. This section contains a full account of the arrangement and use of the apparatus required for a modern X-ray installation, and has been added mainly to meet the needs of hospital workers who would find it difficult to pick out for themselves the practical hints scattered throughout the book. The recommendations for the protection of X-ray operators, issued by the Rontgen Society, have been printed as an appendix. The index remains, as before, a model of its kind.

D. O. W.

CHEMISTRY

A Text-Book of Thermo-chemistry and Thermodynamics. By PROF. OTTO SACKUR, Ph.D. Translated by G. E. Gibson, Ph.D. [Pp. xvi + 439, with 44 illustrations.] (London: Macmillan & Co., 1917. Price 12*s*. net.)

THE recognition which chemistry has received as a result of the events of the past two years is one of the most important matters with which the chemist himself has to deal. What is true of chemistry as a whole is equally true of that important, and indeed fundamental, part of it which we call physical chemistry. At such a time, any worthy contribution to the literature of physical chemistry, which helps to bring into prominence its essential significance for both pure and applied science, is particularly welcome. In the late Prof. Sackur's book we find such a contribution. The author's point of view is clearly expressed in the opening sentence: "Everyone will admit that a thorough understanding of physical chemistry, and of the success of its applications in science and in technology, can only be obtained on the basis of thermodynamics." The book itself is a commentary on that statement, which will bring conviction to any serious student.

Perhaps the most striking feature of the book is its clearness. Admittedly the subject is a profound one, taking us down to principles and laws which underlie all chemical processes. Prof. Sackur does not hesitate to bring the reader into early contact with some of the most recent advances in physico-chemical thinking, but the matter is put in an exceedingly lucid way. The general thermodynamical considerations are never allowed to become too abstract. The student has his attention called continually to the experimental material available. The effect of this is to give a certain concreteness to the treatment, which is eminently desirable in view of the fact that the book is a contribution to an essentially experimental science.

Coming to points of detail, one is struck by the number of very useful tables of data which the book contains, data which serve to illustrate the conclusions arrived at. Certain subjects are particularly well treated, *e.g.*, liquid mixtures and fractional distillation, the deduction of the law of mass action, Helmholtz's theory of the E.M.F. of a cell, and the proof of the Stefan radiation law.

Dr. Gibson is to be congratulated on the excellence of the translation.

W. C. McC. LEWIS

The Rare Earth Industry. By SIDNEY J. JOHNSTONE, B.Sc. (Lond.) Imp. Inst. London. [Pp. xii + 136, with 42 illustrations.] (London: Crosby Lockwood & Son, 1915. Price 7s. 6d. net.)

Industrial Nitrogen Compounds and Explosives. By GEOFFREY MARTIN, Ph.D., D.Sc., B.Sc., F.C.S., and WILLIAM BARBOUR, M.A., B.Sc., F.I.C., F.C.S. [Pp. viii + 125, with 39 illustrations.] (London: Crosby Lockwood & Son, 1915. Price 7s. 6d. net.)

Chlorine and Chlorine Products. By GEOFFREY MARTIN, Ph.D., D.Sc., B.Sc., F.C.S. [Pp. viii + 100, with 46 illustrations.] (London. Crosby Lockwood & Son, 1915. Price 7s. 6d. net.)

THESE three volumes constitute Numbers II, III, and IV respectively of a new series of manuals of chemical technology from the editorial pen of Dr. Geoffrey Martin. At the present time, more than ever, the utility of such a series of manuals is very great. Even to-day our chemical literature shows by contrast with that of other nations a decided paucity of reliable manuals and handbooks dealing with the thousand and one processes in operation in chemical industry. These manuals assist in some measure in remedying this deficiency, but there are still large fields of chemical industry on which they do not touch.

Dr. Martin has rightly called to his aid several of the younger experts in the branches of industry concerned, and the subject matter of these manuals is reasonably up-to-date.

The volume on the rare-earth industry includes chapters on thorium, cerium, titanium, zirconium, tantalum, niobium, tungsten, uranium, and vanadium, together with a chapter on the manufacture of incandescent mantles, pyrophoric alloys, and electrical glow lamps. The volume concludes with a section on the industry of the radioactive substances contributed by Alexander S. Russell, M.A., D.Sc.

The scope of the second volume on nitrogen compounds comprises treatment on the manufacture, properties and industrial uses of nitric acid, nitrates, nitrites, ammonia, including synthetic ammonia, ammonium salts, cyanides, cyanamide and nitrous oxide, whilst the section on explosives covers gunpowders, amide powders, nitro-glycerines, picric acid, T.N.T., ammonium nitrate, chlorate and perchlorate explosives, fulminates, detonators, and smokeless powders. Observations are included on the testing of explosives, their composition and statistics relative to the proprietary brands most commonly used.

The volume on chlorine and its products includes the manufacture of chlorine by the Weldon and Deacon processes, the electrolytic processes, a chapter on liquid chlorine, the manufacture of bleaching powder, hypochlorites, chlorates and perchlorates, chapters on the manufacture of hydrochloric acid, the bromine industry, the iodine industry, hydrofluoric acid, hydrofluorsilicic acid, and potassium bromide. There is a concluding chapter on recent oxidising agents by G. W. Clough, B.Sc., covering the peroxides of the alkali and alkaline-earth metals, the persulphuric, perboric, and percarbonic acids and their salts.

The manuals are well printed, and one of their most notable features is the excellent method in which the subject matter is paragraphed out by the use of several varieties of type. The drawings are good and show a sufficiency of detail, and the authors are to be commended on the very frequent use of tabular synopses. Another very important feature incorporated in these manuals is the prolific references both to scientific and patent literature, and throughout the texts there are extended descriptions of the manufacturing details involved in British and foreign patents.

The style adopted by the authors is somewhat unusual and might be called synoptic or telegraphic, and in some cases only the bare outline of the subject under treatment is furnished. Such a fault is permissible when very full references are given for further information and when it is desired to keep the individual manuals within a reasonable compass as regards size and price, as is here the case.

We regard the manuals as a very satisfactory groundwork, which, if kept up to date by the periodical issue of new editions, ought to prove beneficial to all engaged in these branches of technology.

C. S. G.

The Molecular Volumes of Liquid Chemical Compounds from the Point of View of Kopp. By GERVASE LE BAS, B.Sc., Lond. [Pp. xii + 275, with 9 diagrams.] (London: Longmans, Green & Co., 1915. Price 7*s.* 6*d.* net.)

THIS volume is one of a series of monographs on inorganic and physical chemistry edited by Prof. Findlay. It constitutes the first attempt in English to collect and systematise in one compass the known data on molecular volumes. The issue of such a volume as this will ensure for the author's subject more attention from advanced students and workers in physical chemistry than would be the case if reference had still to be made solely to original papers. The gain to physical chemistry by thus bringing to general knowledge an as yet little used and not highly developed method of attack is considerable.

The author takes first in sequence molecular volumes at the melting point and at the boiling point, followed by the volumes of halogen, oxygen, sulphur, nitrogen and phosphorus compounds, together with those of a few other non-metallic derivatives and a few metals. The volume concludes with a theoretical discussion of the subject, (*a*) as regards the "additive" principle, (*b*) the constitutive influences, and (*c*) relationships to other known physical properties.

The text contains a few spelling errors which will need correction in a second edition, and the author has made a mistake in not preparing any index of contents to his subject matter.

C. S. G.

Analytical Chemistry. Based on the German text of F. P. TREADWELL, Ph.D., Professor of Analytical Chemistry at the Polytechnic Institute of Zürich. Translated and revised by WILLIAM T. HALL, S.B., Assistant Professor of Analytical Chemistry, Massachusetts Institute of Technology. Vol. I. Qualitative Analysis. Fourth English after the eighth German Edition. (Pp. xxiii + 538.) (New York: John Wiley & Sons, Inc.; London: Chapman & Hall, Ltd., 1916. Price 12*s.* 6*d.* net.)

A Text-book of Quantitative Chemical Analysis. By ALEX. CHARLES CUMMING, D.Sc., F.R.S.E., and SYDNEY ALEXANDER KAY, D.Sc., Lecturers in the University of Edinburgh. (Pp. xv + 402.) Second Edition. (London: Gurney & Jackson; Edinburgh: Oliver & Boyd, Tweeddale Court, 1916. Price 9*s.* net.)

IT is a somewhat moot point as to the exact position which qualitative and quantitative chemical analysis should occupy in a University Science curriculum, and signs are not wanting that there is likely to be a reversal of the old idea—due largely to the influence of Bunsen—that the aim and object of practical work is to enable one to perform the qualitative analysis of mixtures. The tendency at present is certainly to increase the importance attached to exact quantitative

work, and to make the qualitative work a subsidiary and more specialised matter, particularly for those who have a leaning towards the analytical rather than the synthetic side of chemistry.

It is a matter for congratulation, both for Dr. Cumming and Dr. Kay and for the students who use their excellent text-book, that a second edition should have been called for so soon even during the stress of war. The arrangement of the new edition remains unaltered, but the whole book has undergone a careful revision, and one or two new exercises have been introduced.

Any one who places a copy of this book in the hands of his students may feel assured that if they do not progress it will not be the fault of the text-book.

As regards the new edition of Treadwell, Prof. Hall reminds us in the Preface to the fourth English Edition that it is no longer to be regarded as a literal translation of the German text, as he has assimilated a good deal also from the text-books of Noyes, Stieglitz and others, "from which many of the ideas introduced into the text have been copied. The general plan of the book has been kept the same, but greater stress has been laid upon the theoretical side of the subject, particularly with regard to the application of the mass-action principle, the ionisation theory, and the theory of oxidation and reduction."

It is unnecessary to criticise the work further than to say that it is fully up to the standard of previous editions which has made "Treadwell" a household word in the chemical world.

F. A. MASON.

A Method for the Identification of Pure Organic Compounds. Vol. II. Containing classified descriptions of about 4,000 of the more important compounds of carbon with the elements of nitrogen, hydrogen, and oxygen. By S. P. MULLIKEN, Ph.D. [Pp. ix + 327, with 3 illustrations.] (New York: John Wiley & Sons, 1916. Price 21*s*. net.)

THE syllabuses of many examining bodies require candidates to refer an organic compound to its proper chemical class by a study of its reactions and properties and then by reference to standard text-books or chemical dictionaries to identify the particular compound in hand by means of its physical constants or some characteristic chemical reaction. For examination purposes such an exercise serves as a test of the candidate's systematised chemical mode of thought and of his ability to draw conclusions from his own observations. Once, however, the examination stage of a chemist's life is over, he is on the whole rarely likely to be confronted with the problem of having to identify any known pure organic compound without having the remotest indication of what to look for, and to succeed in such an identification presupposes the possession of a fairly considerable quantity of material. To meet such a case, however, the author has set himself to write a systematic treatise in four volumes embodying a method for the identification of pure organic compounds. Apart altogether from the merits or demerits of the system elaborated, it must be noted at the outset that, as set forth in the sub-title, the book suffers from the grave defect of being incomplete, inasmuch as it contains only those compounds which in the author's opinion are of sufficient importance to deserve inclusion. The "sceptical chemist" will therefore approach the book with a certain lack of confidence lest the particular compound which he is trying to identify is not included within the covers of the book. Such a feeling does not cross the mind of those who are accustomed to refer to such standard works as Beilstein's *Handbuch*, or Richter's *Lexicon*, for if the compound whose characteristics are required is known, it may

be found with certainty in one of these books or by reference to the indexes of the *Zentralblatt* or some other abstracting journal for the years subsequent to the date of publication of the dictionaries above mentioned. Coming now to the method adopted in the book, one cannot help feeling that it is the product of the one guiding mind which has originated and developed the system, and is thoroughly familiar with its intricacies, but it is somewhat questionable whether other minds will take quite as kindly to it as the author's. It cannot be denied that it is quite a little study in itself to master the system of classification and nomenclature adopted by the author, and any one using the book without having thoroughly familiarised himself with the exact shades of meaning attached to the terms order, genus, division, section, species, specific, semi-specific and co-ordinating tests runs a serious risk of taking the wrong turning and getting lost. It is true that by using the index and looking up the characteristic tests of any particular compound much useful information may be gathered, but this is hardly the way in which the book is meant to be used. The author has no doubt been at great pains to perfect his system, and there is evidence of much conscientious work in the book, but it may be doubted whether the result attained is commensurate with the great labour involved in its production.

P. H.

GEOLOGY

The Rhythmic Deposition of Flint. By Prof. GRENVILLE A. J. COLE, M.R.I.A., F.G.S. (*Geological Magazine*, N.S., Decade VI, Vol. iv., pp. 64-8, February 1917.)

"THE Rhythmic Deposition of Flint," by Prof. Cole, is a suggestion that Liesegang's experimental work on stratified precipitation in jelly may be applied to explain the layers of flint nodules in the upper chalk. Liesegang, according to Prof. Cole, "pictures a solution of silica or a silicate spreading through a fairly uniform sediment. Or, in the case of the chalk, the silica may have been at first distributed with approximate uniformity, and then became affected by a progressive 'one-sided' precipitation." If this explanation of the origin of the layers of flint be accepted, the obvious difficulties on the old view of periodic sponge growths are removed. The crux of the matter seems to be whether the laboratory experiments on the precipitation of silver bichromate in jelly have any real analogy with anything which could be supposed to happen in the case of silica diffused through calcium carbonate. Could such precipitation occur without some exciting cause, such as the sponge layer might be supposed to supply? And more especially, does it account for the fact that the flint layers are formed of separate nodules instead of continuous strata as in the laboratory experiments? Liesegang himself suggests exciting causes or starting-points—"Keime"—for precipitation which may be radiolaria or the hollows of molluscan shells. Which seems to be almost admitting the need of definite lines of some form of organic growth.

G. W. BULMAN.

The Lower Oolite of North Oxfordshire. By EDWIN A. WALFORD, F.G.S. [Pp. 15, with 7 illustrations.] (Banbury: 71, High Street, 1917.)

AN interesting study of the details of the Lower Oolite as it occurs in North Oxfordshire. Such minute investigations of local geology are of no little importance for the general progress of the science. The information, as the writer tells us, has been gathered chiefly from railway cuttings.

The material left for the geologist to work upon in this part of the country seems to consist chiefly of such scattered fragments as have escaped denudation.

In the first place we have a description of beds of the Stonesfield type near Fritwell. These, owing to their fragmentary condition, appear to have escaped the attention of workers. To reconcile the apparent differences between these beds and those of West Oxford is, the writer tells us, the main purpose of his notes.

One of the most interesting structural details mentioned is the "striping" of certain beds. "A course of blue-black limestone," says Mr. Walford, "bore casts and outlines of crinoidal calices, whilst below and through the block were vertical stripes of metal-green with infillings of grey marl." The striped beds of the Lower Oolite were thought to be of plant remains, but the author considers they are crinoidal. "The blue crystalline limestone of Blackthorn is a weld of ossicles of the brachial arms of a species of *Apiocrinus* . . . the black stripes piercing the lower bed represent the rootlets."

G. W. BULMAN.

Economic Geology. By HEINRICH RIES, A.M., Ph.D., Professor of Geology at Cornell University. Fourth Edition, thoroughly revised and enlarged. [Pp. xviii + 856, with 291 figures and 75 plates.] (New York: J. Wiley & Sons, Inc.; London: Chapman & Hall, Ltd. Price 17*s*. net.)

THE fourth edition of this work, which was originally known as *The Economic Geology of the United States*, has been considerably revised, partly rewritten, and enlarged. In a geographic sense, however, its scope is still largely confined to the United States, although description of the more important Canadian mineral deposits is a new feature welcome to British economic geologists. Brief references are also made to well-known foreign occurrences, and the usefulness of the book would have been increased to European students by an expansion of this feature.

The book is divided into two parts: "Non-metallics"—an ugly term, although it would be hard to suggest a substitute—and "Ore Deposits," of which the first-named occupies a little more than half the book. The first part reveals the surprising scope applied geology has, quite apart from the winning of useful and precious metals. The subjects treated in this part are coal, petroleum, natural gas and other hydrocarbons, building-stones, clay, limes and calcareous cements, salines, gypsum, fertilisers, abrasives, asbestos, foundry and glass sands, graphite, monazite, gems, and underground waters, a list which helps in the realisation how important a part geology plays in the winning and investigation of many essential raw materials. Road metal is the only notable omission from this list.

Prof. Ries has a great reputation as a specialist in the economic geology of "non-metallics," and this part of the book is of great value. Yet the work plunges at once into the subject of coal, without any preliminary or general discussion of the non-metallics, such as might have been expected, especially as there is an excellent general chapter prefacing the second part of the book dealing with ore deposits.

Misprints are few, and the book is a pleasure to the eye. "P. Brun" (p. 441) should be A. Brün; and there are misspellings of "propylitisation" (p. 486) and "porphyry" (p. 518). There are a few clumsy or ambiguous statements, as, for example, in the third paragraph on p. 432.

The book is a compilation rather than a philosophical treatise on applied geology; but the facts and figures relating to the various subjects are so well arranged and displayed that it will be very useful as a reference work for student and specialist alike. Its value is greatly enhanced by a fine series of illustrations, and by comprehensive references to authorities at the end of each chapter.

G. W. T.

BOTANY

Algæ. Vol. I. Myxophyceæ, Peridinieæ, Bacillarieæ, Chlorophyceæ, together with a brief summary of the occurrence and distribution of Fresh-water Algæ. By G. S. West, M.A., D.Sc., A.R.C.S., F.L.S. [Pp. viii+475, with 271 illustrations.] (Cambridge: at the University Press, 1916. Price 25s. net)

The need has long been felt for a text-book on the algæ that embraced both fresh-water and marine representatives, whether British or foreign, and the present volume is the first of a two-volume work that aims at covering this wide field. The inclusion of a section on the Peridinieæ is a feature at once novel and welcome, and we cannot but regret the author's decision to omit any account of the Flagellata. Our greatly augmented knowledge of this group in recent years has only served to emphasise its importance in phylogenetic considerations, and the significance of many features presented by the lower algæ can scarcely be appreciated without some acquaintance with the lines of evolution on the Flagellata.

The classification adopted in the present work is considerably modified from that in the author's treatise on *British Fresh-water Algæ*. Thus the Chlorophyceæ are subdivided on the lines indicated by Bohlin and elaborated by Blackman and Tansley.

Following Pascher, three orders are recognised in the Heterokontæ, viz the Heterococcales comprising the unicellular and colonial forms, the Heterotrichales comprising filamentous species, and the Heterosiphonales, with the single genus *Botrydium*.

The account of the Cyanophyceæ contains a useful résumé of modern work upon the cytology of the group and an interesting summary of the conflicting views regarding the nature and function of the heterocyst. The view is expressed that in the living members of the group "the heterocysts appear to have no other function than that of limiting either the hormogones or filaments," but the method of occurrence in *Nostochopsis* and Spratt's observations on *Anabæna cycadearum* suggest that the heterocyst may phylogenetically be a gonidangium.

Schutt's main divisions of the Bacillarieæ into Centricæ and Pennatæ are maintained, but the latter group is subdivided on Forti's plan into mobile and non-mobile species.

The literature on the causes of movement in Diatoms is briefly reviewed, and an excellent account is given of Muller's hypothesis, which regards these spontaneous movements as due to the protoplasmic currents circulating in the raphe.

A very interesting chapter treats of the ecology and distribution of algæ, but the value of the habitat lists would have been greatly enhanced had they been accompanied by a fuller discussion of the factors involved and a more complete account of the work that has recently been accomplished in this direction. Moreover, the biotic relations have scarcely, we think, received the attention they deserve.

Mention should be made of the useful bibliographies appended to each of the main divisions of the subject-matter and which should prove valuable alike to student and teacher. Also there are numerous excellent figures of which more than half are examples of the author's own inimitable draughtsmanship.

In view of Prof. West's wide experience of algæ from all parts of the world we cannot but feel that a more philosophical and less taxonomic treatment of his subject would have added greatly to the value of the work

Prof. West has, however, undoubtedly supplied a real need and filled a long-standing lacuna in our algological literature. We nevertheless fear that the

costly character of the work will place it beyond the reach of that large majority of students for whom the algæ are but one of many groups which claim their attention and interest. E. J. SALISBURY.

Principles of Plant Teratology. Vols. I and II By W. C. WORSDELL, F.L.S. [Vol. I, pp xxiv + 270, with 25 plates and 60 text-figures; Vol. II, pp. xvi + 296, with 25 plates and 95 text-figures.] (London: Ray Society, 1915 and 1916. Price 25*s*. net per volume.)

AFTER the lapse of nearly half a century since the appearance of Masters's volume, botanists are again laid under a debt to the Ray Society for the publication of a work on vegetable abnormalities. No one is better fitted to deal with the large number of facts that have accumulated since Masters's day than the author of the present volumes, who brings to the task many original observations of his own gathered from a wide field.

The first volume deals with the vegetative organs of flowering plants and the comparatively few recorded abnormalities in the Fungi and Bryophyta, and the second with the flower. The author, in his Introduction, states that he regards teratological investigation as the most important method for the elucidation of morphological problems—a view with which many of us will hardly be in agreement. As Mr. Worsdell himself admits, all abnormalities are not reversions; some are progressive, others purely pathological phenomena. In this decision as to the significance of any given case, we can only rely on the indications afforded us by Comparative Morphology and Ontogeny. Herein lies the difficulty of teratological interpretation, and hence the significance attached to any abnormality will often vary with the interpreter.

Although we cannot always agree with Mr. Worsdell's conclusions, the presentation of which is perhaps at times too forensic, in nearly all cases the explanations advanced have the great merit of stimulating thought.

Many of the views expressed deal with controversial matters, but space does not permit of our discussing these in detail. However, we must enter a protest against the statement (Vol. II, p. 128) to the effect that "the facts of floral morphology in the Ranunculaceæ strongly support the view of the staminal origin of the calyx. All the facts put forward by the author in support of this hypothesis are capable of quite another interpretation, and the position taken up really rests on the *à priori* ground that positive *dédoublement* of the perianth is most unlikely to have occurred. As a matter of fact, such numerical increase without metamorphosis does take place quite commonly in several genera belonging to this family.

The work is profusely illustrated with text-figures and photographs, and one can only regret that several of the latter, owing to lack of gradation and faulty lighting, scarcely do justice to the beauty of the subjects. A very commendable feature is the excellent index which accompanies each of the volumes and greatly adds to their usefulness. E. J. SALISBURY.

Plants, Seeds, and Currents in the West Indies and Azores. By H. B. GUPPY, M.B., F.R.S.E. [Pp. vi + 531, with three maps and frontispiece.] (London: Williams & Norgate, 1917. Price 25*s*. net.)

IT is with a sense of pleasurable anticipation that the reader opens a new work on seed dispersal by Mr. Guppy. One cannot, however, attempt to do more than adumbrate the contents of a book so replete with facts and interesting suggestions.

The earlier part of the volume is devoted to the treatment of seed dispersal in the West Indies, and, with this object in view, the drift of the Turks Islands is considered in detail. The author maintains that here the West Indian drift is exhibited in an early stage of its transit of the Atlantic, towards the coast of Europe. The wisdom of this choice is shown by the fact that one-third of the fruits and seeds found in the drift of Turks Islands has, at one time or another, been stranded on European coasts. Something of romance attaches to the history of the tropical drift, comprising some dozen species which have made this journey of nearly 4,000 miles, and it is scarcely surprising that superstitions and traditions have grown up around the Molucca beans and Mary's beans of Northern folklore. The most frequently occurring seeds are those of *Entada scandens* and other members of the Leguminosæ.

An excellent summary of the records of bottle drift, etc., forms the basis for a consideration of the ocean tracks and the rapidity of travel. It would appear that not only is the drift, of the West Atlantic seaboard, carried by the Gulf Stream in about fourteen months to all parts from Norway to Morocco, but the return journey is accomplished in about sixteen months by the agency of the North Equatorial Current.

Similarly the South Atlantic has its own circulatory system of currents, and it is estimated that the passage from the Gulf of Guinea to Brazil would generally occupy not more than twelve weeks—a period sufficiently short to be effective in the dispersal of germinable seeds. In illustration of this, fifty-three characteristic littoral, estuarine, and riverside plants of the West Indies are enumerated, of which thirty-two flourish on the West Coast of Africa. All of these, except four, have seeds or fruits capable of retaining their vitality in sea water for a sufficient length of time to have been transported by the main Equatorial Current.

An interesting account is given of the Geology and Flora of Turks Islands, and Mr. Guppy takes the opportunity of supporting the view that the ultimate basis of Evolution is geographical environment. He regards the cosmopolitan families and genera as the more primitive, characterised by an absence of specialisation that enables them to occupy a wide range. Willis's work, however, on the Ceylon Flora led him to conclude that the species with a restricted range were those of recent origin. The truth may well lie between these two extremes, but in any event the Differentiation Theory here advocated does not appear to us so incompatible with modern evolutionary views as the author maintains.

In the final chapters a description of the Vegetation of the Azores is given, and stress is laid on the small number of species that are truly native. One is particularly struck by the close similarity of the moorland flora and that of the upland lakes to the corresponding plant associations in this country.

If one has any criticism to offer it is that the author's thesis is sometimes obscured by detail which, though intrinsically valuable, distracts from the continuity of the major theme.

E. J. SALISBURY.

ZOOLOGY

An Introduction to a Biology and Other Papers. By A. D. DARBISHIRE. [Pp. xviii + 291.] (London: Cassell & Co., Ltd., 1917. Price 7*s*. 6*d*. net.)

IN A. D. Darbishire the war claims another victim from the rank of English biologists. Although not killed in the field he died in camp a private in the Argyll and Sutherland Highlanders three days before he was gazetted to the Royal Garrison Artillery. He had already made a name for himself by his contributions to current biological theory and for his frank criticism of various

schools of thought. For some time he had been dissatisfied with the present state of biology and contemplated writing a book advocating a new point of view from which to regard evolutionary problems. Two of the four chapters, mainly critical, were completed, the third only very roughly sketched, and the fourth, which was to be constructive and would have been particularly interesting, was unfortunately never started. The finished chapters and various odd jottings have been printed here, and to them has been added reprints of a number of his less accessible papers on theoretical topics. It is sad that he was unable to finish the book, as whether one agreed with him or not, anything he had to say was always of interest and presented in an attractive way. At one time he greatly favoured Mendelism, but in later years he came under the influence of the writings of Samuel Butler and Henri Bergson, as can be clearly seen in reading these pages. The various essays thus give a very clear indication of the development of his ideas, for he was always quite frank in his writings. He will be missed by many friends, and we are glad to find that some of the charm and whimsicality of his attractive personality has been preserved in this book, particularly in his fable and in the introduction.

C. H. O'D.

Form and Function, a Contribution to the History of Animal Morphology. By E. S. RUSSELL, M.A., B.Sc., F.Z.S. [Pp. ix + 383, with 15 illustrations.] (London: John Murray, 1916. Price 10*s*. 6*d*. net.)

THE author of this volume sets out to solve the old problem of whether form precedes function or *vice versâ*. He does not bring forward any new morphological or physiological observations, but, as he states in the title, deals with the subject in its historical aspect. After a longer or shorter period there comes a time in all sciences when an historical review of the main theories is desirable, and that time had certainly been reached in morphology, so that the appearance of the book is timely. It is a salutary thing for workers in a subject, who must of necessity be limited to an intimate knowledge of but a comparatively small field, to look back in a more general way over a wider area. The present work is very useful in that it renders this a comparatively easy task, and is valuable because it has been thoroughly and reliably carried out. The few errors that occur, *e.g.* "no slightest" for not the slightest, p. 264, and the constant reference to Rathke's work on the development of the "*adder*," pp. 137, 144, 150, 152, 153 and 156, whereas the form dealt with was *Coluber* (i.e. *Tropidonotus*) *natrix*, the ordinary grass snake, are of detail and do not affect the main thesis of the book.

In the preface the author states that "In the course of this book I have not hidden my own sympathy with the functional attitude," and we constantly find this point of view introduced, and sometimes somewhat unnecessarily. To take one illustration, it is stated that to Schwann the cell was not a "morphological concept at all, but a physiological." Surely, as far as can be gathered from his works, it is as much one as the other, no more physiological than morphological, but essentially both. The author passes severe strictures on "dogmatic materialism," which will probably meet with approval, but we are not sure that dogmatic vitalism is not worse, for its very "mysteriousness" tends to prevent inquiry into the cause of phenomena.

The earlier more strictly historical parts of the book, starting with the Greeks and continuing, are excellent, but towards the end, where it enters into the realm of recent work, it is a little less satisfactory. A great point is made of the insufficiency of Palæontological evidence, and we personally always get suspicious when

this appears as the main support of any theory. Such evidence must from its nature be fragmentary. It is maintained that the great classificatory groups are not often united by fossil links, and Depéret is quoted with approval when he says that intermediate forms are lacking between Amphibia and Reptiles, and that mammals are also isolated. This, however, is not the case, and Depéret, excellent as are many of his conclusions, is not to be followed in this. The large amount of modern work on Therapsida and on the early Reptilia instead of endorsing this view is rendering it more and more difficult to draw a line of demarkation between the Reptilia and the Amphibia on the one hand and the Mammalia on the other: the three classes grade almost imperceptibly into one another.

This criticism only applies to quite a small portion of the work, which, taken altogether, is a much-needed and well-written addition to morphological literature. No similar work is available to the English reader, and the author is to be congratulated on the careful and complete way in which he has sifted such a mass of literature. It is certainly a book to be read by all interested in the science of Morphology and by the layman who wishes to become familiar with the trend of morphological ideas.

C. H. O'D.

A Naturalist in Borneo. By the late R. W. C. SHELFORD, M A, F.Z.S. Edited by E. B. POULTON, D Sc., LL.D., F.R.S. [Pp. xxviii + 331, with 32 illustrations.] (London: T. Fisher Unwin, 1916. Price 15*s*. net.)

THE late R. W. C. Shelford, who was generally recognised as a leading authority on the insects belonging to the cockroach family, had the advantage of being for seven years the curator of the Sarawak Museum maintained by Rajah Brooke. He utilised to the full the exceptional opportunities that this position offered, and the present volume is an account of his observations on the fauna of that region, particularly the Insecta, but also including the Vertebrata and some anthropology. Unfortunately the author died before the MS. was completed, and the result is a certain amount of inequality in the various chapters; for this of course the editor, who has performed his task with great thoroughness, is in no way to blame. The completed chapters, *e.g.* those on beetles and cockroaches, give an indication of what might have been expected from the others. This criticism must not be taken to imply that any of them are uninteresting—quite the reverse; but one feels in reading them how much more would have been included had the work been completed. As it is, however, the book is a very interesting one and gives a vivid account of the many sides of animal life in a very rich region, by one who was both a keen observer and naturalist. The chapters on the relations between ants and certain plants and on mimicry call for notice as they deal with two controversial matters, and as the author had ample chance of observing the phenomena at first hand he consequently handles them in a manner far removed from that of the laboratory biologist.

The last chapters contain attractive accounts of some of the author's journeys "up country," and a good series of illustrations is finished by a series of photographs of Kuching from negatives taken by the author during his residence there.

C. H. O'D.

The Morphology of Invertebrate Types. By A. PETRUNKEVITCH, Ph.D. [Pp. 263 + xiii, with 50 text figures.] (New York: The Macmillan Co., 1916.)

THIS is a book designed for a course of practical work on invertebrate forms for use in the United States of America. It contains far more types than are dealt

with in junior courses in this country, but does not reach the standard that is generally expected from senior students. In order to avoid the possibility of the student copying figures instead of drawing them from his own preparations, the plan of introducing species other than those dissected is adopted. The end aimed at is a laudable one, but we do not like this way of attaining it, for there is always the possibility of misinterpretation. It is better for the teacher to *see* that the student does not merely reproduce text figures than to do as is done here in certain cases, *e.g.*, to provide a diagram of a cockchafer for a dissection of a locust, not merely a different species but a different order of insect. Most of the figures are reproductions from other text-books—a matter for regret since those from original papers, *e.g.*, 21, 22, 47, and 49 are very useful, while the two original diagrams, namely 32 and 38, are excellent models of what text-figures in a practical book should be. They are clear, more or less generalised, and instructive. The descriptions and directions are throughout easily followed, although certain misprints have crept in, *e.g.*, Bentam for Benham, fig. 20, Dalage for Délage, fig. 5, and *Anadonta* for *Anodonta*, fig. 43. Although perhaps useful in the United States, it is not likely to be employed generally in this country, where the types are not available, but only for occasional reference.

C. H. O'D.

Growth in Length: Embryological Essays. By RICHARD ASSHETON, M.A., Sc.D., F.R.S. [Pp. xi + 104, with 42 illustrations.] (Cambridge: at the University Press, 1916. Price 2*s*. 6*d*. net.)

THIS interesting little book is composed of two parts. The first is a series of three lectures on "The Growth in Length of the Vertebrate Embryo," originally delivered to the University of London, and the second is the reprint of an article on the Mechanics of Gastrulation from the *Archiv für Entwicklungsmechanik der Organismem*. Both these are cognate subjects to which the author devoted many years of thought, and so the book forms a fitting memorial to one whose early death has left a noticeable gap in the ranks of the embryologists in this country. The author himself intended to publish the lectures, but the actual work of arranging them for the press devolved on his widow. The subject-matter of the lectures was doubtless left mainly in the form of notes, and one feels that in certain places a little more explanation, doubtless supplied *en passant* in the lectures when delivered, would have rendered them more easily followed by the general zoologist not possessing considerable knowledge of vertebrate embryology.

Apart from this point, which makes difficult reading here and there, the lectures are extremely interesting. It is not possible in the limits of a short review to do justice to the theories so cogently put forward, for the lectures themselves are the summaries of much original work and thought. If the main thesis of the relations of the long axis of the vertebrate to the plane of the cœlenterate mouth be established, it throws considerable light on the origin of the Vertebrata. It will be necessary to go a long way back in the animal kingdom in order to find any representative of the ancestral stock of the vertebrates. It will also be necessary to consider it when dealing with questions of metameric segmentation. Whether the theories be finally accepted or not they are presented in such a thoughtful way and accompanied by such evidence that they will have to be well considered by all future workers. More than this, we feel sure that the hope expressed in the introduction will be realised, and the publication of this volume will "inspire others to delve into this . . . fascinating field of research." Beside the general interest of the

work there is the personal side, and his widow has laid under a debt of gratitude all those who had the good fortune to know Richard Assheton as a research worker, teacher, or friend.

C. H. O'D.

Recent Progress in the Study of Variation, Heredity and Evolution. By R. H. LOCK, M.A., Sc.D. Revised by L. DONCASTER, Sc.D., F.R.S. [Pp. xxiv + 336, with 6 portraits and 47 figures.] (London : John Murray, 1916. Price 6*s*. net)

THIS book is so well known that little need now be said of its general scope and contents. R. H. Lock is another name added to the list of British biologists who, directly or indirectly, have laid down their lives serving their country in her time of need. Although wishful to serve as a combatant, his special and expert knowledge led to his being appointed chairman of a committee in charge of a station for drying and preserving vegetables and fruit, not only for home consumption, but also to provide variety in the food for the troops. Into this work, as into everything he did, he threw himself wholeheartedly, and continued to go on with it in spite of a breakdown and medical advice to the contrary. When too late he gave in, and after a few days died. The present edition is much enhanced by the addition of a short biography by his wife and a very good portrait of Lock himself

Dr. Doncaster has been responsible for the revision of the present work, and his name is sufficient guarantee for the alterations made, and these are so few that they speak well for the work in its original form. The results of recent work, particularly that of the American school, have been included, and certain additions have been made to the short biographies at the end of each chapter. The book is a useful one, and will continue to be widely read. In addition it now forms a memorial to a worker whose clarity of expression, critical outlook, and general enthusiasm for biological inquiry we could ill afford to lose. It can thoroughly be recommended to all who desire to get an introduction to the modern results and theories concerning the problems indicated in its title.

C. H. O'D.

The Cruise of the Tomas Barrera. By JOHN B. HENDERSON. [Pp. ix + 320, with 36 illustrations and maps] (New York and London : G. P. Putnam's Sons, 1916. Price 12*s*. 6*d*. net.)

THE scope of this book is well indicated in its sub-title, "The Narrative of a Scientific Expedition to Western Cuba and the Colorados Reefs, with Observations on the Geology, Fauna, and Flora of the Region." It is a well-written and throughout interesting account of a six weeks' collecting trip. The original object was to study and collect the Cuban marine mollusca, a subject in which the author had long been interested. Owing to the unexpected generosity of the owners of a fishing fleet at Havana, who set an example that might with advantage be followed in this country, the newest and best boat was put freely at his disposal, and thus the original plan could be considerably extended. The boat was furnished with a large central tank in which live fish could be kept, and this proved of very great value. Various other naturalists also accepted the invitation to join, the National Museum supplied a large amount of collecting material, and the result was, for the time available, a very thorough exploration of the fauna, particularly the invertebrates, of the region. Very interesting accounts are given of the use of copper sulphate for "doping" the tide pools and so bringing out in an anæsthetised condition many animals that would otherwise have been over-

looked. The employment of a submarine light, *i.e.* a properly insulated and waterproofed electric light, led to the capture of a large number of nocturnal marine animals mostly planctonic, but including some interesting fish. Although mainly concerned with marine work, subdivision of the party allowed of a fair amount of work on shore which resulted in a rich "bag" of land molluscs. Several problems regarding Chilian land shells await solution, and the present expedition will no doubt throw considerable light on them.

The book is plentifully supplied with illustrations, some from photographs and some in colour, and on the whole they reach a very high level. The whole book is full of interest and makes one look forward to the fuller and more scientific records of the rich and varied collections that were made under such ideal conditions, for, as this book clearly shows, few things can be more enjoyable than a prolonged hard-working picnic with an amiable group of enthusiastic naturalists.

C. H. O'D.

Instincts of the Herd in Peace and War. By W. TROTTER. [Pp. 213.] (London: T. FISHER UNWIN. Price 3*s.* 6*d.* net.)

MR. TROTTER'S book is a reasonable and seasonable study of certain sociological aspects of the gregarious instinct. The relationship between individuals and the communities which they constitute is not unlike the relationship between the body and its constituent cells. The cells are more or less capable of living their own individual lives; but, incorporate in the body, they are subject to constitutional incitements and excitements *en masse.* And even so, both in peace and war, individuals, by processes of suggestion and emotional contagion, acquire functions and characters that are integrated to a common end. In peace we have such pathological instances of gregarious impulse, as tarantism and flagellation. In war we have the amazing spectacle of millions of men destroying each other. And instances of everyday less sensational herd-instinct abound.

Mr. Trotter deals with his subject in a competent and interesting manner; but his mode of expression is often clumsy and involved, and we are inclined to question whether his division of gregariousness into aggressive, protective, and social types can be singly exemplified in civilised nations of to-day. Rather, perhaps, all civilised nations and their gregarious impulses are complexes representative of all three types.

Read in conjunction with such treatises as Le Bon's *Psychology of Peoples*, Spencer's *Political Bias*, Sidis' *Psychology of Suggestion*, Mr. Trotter's book will certainly suffice to set the reader on the road towards the comprehension and solution of various social problems particularly instant at the present time.

RONALD CAMPBELL MACFIE.

The Insects attacking Stored Wheat in the Punjab, and the methods of combating them, together with a Chapter on the Chemistry of Respiration. By J. H. BARNES, B.Sc., F.I.C., F.C.S., and A. J. GROVE, M.Sc. Memoir of the Department of Agriculture in India, Chemical Series, Vol. iv., No. 6, 1916. Agricultural Research Institute, Pusa. [Pp. vi, 175–280, with 12 text-figures and 4 coloured plates.] (Calcutta: Thacker, Spink & Co.; London W. Thacker & Co. Price 5*s.* 6*d.* net.)

THE senior author of this memoir was endeavouring to prevent the damage caused in seed wheat through the action of insect pests, using for the purpose

such methods as could be worked efficiently and safely under the limitations of a Punjab village community. Since these limitations exclude expensive appliances and dangerous toxic substances he was led to experiment on the use of carbon dioxide. The effect of the CO_2 on germination was studied, and also the moisture content of the wheat as found on the market, but the problems obviously needed the collaboration of a biologist for adequate solution; this was provided by the appointment of the junior author, and the biochemical combination led to some interesting results, of a type which is comparatively rare in entomological work, though sufficiently familiar to plant physiologists.

The detailed life-history of each of the principal insects infesting wheat in the Punjab was worked out, and is presented with coloured plates. Their distribution in relation to climate within the Punjab was also studied. The chief culprits were the Dermestid *Attagenus undulatus*, *Rhizopertha dominica*, and the weevil *Calandra oryzæ*.

The studies of germination, of moisture content, and of the effects of CO_2 and SO_2 during germination, both with and without fresh air-supply, provided a large amount of data, though the inclusion of a plant physiologist in the partnership would have enhanced their value by preventing a few crudities in the interpretation. Also, an exposure of wheat grains to chloroform vapour for a quarter of an hour can scarcely be depended upon for sterilising purposes. The experiments showed that the moisture content of ordinary Punjab wheat was usually low enough to protect it against *C. oryzæ*, as Lefroy had stated, but that this dryness was no bar to the activities of the two other pests, which are the more important except in the damper parts of the province. The injury caused by CO_2 and SO_2 was shown to be such as to preclude all possibility of using these gases as fumigants for seed wheat.

The next two chapters deal with the behaviour of the pests themselves in atmospheres of CO_2, nitrogen, and hydrogen. Extensive gas analysis experiments are described, and a lengthy historical account of the physiology of respiration in general is given. An "oxidase," or mixture of oxidising enzymes, was obtained from the crushed larvæ, and is considered to be the active agent in that output of CO_2 from the living larvæ which takes place under anærobic conditions. The effect of a rise of temperature in shortening the lethal period was demonstrated, but without tracing any exact relation.

Perhaps the most striking feature of the enquiry is the very pretty demonstration of specific differences between the three pests in their relation to moisture (Chap. V.). Though the results themselves need explanation . . . indeed, they open up a whole field of research . . . yet they in themselves explain all the known facts of the distribution and activities of these pests in the Punjab. The insects were starved at various temperatures in dry air and in saturated air. In the case of *R. dominica* the change of humidity had practically no effect on the duration of life; in that of *A. undulatus* (the "woolly-bear" Dermestid) life was shorter at all temperatures in moist air than in dry, while although for *C. oryzæ* the differences were trivial at 40° C., yet the insects kept in moist air at 31° C. lived thrice as long as those deprived of water. The run of the data seems to indicate further interesting possibilities with regard to the death-point.

The authors, having found naphthalene useless for food-wheat, on account of the flavour which it imparts subsequently to the bread, finally devised an air-blast method for cleaning out the damaged grains.

The memoir covers so much ground that it cannot be exhaustive, but the results are full of suggestiveness and of data which can be utilised in several

branches of science; its authors are to be congratulated on having struck out novel and useful lines in a department of economic science which has a tendency to be conventional.

L. B.

Horses. By ROGER POCOCK. With an Introduction by Prof. J. COSSAR EWART, F.R.S. [Pp. x + 252.] (London: John Murray, 1917. Price 5*s*. net.)

THE politician traces the development of man in the history of parliaments and wars, the architect reads man as he expresses himself in brick and stone, and the artist builds up his estimate from a knowledge of man's productions in the realm of art; but to Roger Pocock, the frontiersman, the history of the world is the history of the horse, with man as an adjunct. The effect is as interesting as it is unusual. His book on this subject begins with a description of the origin of the horse and the early civilisation of man, which is based evidently on the author's reading, and has, therefore, a tendency to dullness; but when he abandons his erudition and plunges into his own wide experiences of equestrian travel the matter is vivid and striking, and he succeeds in imparting to his reader some of his all-absorbing love for his favourite animal. The account treats of the horse from all points of view, his usefulness for transport, travel, pleasure, sport and war; and the information is seasoned, not only with excellent descriptions of the country through which the author has passed, but with flashes of a fresh and caustic wit that is never for a moment tainted with malice; and his writing discloses, quite unconsciously, the strength, sweetness, and sympathy of his own character. Not the least important chapter is that on the "soldier horse," wherein he criticises strongly the folly of the British War Office in throwing on to the field of battle a strain of horses raised exclusively for the exigencies of the hunting-field, and consequently lacking the staying power and mobility required for the purposes of war. He shows also how the equipment of both horse and rider and the training of the latter wofully need the application of a little common sense. These criticisms are not born of the conceit of his own opinion, for he brings forward specific instances of ignorance, of which the most notable is the loss, during the Boer War, of a large number of horses within a fortnight, due to the fact that the officer in charge insisted on applying stable treatment to animals which had to live in the open without a stable. The interest of the book is not diminished by the circumstance that the author has written it in the intervals of his duties at the front in the present war.

ANTHROPOLOGY

Arboreal Man. By F. WOOD JONES, M.B., D.Sc. [Pp. x + 230.] (London: Edward Arnold, 1916. Price 8*s*. 6*d*. net.)

TRUTH is stranger than fiction, but it is commonly much less interesting. This must be so whenever it is presented to us in a dishevelled state, with the excuse that "Truth needs no adornment." This seems to have been the point of view adopted by Dr. Wood Jones when he decided to give us this book on arboreal man. He tells his readers, in his Preface, that he is fully aware of the fact that his pages are marred by literary defects, and "lacking in proper literary sequence." But these blemishes, he explains, are inseparable from the origin of his book, which is formed out of the material of certain Arris & Gale Lectures, delivered in the Theatre of the Royal College of Surgeons of England, during the years 1915 and 1916. For some reason, not clearly apparent, the author has re-issued these

lectures with but little alteration, though he evidently fully realised that to make a readable book they needed to have been re-written

It is well, however, that the reader should be prepared for what is before him, for he will certainly find this not only a colourless, but also a bewildering book. Though his pages are crammed with facts, and evidences of original research, they nowhere grip the attention

Briefly, the author has set himself the task of demonstrating the exceedingly primitive character of man in regard to just those features wherein so many writers have seen in him the last word in evolution, or evidences of design, or special Creation, as the sympathies of the champions of these very opposite points of view inclined them

In marshalling his facts, so as to crush the extremists of these opposing camps, the author has over-reached himself, that is to say he has pushed his arguments too far Throughout, he has insisted on the wide differences between man and the pronograde mammals, among which he contends man has never been included Dr. Wood Jones may argue that all he means is that man was never a terrestrial pronograde This will not suffice He strives, laboriously, to show that the descent of man is to be traced continuously from an arboreal stock, which, from the very nature of its habitat, was saved from becoming pronograde. Yet indutiable witness to the contrary is to be found in the human body to-day, as witness the suspension of the intestine, which, in the early embryonic stages of development, is like that of the pronograde monkeys, while later it assumes the arrangement which obtains in the orthograde Primates, like the gibbons The vestigial azygos lobe of the right lung, in like manner, bears witness to a pronograde stage, and the same is true of the case of a number of muscles now found in the human body only as mere vestiges, but which were large and functional, during a pronograde stage of development, answering to that of monkeys to-day

More might profitably have been said of the foot of the Chimpanzee, and mention might also have been made of the fact that the human infant, in its first efforts to stand alone, commonly endeavours to obtain a grip of the ground with its toes, reminiscent of the time when it was a tree-dweller. Finally, we venture to think that the post-arboreal stages of man, to which the author makes many allusions, deserved a chapter to themselves They are of immense interest, and they would have served as a standard of comparison for the earlier chapters of the book

Our complaint is not that Dr. Wood Jones has proved himself unequal to the task he set himself, a view which would not be tenable for a moment, but that he could have made his book vastly more entertaining: while, further, he would have given us, in place of a vague, nebulous conception, a crisply defined image of "Arboreal Man" which one could have contrasted with that most exquisitely beautiful product of evolution—the human body.

W. P. P.

MEDICINE

The Essentials of Chemical Physiology for the use of Students. By W. D. HALLIBURTON, M.D., LL.D., F.R.S. Ninth edition. [Pp. xi + 324, with 72 illustrations] (London: Longmans, Green & Co., 1916. Price 6s. net.)

PROF HALLIBURTON'S *Essentials of Chemical Physiology* has reached its ninth edition within twenty-three years.

Its success was to be expected, for the author combines the faculty of making

the complex appear simple with that of discriminating, with accuracy, between the more and the less essential

The book is so well known as a full, clear, and reliable guide to elementary work, that no detailed description of its contents is necessary

The present edition differs but little from its immediate predecessor. The ninhydrin reaction has been introduced, but exercises on the analysis of simple gas mixtures such as the atmosphere, expired air, and the blood gases are again omitted. This omission is a little surprising The apparatus required for simple gas analysis is neither complicated nor costly, and affords valuable practice both in measurement and in calculation The physical aspect of Chemical Physiology, somewhat scantily and disconnectedly presented, is still relegated to the Appendix It may be predicted that, in course of time, this section will be more fully treated and transferred to its proper place in the body of the work

The book is admirably illustrated, with a coloured plate (osazone crystals), with structural formulæ, and with 71 clear diagrams showing (amongst other things) chemical products and apparatus Diagrams of the last type are particularly helpful in elementary work and cannot be too numerous. One would welcome, for example, the inclusion of apparatus for extraction and for filtration amongst the excellent illustrations already provided

The text is well printed, unusually well spaced, and therefore exceptionally easy to read.

The work is, in short, as good as ever, and its continued success appears to be assured.

W. L SYMES.

The Biology of Tumours. By C. MANSELL MOULLIN, M D, F R C S [Pp 55] (London H. K. Lewis & Co, 1916 Price 2s 6d net)

IN this volume the author deals more thoroughly with the views which he had already expressed on the biology of tumours in the Bradshaw Lecture for 1912. A scheme of classification of tumours is advocated which is based on their mode of origin, and from this point of view they are divided into two classes Those in the first class are due to the retention, within the organs or tissues, of cells whose development has been arrested, and which are consequently able to manifest their power of reproduction when a suitable stimulus, such as chronic irritation, is provided. Those in the second class are due to defects in structural development. With the exception of a few comparatively rare types all tumours belong to the first class.

No subject in the whole of pathology is so difficult as that of tumours, and in endeavouring to place their origin and classification upon a scientific basis the author has undertaken a task of the first magnitude. He has presented his theory, which in some respects is a combination of Cohnheim's theory of "cell-rests" and Adami's theory of the "habit of growth," in a clear and masterly way, the attention of the reader is held throughout the book, and a powerful stimulus to thought on the vexed question of tumour formation is provided.

The general adoption of the suggested scheme of classification would, however, virtually amount to an acceptance of the above theory of the origin of tumours, and it is possibly better, for the present at any rate, to rely on the older methods of classification, which take into consideration the structure of the tumour, and which are for this reason of some practical value.

J. W. CROPPER.

ENGINEERING

The Flying Machine from an Engineering Standpoint. By F. W. LANCHESTER, M.Inst.C.E., M.I.Mech.I., M.Inst.A.E. [Pp. viii + 135, with 56 figures in the text.] (London: Constable & Co., Ltd., 1916. Price 4*s*. 6*d*. net.)

THIS volume is a reprint of the "James Forrest" lecture delivered before the Institution of Civil Engineers in May 1914. As a lecture at that date there can be no question it was a valuable contribution to our knowledge on the subject, and it will remain as such in the *Proceedings* of the Institution before which it was read. As a book published at the end of 1916 and placed on the market in 1917, after two years of full experience and progress in air-craft and its machinery, it is very much out of place and out of date. So many of the best of our young manhood have taken up flying that there is little wonder that the demand for books on flying machines is very great. This does not justify the present reprint. The engineer is not concerned at the moment with "Catastrophic Instability" as treated in this volume; he is much more concerned with the peculiar constructional problems of the various classes of machines, from strategical-reconnaissance machines through a long range of other machines to pursuit machines.

Engine troubles are also his direct concern. The frequency with which certain parts have failed owing to the fatigue of the materials used has been a feature of the war. The freezing up of the water-cooling system is another serious matter.

The adjustments of the carburettor and questions of ignition are problems of the moment. Landing brakes and gear are giving the engineer some concern. Yet in the volume before us not one of these important engineering problems is discussed. The least that can be said is that the book should have been published under the title of "The Flying Machine from an Engineering Standpoint in 1913."

J. WEMYSS ANDERSON.

MISCELLANEOUS

The Johnson Calendar; or, Samuel Johnson for Every Day in the Year, being a Series of Sayings and Tales, Collected from his Life and Writings. By ALEXANDER MONTGOMERIE BELL. [Pp. 234.] (Oxford: at the Clarendon Press, 1916. Price 2*s*. net.)

DEDICATED to Mr. Asquith because, out of many judges, members of Parliament, writers of leading articles, reforming ladies and other celebrities, he was the only one who quoted Dr. Johnson correctly! The other day a member of Parliament asked Mr. Asquith his opinion on some subject, and the late Premier replied wistfully, "I suppose that I must not say 'Wait and see.'" But if his policy of waiting and seeing has had no other fruit except perhaps dead-sea fruit, it has at least enabled him to earn the dedication of this charming little book. It is only 6 inches by 4 inches in size, and may therefore be carried in the pocket for railway journeys, where it will do much to counteract the effects of freezing feet and basket-lunches. Samuel Johnson deserves the high place given to him in *Hero Worship* by Carlyle. A really great man, because, chiefly, of his passion for accuracy combined with his courage and judgment. Who has ever written a better summing-up of Whigs and Tories? In Mr. Bell's Calendar for June 18 we read that "A high Tory makes government unintelligible: it is lost in the clouds. A violent Whig makes it impracticable: he is for allowing so much liberty to every man, that there is not power enough to govern any man. The prejudice of the Tory is for establishment: the prejudice of the Whig is for innovation." Of women preachers

Dr. Johnson said: "Sir, a woman's preaching is like a dog's walking on his hinder legs. It is not done well; but you are surprised to find it done at all." He also said of the sex, "That one of the great felicities of female life was the general consent of the world that they might amuse themselves with petty occupations, which contributed to the lengthening of their lives and preserving their minds in a state of sanity." A lady said to him that because men cannot hem pocket-handkerchiefs they torment their families and friends; and this text we once saw much embroidered by Mrs. Sarah Grand, who suggested that retired generals and colonels would do well to take to knitting. Let this little volume be added to the scientist's library and its contents to his philosophy.

R. R.

Raymond; or, Life and Death: with Examples of the Evidence for Survival of Memory and Affection after Death. By Sir OLIVER J. LODGE. [Pp. viii + 404, with 18 illustrations.] (London: Methuen & Co., Ltd. Price 10*s*. 6*d*. net.)

THE first part of this book is a brief account, written with reticence and good taste, of one of the heroes of the war. Of these young men, whose privilege it has been to live most of their working lives, and finally to lay them nobly down, in the service of a great ideal, Raymond Lodge was a fine example. And, while we must condole with his parents in their sense of loss and their disappointed hopes, we cannot but congratulate them on the greatness of the sacrifice which the possession of such a son enabled them to offer. It is an inspiring record, and especially so because we know it to be typical.

The second part is on different lines. It may be taken, we suppose, to indicate the nature of that "proof amounting now to certainty" which Sir Oliver Lodge has for years declared to be accumulating in favour of his belief that the human mind not only survives bodily death, but can and does communicate with those still alive. The communication is less direct than that between earthly friends: there has to be usually, beside the "medium" on this side, a corresponding medium, called the "control," on the other; and the message may be spoken by the voice of the medium (usually in trance—more or less deep), or written by the hand of a human agent more or less conscious, or signalled in an alphabetical code by tilting a table on which the medium's hands are placed. Very shortly after Raymond Lodge's death his relatives attempted to get into communication with him by these methods. The results are interesting. It may be taken for granted that a competent medium will say things implying a knowledge of the sitter's affairs that has not been acquired in any ordinary way. We think it is also proved that occasionally a medium will give true indications of matters which he has not learnt in any ordinary way, and which the sitter himself does not know. There is an instance in the book before us, where the medium described with some detail a photograph which no member of the Lodge family had heard of, and which at the time had not reached England in any shape. And thirdly it is claimed that the communications have in certain very rare instances shown a knowledge, not to be explained by coincidence or natural foresight, of events still to come. These, summarily set out, are the facts; and on them Sir Oliver Lodge founds his doctrine that the medium is controlled by, and the messages come from, the surviving minds of persons who once lived on this earth and are now dead.

The argument is that we have certain facts not at present explicable: Sir Oliver Lodge offers an explanation, and claims that this ought to be accepted

unless we are prepared with another. He has hard words for the attitude of the man who admits the facts, or many of them, but cannot accept the theory, and is content to wait for further light. Now the logic of hypothesis is difficult, and has been much debated; but, at any rate, there can be no compulsion to accept a theory merely because it is the only one offered, unless the theory does fairly cover the facts as a whole. And this theory does not. It offers a kind of explanation of the central fact, that mediums show a knowledge which we cannot otherwise explain as yet. But when we ask how the theory is to be reconciled with all the circumstances, we are met simply with excuses we cannot expect to understand everything, the study of spiritualist phenomena is in its infancy; we have no right to expect things to fit in with our preconceived ideas (though this is just what we mean by a theory fitting the facts), and so forth. The truth is, the doctrine that these phenomena are due to the intervention of discarnate minds, far from explaining them, makes them more incomprehensible than ever. Is it really Raymond Lodge, the trained engineer, who tells Sir Oliver Lodge, the physicist, that a table tilts because it is full of magnetism, or that magnetism has to be stored up for a sitting, and therefore it is well always to use the same room and furniture? When the "spirit-control" calls itself Phinuit or Moonstone or Rector, talks broken English, says the departed are miserable because their living friends will not go to mediums, or dwells on the sitter's great name, strong personality, and wonderful influence is it more probably an authentic voice from beyond the grave or a deliverance of the medium's sub liminal consciousness? Is it valuable and valid information about the next world, when we learn that the people there live in houses built of bricks, "certain unstable atoms being drawn from the atmosphere and crystallised as they draw near a certain central attraction"; that there are dogs in that land, the ghosts of dogs that have lived on earth, that there are flowers, which "do not die and grow again but renew themselves," which are "like a pansy and not quite a pansy", that there are seven spirit spheres, one of them being called the astral plane and another Homeland or Summerland; that "the spirit-spheres are built round the earth plane, and seem to revolve with it. Only, naturally, the first sphere isn't revolving at such a rate as the third, fourth, fifth, sixth, and seventh spheres. Greater circumference seems to make it revolve more rapidly That seems to have an actual effect on the atmospheric conditions prevailing in any one of the spheres"—is this the kind of information from the other world that we all long for?

Does Sir Oliver Lodge in his inmost heart believe it himself? He tells us of foolish and wicked spirits disturbing the sittings, of spirits skylarking with mediums, and of messages so trivial that they would evaporate from his mind unless something occurred to give them importance. Think of it. your lost friend, whose every earthly word you treasure as a sacred memory, speaks to you from beyond the grave, from that land whose silence since the day that the first man died has been so complete and unbroken that "as silent as the grave" is our very similitude for the absolutely soundless; but, now, so commonplace and trivial is the stream of chatter from that hitherto silent land, that his words "evaporate from your mind"! Is it credible?

The attitude of the average thoughtful man towards all these super-normal manifestations is at present one of suspended judgment. He does not deny their existence, he admits their real interest and their possible importance; he is grateful to any competent observer who will impartially investigate them. But he is firmly convinced that their classification as super-normal is merely a temporary expedient, and that they will ultimately prove to be as natural, as amenable to law

and system, as the facts of magnetism and gravitation. This would not of itself exclude the spiritualist explanation; but in the proportion that his faith in the universe as rational is firm and abiding, he will hesitate before a hypothesis which gives such a depressing and unattractive picture of life after death, and which looks upon the souls of the departed as actively competing for attention with the fraudulent medium, and unable to provide us with any certain criterion to distinguish between them. Our own opinion is that to many who were previously in doubt and perhaps inclined to the spiritualist hypothesis, *Raymond* will bring an absolutely settled conviction that, whatever may be the real explanation of these phenomena, they have at any rate no connection with a future life or with the spirits of the departed. And as the true explanation of any phenomena is nearly always more wonderful and beautiful than the false hypotheses which preceded it and which it dethrones, so we may trust it will be in this case. Surely, if one did indeed rise from the dead and speak to us earthly men of his life behind the veil, he would tell us something, not fashioned after the vain imaginings of spiritualist or theosophist, but something utterly unexpected, yet so simple and probable, so convincing and compelling, that we should accept it at once, without doubt or hesitation, in virtue of its revealing self-evidence.

W. ARNISON SLATER.

BOOKS RECEIVED

(*Publishers are requested to notify prices*)

Differential Calculus. By H. B. Phillips, Ph.D., Assistant Professor of Mathematics in the Massachusetts Institute of Technology. New York: John Wiley & Sons; London: Chapman & Hall, 1916. (Pp. v + 162.) Price 5*s*. 6*d*. net.

X-Rays. By G. W. C. Kaye, M.A., D.Sc., Capt. R.E (T.), Head of the Radium and X-Ray Department at the National Physical Laboratory. Second Edition, with Illustrations. London: Longmans, Green & Co., 39, Paternoster Row, 1917. (Pp. xxi + 285.) Price 9*s*. net.

An Introduction to the Use of Generalised Co-ordinates in Mechanics and Physics. By William Elwood Byerly, Perkins Professor of Mathematics Emeritus in Harvard University. London: Ginn & Co., and Boston, New York, and Chicago. (Pp. vii + 118.) Price 5*s*. 6*d*.

Ozone: Its Manufacture, Properties, and Uses. By A. Vosmaer, Ph.D., Chemical and Electrical Engineer, Member of the American Institute of Electrical Engineers, Member of the Iron and Steel Institute (London). London: Constable & Co., 10, Orange Street, Leicester Square, W.C., 1916. (Pp. xii + 197.) Price 10*s*. 6*d*. net.

Annual Chemical Directory of the United States. Editor, B. F. Lovelace. Baltimore, U.S.A.: Williams & Wilkins Company. (Pp. 305.) Price $5.

Some Main Lines of Advance in the Domain of Modern Analytical Chemistry. A Lecture delivered before the Chemical Society on March 15, 1916, by A. Chaston Chapman. From the *Transactions of the Chemical Society*, 1917. Vol. III. (Pp. 203–20.)

Proceedings of the Yorkshire Geological Society. Vol. XIX. Part III. with 17 plates. Edited by W. Lower Carter, M.A., F.G.S. Issued March 1917. (Pp. 75–253.)

Carbon Assimilation. A Review of Recent Work on the Pigments of the Green Leaf and the Processes connected with them. By Ingvar Jorgensen and Walter Stiles. *New Phytologist* Reprint, No. 10. London: William Wesley & Son, 28, Essex Street, Strand. (Pp. 180.) Price 4*s*. net.

Memoirs of the Department of Agriculture in India: The Dissemination of Parasitic Fungi and International Legislation. By E. J. Butler, M.B., F.L.S., Imperial Mycologist, Agricultural Research Institute, Pusa. Botanical Series, Vol. IX. No. 1, February 1917. Printed and published for the Imperial Department of Agriculture in India by Thacker, Spink & Co., Calcutta. London: W. Thacker & Co., 2, Creed Lane. (Pp. 73) Price 2*s*. net.

Records of the Indian Museum. Vol. X. Part III. Nos. 9, 10, and 11. Studies in Indian Helminthology. No. 1. By F. H. Stewart. Calcutta: July 1914.

Studies in Insect Life and other Essays. By Arthur Everett Shipley, Sc.D., F.R.S., Master of Christ's College, Cambridge. London: T. Fisher Unwin, Adelphi Terrace. (Pp. xi + 338, with 11 illustrations.) Price 10*s* 6*d*. net.

The Study of Animal Life. By J. Arthur Thomson, M.A., LL.D., Regius Professor of Natural History in the University of Aberdeen. Revised Edition. London: John Murray, Albemarle Street, W., 1917. (Pp. xvi + 477, with 124 illustrations.) Price 6*s*. net.

This is the fourth and thoroughly revised edition of a well-known text-book. It is a simple introductory account of Zoology written in the author's well-known lucid style, and, having already been in use a quarter of a century, is assured of a new lease of life in its present form.

Genetics and Eugenics. A Text-book for Students of Biology and a Reference Book for Animal and Plant Breeders. By W. E. Castle, Professor of Zoology in Harvard University and Research Associate of the Carnegie Institute of Washington. Cambridge, U.S.A.: Harvard University Press; London: Humphrey Milford, Oxford University Press, 1916. (Pp. vi + 353, with 134 figures) Price 8*s*. 6*d*. net.

The Order of Nature. An Essay. By Lawrence J. Henderson, Assistant Professor of Biological Chemistry in Harvard University. Cambridge, U.S.A.: Harvard University Press; London: Humphrey Milford, Oxford University Press, 1917. (Pp. v + 234.) Price 6*s*. net.

The Method of Enzyme Action. By James Beatty, M.A., M.D., D.P.H., with Introduction by Prof. E. H. Starling, M.D., Sc.D., F.R.S. London: J. & A. Churchill, 7, Great Marlborough Street, 1917. (Pp. v + 143.) Price 5*s*. net.

The Causation of Sex in Man. A New Theory of Sex Based on Clinical Materials together with Chapters on Forecasting or Predicting the Sex of the Unborn Child and on the Determination or Production of either Sex at Will. By E. Rumley Dawson, L.R.C.P. Lond., M.R.C.S. England, late Member of the Council of the Obstetrical Society of London, and Fellow of the Royal Society of Medicine. Second Edition. London: H. K. Lewis & Co., 136, Gower Street, W.C., 1917. (Pp. xiv + 226, with 22 illustrations.) Price 7*s*. 6*d*. net.

Some years ago the author produced this book, which caused a good deal of discussion at the time. In view of the criticisms and of subsequent knowledge acquired, he has made several alterations and revisions in this present second edition without any weighty modification of any of the theories put forward previously.

On Growth and Form. By D'Arcy Wentworth Thompson. Cambridge: at the University Press, 1917. (Pp. xv + 793, with 408 illustrations.) Price 21*s*. net.

The Carnegie United Kingdom Trust. Report on the Physical Welfare of Mothers and Children: England and Wales, 1917. Vol. I. By E. W. Hope, M.D., D.Sc., Medical Officer of Health for the City and Port of Liverpool, and Professor of Public Health, University of Liverpool. (Pp. xvi + 434.) Vol. II. By Janet M. Campbell, M.D., M.S., one of the Senior Medical Officers, Board of Education. (Pp. viii + 190.) Liverpool: C. Tinling & Co., 53, Victoria Street.

The Secretion of Urine. By Arthur R. Cushny, M.A., M.D., LL.D., F.R.S., Professor of Pharmacology, University of London. Monographs on Physiology. Edited by Ernest H. Starling, M.D., D.Sc., F.R.S., F.R.C.P. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1917. (Pp. xi + 241, with diagrams.) Price 9*s*. net.

Lessons in Pharmaceutical Latin and Prescription Writing and Interpretation. By Hugh C. Muldoon, Ph.G., Instructor in Inorganic and Analytical Chemistry and Latin, Massachusetts College of Pharmacy, Boston. New York: John Wiley & Sons; London: Chapman & Hall, 1916. (Pp. vii + 173.) Price 6*s*. net.

"This book is the product of several years' experience in the teaching of 'Pharmaceutical Latin,' and is designed to present, in a simple and practical manner, such rudiments of Latin as will enable the pharmacist to interpret correctly those portions of the language which he may encounter in the practice of his profession." The book is well done.

Science and the Nation. Essays by Cambridge Graduates, with an Introduction by the Right Hon. Lord Moulton, K.C.B., F.R.S. Edited by A. C. Seward, F.R.S., Master of Downing College, Cambridge. Cambridge: at the University Press, 1917. (Pp. xxii + 328.) Price 5*s*. net.

National Defence. A Study in Militarism. By J. Ramsay Macdonald, M.P. London: George Allen & Unwin, 40, Museum Street, W.C. (Pp. 132.) Price 2*s*. 6*d*.

Janus and Vesta. A Study of the World Crisis and After. By Benchara Branford. London: Chatto & Windus, 1916. (Pp. xviii + 316.) Price 6*s*. net.

A book of much wisdom of the gentle and discursive character, containing many good things in the form of short essays on numerous themes, but scarcely suitable for detailed review. The book may, however, please those who have leisure to dip into its three hundred pages from time to time.

Proportional Representation. A Reply to the Pamphlets issued by the Executive Committee of the London Liberal Federation. By H. G. Chancellor, M.P., and Aneurin Williams, M.P. London: The Proportional Representation Society, 179, St. Stephen's House, Westminster, S.W. (Pp. 24.) Price 2*d*.

The Unseen Hand. In Five Parts. By Kirton Varley. London: The Generation Press, 1, Adelaide Street, Strand, W.C., 1917. (Pp. 126.) Price 1*s*. net.

Fool Culture. On Education, Culture, Misgovernment, and Destiny. By Kirton Varley. London: The Generation Press Co., 1, Adelaide Street, Strand, W.C. (Pp. 109.) Price 2*s*. net.

The Passing of the Great Race; or, the Racial Basis of European History. By Madison Grant, Chairman, New York Zoological Society; Trustee, American Museum of Natural History; Councillor, American Geographical Society. London: G. Bell & Sons, 1917. (Pp. xxi + 245.) Price 8*s*. 6*d*. net.

Poems of the Great War. Selected by J. W. Cunliffe, Professor of English and Associate Director of the School of Journalism of Columbia University in the City of New York. On Behalf of the Belgian Scholarship Committee. New York: The Macmillan Company, 1916. (Pp. xx + 297.) Price $1.50.

Seventy-Eighth Annual Report of the Registrar-General of Births, Deaths, and Marriages in England and Wales, 1915. Presented to both Houses of Parliament by Command of His Majesty. London: Published by His Majesty's Stationery Office, 1917. Cd. 8484. (Pp. lxviii + 516.) Price 5*s*. net

The Borderlands of Science. By A. T. Schofield, M.D. London: Cassell & Co., and New York, Toronto, and Melbourne, 1917. (Pp. viii + 255.) Price 6*s*. net.

Thomas Alva Edison. By Francis Rolt-Wheeler. New York: The Macmillan Co., 1915. (Pp. viii + 201, with 5 illustrations and frontispiece.) Price 50 cents.

The Preface of this book closes with these words: "That his (Edison's) life is a splendid stimulus to every American boy and girl, to every American man and woman, is the writer's belief, and to fling before the youth of the United States the heroic figure of America's greatest inventor in its most clarion appeal is the aim and purpose of the author." This exactly describes the contents, and the work is specially suitable for the juvenile reader of all countries. That such a biography as this should have been written during Mr. Edison's lifetime is the best testimony to the value of his work.

Food Gardening for Beginners and Experts. A new, simple, and scientific plan for growing food vegetables all the year round. By H. Valentine Davis, B.Sc., Lecturer in Rural Science and Gardening in the Cheshire County Training College, Crewe. London: G. Bell & Sons, 1917. (Pp. vii + 44.) Price 6*d*. net.

A well-timed publication designed more particularly for allotment holders, but suitable for any one who has a small amount of ground to cultivate. Essential knowledge, only, is given in such a form that the veriest beginner could make a success of his gardening, and in such a small compass that the plea of want of time would constitute no excuse for omitting to study the book carefully.

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By PHILIP E. B. JOURDAIN, M.A., Cambridge.

Various.—Among recent deaths of well-known workers in mathematics, we may notice the following: Sir W. D. Niven died on May 29, 1917, at seventy-five years of age, and Dr. W. H. Besant on June 2 in his eighty-ninth year.

The London Mathematical Society has just passed, without notice, its fiftieth year of activity. This fact, and some points in the early history of the Society, were mentioned, among other matters, in Sir Joseph Larmor's Address (*Proc. Lond. Math. Soc.* 1917, **16**, 1–7).

There has recently been a good deal written about the kind of mathematical typography which should be used to secure uniformity and clearness combined with the least unnecessary trouble possible for the printer. Some time ago the London Mathematical Society issued a leaflet on the subject, and the suggestions contained in it have been commented upon in *Nature* and the *Mathematical Gazette*. G. Peano, who has always paid great attention to typographical convenience in his mathematical and logical work—and this is the most striking of the external features in which his work differs from that of Frege—has lately (*Atti della R. Accad. delle Scienze di Torino*, 1915–16, **51**, 229–36) explained some methods of writing mathematical formulæ so that they may be easily printed.

Judging by a note in the *American Mathematical Monthly* for May, 1917 (p. 254), the Committee on the *Fortschritte* and the *Revue Semestrielle*, referred to in SCIENCE PROGRESS for July, has been able to make very little progress owing to the fact that the last numbers of the *Revue* have not reached America. At the same time it may be pointed out that there have been a great number of discussions, both in this country, in France, and in Italy, on the question of some common action by the nations of the Entente in the organisation of scientific literature, which has hitherto been almost entirely in the hands of Germany. We may refer to the letter of Prof. Eugenio Rignano in *Nature* of January 25 of this year, to another letter

in *Nature* of June 14, to an article in SCIENCE PROGRESS for April, to recent numbers of the *Revue générale des Sciences*, and to the account of a recent meeting of the Italian Association for the Advancement of Science in *Scientia* for June.

History.—A. E. Taylor (*Proc. Aristot. Soc.* 1916, **16**, 234–89) makes a most learned and important contribution to the history of Greek logical theory, by discussing the use made in Plato's *Parmenides* of the appeal to an infinite regress.

J. H. Weaver (*Bull. Amer. Math. Soc.* 1917, **23**, 357–65) gives a short historical sketch of the development of the properties of conics connected with the foci, and also some of the theorems from Pappus which have a bearing on foci and tangents.

F. Cajori (*Science*, 1916, **44**, 714–7) calls attention to the early use of a symbol for zero and the principle of local value by the Maya of Central America and Southern Mexico, which probably dates back to about the beginning of the Christian era, and was thus apparently the very earliest. The unit of that number system of theirs which is of the greatest interest was 20, for which a special symbol, which looks something like a half-closed eye (the symbol for zero) with a dot above it, was used, and separate symbols composed of dots and bars represented the numbers from 1 to 19, each dot representing a unit and each bar five units.

J. Ginsburg (*Bull. Amer. Math. Soc.* 1917, **23**, 366–9) gives an account of the evidence discovered by F. Nau and described in the *Journal Asiatique* of 1910 that the Hindu numerals reached Arab lands much earlier than was formerly supposed, since they were known to and justly appreciated by the Syrian writer Severus Sebokht, who lived in the second half of the seventh century. This is, then, about a hundred years before the date of the first definite trace that we have hitherto had of the introduction of the system into Bagdad. Also Nau's work shows that the zero was probably not in this system (cf. *ibid.* 467).

A critical review by F. E. Robbins of George Johnson's dissertation on *The Arithmetical Philosophy of Nicomachus of Gerasa* (Lancaster, Pa., 1916), which presents a partial translation of and a short commentary on the *Introduction to Arithmetic* of Nicomachus, is given in the *Amer. Math. Monthly* (1917, **24**, 121–3).

W. von Dyck (*Abh. der k. b. Akad. der Wiss. zu München*, 1914, 28) communicates some lately recovered correspondence of Kepler with Edmund Bruce.

In 1916 was the two-hundredth anniversary of the death of Gottfried Wilhelm Leibniz (1646–1716), and the number of the *Monist* for October, 1916, was almost entirely devoted to articles on various aspects of Leibniz's life and work. From the point of view of mathematics the most interesting are those on Leibniz's logical work (Philip E. B. Jourdain, *Monist*, **26**, 504–23), on Leibniz's dyadic system of numeration (Florian Cajori, *ibid.* 557–65), and a translation by J. M. Child (*ibid.* 577–629) of Leibniz's *Historia et Origo* and the cancelled part of a letter to Jacob Bernoulli of April, 1703. Child continues his translations of Leibniz's manuscripts written between 1673 and 1675 in the number of the *Monist* for April, 1917 (**27**, 238–94), from the well-known publications of Gerhardt of 1848 and 1855. It is to be noticed that the manuscripts are arranged with great care and some very useful notes are added.

The Address of Cajori on discussions of fluxions from Berkeley to Woodhouse, which was read before the Mathematical Association of America at the end of 1916, is printed in the *Amer. Math. Monthly* for April, 1917 (**24**, 145–54). This exceedingly important period of critical work on the foundations of the calculus has been hitherto not very well known, and yet the *Analyst* of Berkeley and the *Discourse* of Robins, as well as Newton's tract on the quadrature of curves and Maclaurin's *Fluxions*, are landmarks in the logical treatment of mathematics. It may be mentioned that Cajori's full researches on this subject are to be published after the war in the "Open Court Classics of Science and Philosophy."

Hk. de Vries (*Nieuw Tijdschrift voor Wiskunde*, 1915–6, **3**, 255–69) has a study of Gaspard Monge and the discovery of descriptive geometry.

L. Mongardon (*L'Intermédiaire des Mathématiciens*, 1916, **23**, 113) has a note relating to a bibliography of Wronski's works.

E. Lampe and R. Rothe, whose papers on Weierstrass and Weierstrass's *Werke* have been already noticed in Science Progress (1917, **11**, 617), have published papers which are presumably identical with those previously mentioned in the *Archiv der Math. und Phys.* (1916, **25**, 36–58 and 59–62).

The first volume of the *Œuvres de G. H. Halphen* was

published at Paris in 1916 under the editorship of C. Jordan, H. Poincaré, E. Picard, and E. Vessiot. The volume contains three biographical or bibliographical notices and thirty-nine memoirs of dates 1864 to 1876 (*Rev. Sem.* 1917, **25** [1], 82. A full account of the volume appears in *Bull. des sci. math.* 1916, **40**, 297–8).

The third volume of the *Opere Mathematiche* of Luigi Cremona was published at Milano in 1916. This volume contains a notice of Cremona's life and works.

An account of the life and work of Ernst Mach, who lived from 1838 to 1916, was given by G. Bavink in the *Unterrichtsblätter für Math. und Naturwiss.* (1916, **22**, 41–5). We may refer to the short notice given in SCIENCE PROGRESS (1917, **11**, 616).

Logic, Principles, and Theory of Aggregates.—Raphael Demos (*Mind*, 1917, **26**, 188–196) applies to particular negative propositions the treatment which Russell has applied to simple descriptive phrases or incomplete symbols and of which an account is given in the first volume (1910) of *Principia Mathematica*.

E. V. Huntington (*Bull. Amer. Math. Soc.* 1917, **23**, 276–80) establishes the "complete independence"—in the sense of E. H. Moore—of each of three different sets of postulates for serial order, due respectively to Vailati (1892), Huntington (1905), and Huntington (a new and convenient set). Huntington also (*ibid.* 280–2) gives three sets of independent postulates for well-ordered systems, each of these three sets being again "completely independent."

Huntington (*Amer. Math. Monthly*, 1917, **24**, 1–16) outlines in a compact form the logical structure of elementary dynamics. The fundamental concepts include "forces, as suggested by the tension and compression of our own muscles," and not mass. In fact, Huntington gives a system of derived units based on force, length, and time, instead of on mass, length, and time, and maintains the practical superiority of his system (cf. *ibid.* 296–300).

A. Natucci (*Periodico di Mat.* 1916, **18**, 220–34) has a paper on mathematical definitions and the concept of equality in logic, philosophy, and mathematics. Besides discussing such subjects as the homogeneity of definitions and definition by abstraction, the logical postulate of Burali-Forti, which comes

in for the purpose of reconciling the two definitions of finite classes, is treated.

R. J. Kortmulder has published (*De logische grondslagen der wiskunde*, Amsterdam, 1916) his doctorial thesis on the logical and philosophical principles of mathematics, more particularly arithmetic. Finite and transfinite numbers, both cardinal and ordinal, are considered. With this may be compared a paper by Kortmulder with the same title in *Wiskundig Tijdschrift*, 1915–6, **12**, 214–7).

K. Vorovka (*Časopis pro pěstování mathematiky a fysiky*, Prague, 1914, **43**, 154–62) writes on Henri Poincaré's judgment on the relations of mathematics to logic.

A. Korselt (*Jahresber. der Deutschen Mathematiker-Vereinigung*, 1916, **25**, 132–8) has a paper professing to give a solution of the paradoxes of Burali-Forti, Bernstein, Russell, and Richard.

F. Enriques (*Rev. de Métaphys. et de Morale*, 1917, **24**, 149–64) considers the question of whether there is any validity in some work on the infinite in mathematics. " Suppose that there is potentially given by thought an infinity of objects, the question is, is there any reason to consider as logically defined a new object of thought which expresses the totality or the limit, even when the objects spoken of are not constructed with reference to a concept of that kind which we suppose given *a priori*." This kind of " realism," which is chiefly due in a mathematical respect to Georg Cantor " and of which the philosopher B. Russell has developed in the widest sense the philosophical consequences," rests on the fundamental principle that " every infinity of objects virtually defined can be considered as a totality forming a class and constituting a new logical object." Modern work on mathematical logic has shown this principle to be in error in certain cases, and Enriques points out this defect of new realism. It seems to the reviewer, however, that this criticism wholly neglects the essential fact that since 1905 Russell has explicitly abandoned the supposition that there are such things as classes, and that a great part of Cantor's theory can be expressed without supposing that there are such things as classes at all.

Also the theory of Russell that we need not assume that there are any such things as " classes " is quite neglected in a paper by D. Mirimanoff (*L'Enseignement math.* 1917, **19**, 37–52). The solution suggested in this paper of the contradictions of

Russell and Burali-Forti is practically the same as that suggested by Jourdain in 1904 in which individuals were stated not to form a class (" manifold ") unless they could be arranged in a segment of the series W of all ordinal numbers. However, both Jourdain's solution and this one are rendered superfluous by Russell's theory just mentioned.

A new edition of E. V. Huntington's work, first published in 1905, on the *Continuum and other Types of Serial Order* has just been published (Cambridge, Mass., 1917).

Of recent papers on the theory of point-aggregates we may notice those by P. Mahlo (*Jahresber. der Deutschen Mathematiker-Vereinigung*, 1916, **25**, 163–208) on parts of the continuum of the same power as it, F. Hausdorff (*Math. Ann.* 1915–6, **77**, 430–7) in which he proves that a Borel's aggregate is either finite or enumerable or of the power of the continuum, P. Alexandroff (*Compt. Rend.* 1916, **162**, 323–5) on the power of measurable aggregates, W. Sierpinski (*ibid.* 629–32) on a Cantorian curve which contains a one-one and continuous image of any given curve, and the same author (*ibid.* 716–7) on a general property relating to the measure of the parts into which any point-aggregate whatever can be decomposed.

J. H. Kline (*Bull. Amer. Math. Soc.* 1917, **23**, 290–2) proves a theorem that if M is a domain and $G_1, G_2, G_3, \ldots$ is an enumerable set of nowhere dense closed sets of points, no one of which disconnects any domain, then $M - (G_1 + G_2 + G_3 + \ldots)$ is connected. This theorem, which contains as a special case a theorem which F. Hausdorff published in 1914, was proved by R. L. Moore in 1916 (cf. SCIENCE PROGRESS, 1916, **11**, 270) on the basis of a system of axioms proposed by him.

E. Wiechert (*Göttinger Nachrichten*, 1916, 124–41) shows that there are many ways of explaining the motion of the perihelion of Mercury apart from the " most radical theory of relativity," and that one way of doing so is particularly simple and corresponds with modern physical views. E. Papperitz (*Jahresber. der Deutschen Mathematiker-Vereinigung*, 1916, **25**, 84–95) tries to show that classical physics is quite able to explain Michelson's experiment. Further, L. Silberstein read to the Royal Astronomical Society on April 13, 1917 (*Nature*, 1917, **99**, 159), a paper giving a deduction from the classical theory, as opposed to Einstein's most recent " generalised theory of relativity," of the motion of the perihelion of Mercury.

Arithmetic and Algebra.—H. W. Stager has published *A Sylow Factor Table of the First Twelve Thousand Numbers giving the Possible Number of Sylow Sub-Groups of a Group of Given Order between the Limits of* 0 *and* 12,000 (Washington, 1916), and there is a short account of it in *Nature* (1917, **99**, 164).

G. A. Miller (*Bull. Amer. Math. Soc.* 1917, **23**, 283–7), continuing an article of his published in 1910, considers the groups generated by two operators of the same prime order such that the conjugates of one under the powers of the other are commutative.

Olive C. Hazlett (*Annals of Math.* 1916, **18**, 81–98) considers certain rational, integral invariants of nilpotent algebras, and proves theorems analogous to theorems about invariants of algebraic forms, and in particular their finiteness.

Analysis.—A review of W. B. Ford's *Studies in Divergent Series and Summability* (New York, 1916), which was mentioned in the last number of SCIENCE PROGRESS (1917, **12**, 12), was given by C. N. Moore in the *Bull. Amer. Math. Soc.* (1917, **23**, 308–14). Like most of the reviews in this *Bulletin*, it is very full and well done and contains criticisms and a list of misprints which must be very useful to the author and his readers.

S. A. Joffe (*Quart. Journ. Math.* 1916, **47**, 103–26) verifies the calculations of J. W. L. Glaisher for the first twenty-seven Eulerian numbers and gives computations for five more numbers of the series. Joffe's calculations were made from central differences of zero.

We have been awaiting, since its announcement in the *Encyklopädie* in 1898, the publication of A. Pringsheim's *Vorlesungen über Zahlen- und Funktionenlehre.* The first two parts of the first volume were published at Leipzig and Berlin in 1916, and an account of their contents is given in the *Rev. sem.* (1917, **25** [1], 82).

O. Szász (*Math. Ann.* 1915–6, **77**, 482–96) finds conditions that a sequence of powers of a variable approximates to any given continuous function of that variable.

A. Denjoy (*Journ. de Math.* 1915, **1**, 105–240) has a long memoir on the derivatives of continuous functions, and a note (*Compt. Rend.* 1916, **162**, 377–80) on derivation and its inverse calculus. Mrs. (Grace Chisholm) Young (*ibid.* 380–2) writes on the derivatives of a function.

G. Vitali (*Atti della R. Accad. delle Scienze di Torino*, 1915–6,

51, 137–41) writes on the theorems of the mean and of Rolle.

W. H. Young (*Compt. Rend.* 1916, **162**, 909–12) has a note on the foundations of the theory of integration, and N. Lusin (*ibid.* 975–8) has one on the finding of primitive functions, giving the results of his Russian thesis on integrals and trigonometric series.

W. H. Young (*ibid.* **163**, 187–90) proves a theorem on the convergence of Fourier's series.

T. W. Chaundy (*Mess. of Math.* **45**, 115–9) gives a condition for the validity of Taylor's expansion.

G. Mittag-Leffler (*Sitzungsber. der k. b. Akad. der Wiss. zu München*, 1915, 419–24) gives a new and simple proof of the theorem of Serge Bernstein on the necessary and sufficient conditions that a function of a real variable should be analytic along a certain straight line. G. Fubini (*Atti della R. Accad. delle Scienze di Torino*, 1915–6, **51**, 538–40) also has a note on the theorems of Bernstein and Pringsheim on development in Taylor's series. M. Riesz (*Acta Math.* **40**, 337–47) proves Bernstein's theorem by means of Lagrange's interpolation-formula; and also gives (*ibid.* 349–61) a theorem of convergence for Dirichlet's series.

F. Schottky (*Journ. für Math.* **146**, 234–44) gives a historical sketch of the work of Green, Cauchy, Riemann, Goursat, and Moore on Cauchy's integral, and also simplifications and generalisations of Moore's method.

Short notes on Poisson's integral considered as a direct consequence of Cauchy's integral were given by V. Laska (*Casopis pro pěstování mathematiky a fysiky*, Prague, 1913, **42**, 398–401) and K. Petr (*ibid.* 556–8).

J. L. Walsh (*Annals of Math.* 1916, **18**, 79–80) establishes Cauchy's integral-formula by means of the theorem called "the mean value theorem for harmonic (or conjugate) functions." This process is the reverse of the one used in, for example, Burkhardt's *Funktionentheorie* by the use of essentially the same analytic machinery. The proof is one given by M. Bôcher (*Bull. Amer. Math. Soc.* 1894–5, **1**, 206–7) in a paper on Gauss's third proof of the fundamental theorem of algebra.

G. Kowalewski (*Sitzungsber. der k. Akad. der Wiss. in Wien*, 1915, **124** [IIa], 333–8) develops at length the proof of the existence of implicit functions which was indicated in his book

Die klassichen Probleme der Analysis des Unendlichen (Leipzig, 1910).

A. Pringsheim (*Sitzungsber. der k. b. Akad. der Wiss. zu München*, 1915, 387–400) gives an elementary proof of Weierstrass's product theorem for whole transcendental functions, and shows how Weierstrass's method of making his products convergent can also serve to derive a criterion for conditional convergence of infinite products and for determining the variation of value of such a product when the order of the factors is changed.

G. Valiron (*Bull. de la Soc. Math. de France*, 1916, **44**, 45–64) has a paper on the rate of growth (*croissance*) of the maximum modulus of series having the character of a whole function, in which he uses a method given by him in 1913.

Dunham Jackson (*Annals of Math.* 1916, **17**, 172–9) shows that in certain cases a function of several complex variables analytic except for certain non-essential singularities can be expressed as the quotient of two functions analytic in all the variables.

T. H. Gronwall (*ibid.* 1916, **18**, 70–73) establishes two of the most important properties of the function log $(1 + z)$ from the point of view of function-theory by actually performing by simple methods the analytic continuation of the power series used to define this function. It may be noted that Gronwall (*ibid.* 65–9) deals with a problem in geometry connected with the analytic continuation of a power series. Gronwall gave in 1916 a translation, with notes, of J. W. L. V. Jensen's elementary exposition of the theory of the Gamma function (*ibid.* **17**, 124–66), and later (*ibid.* 1916, **18**, 74–8) he extended to the boundary of the region of convergence the discussion given by Jensen of Binet's factorial series for $\log \Gamma(s)$ and $\psi(s)$.

P. Appell (*Acta Math.* **40**, 291–309) collects his investigations on Theta functions of the fourth degree.

W. L. Hart (*Annals of Math.* 1916, **18**, 99–104) treats trigonometric series in which the arguments of the sines and cosines in the various terms are $2\pi a_k t$, where the numbers a_k are not necessarily commensurable, so that such series are generalisations of Fourier series. When the numbers last mentioned are known as well as the function $f(t)$ represented, as they are by observation or possibly theory in problems in applied mathematics, it is shown, under certain hypotheses as to the

convergence of the series, how to compute the coefficients. It is then shown that, if there exists one expansion for $f(t)$ satisfying certain conditions, this expansion is unique.

Some years ago A. Hurwitz showed how to calculate Fourier's constants of the product of two functions from those of the functions themselves. W. W. Küstermann (*Amer. Journ. Math.* 1917, **39**, 113–22) solves the corresponding problem for multiple Fourier's series. The work is carried through for double series only, but the results and proofs admit of obvious generalisation to n-tuple series.

F. Tricomi (*Giorn. di Mat.* 1916, **54**, 1–9) proves a theorem on the convergence of sequences formed by successive iterations of a function of a real variable.

C. A. Fischer (*Bull. Amer. Math. Soc.* 1916, **23**, 88–90) finds an interesting theorem on the representability of a linear function of a line where the function has continuity of the nth order.

In 1914 Fischer published a paper in which he defined the derivative of a function of a surface and proved some important theorems about such derivatives. He now (*Amer. Journ. Math.* 1917, **39**, 123–34) considers functions depending, not only on the surface, but also on the values taken by a function at every point of the surface : such a function has two partial functional derivatives. Similar work for functions of lines was published by Levy in 1914.

Mary Evelyn Wells (*ibid.* 163–84) discusses the inequalities found by E. H. Moore in the theory of the general linear integral equation. These inequalities include an analogue of the inequality of Schwarz in the theory of Fredholm's linear integral equations.

Geometry.—In 1912 E. Kasner published some results of his study of the invariants of a pair of analytic curves under the "equilong" groups with the main object of throwing light on the corresponding question in the more important conformal geometry. The two theories present many analogies, but are not connected by a strict principle of duality. In some questions the two theories differ essentially both in the appropriate methods and the results ; and in the paper mentioned the convergence of the power series entering into the formal calculations was not proved. Recently Kasner (*Bull. Amer. Math. Soc.* 1917, **23**, 341–7) completed the equilong theory by showing

that the series in question are always convergent. It thus follows that the equality of the absolute invariants is a sufficient as well as a necessary condition for the equivalence of two pairs of curves. The method used is one impossible in the conformal theory, and is to reduce the question to one in differential equations and then to apply certain existence theorems, due to Briot and Bouquet, for solutions at a singular point.

A. L. Miller (*ibid.* 292) obtains the theorem, analogous to Pascal's theorem, that if a decagon be doubly inscribed in a cubic the remaining ten intersections of the odd sides with the even ones lie on a conic.

L. P. Eisenhart (*Rend. del Circ. mat. di Palermo,* 1916, **41**, 94–102) shows that right conoids and cylinders are the only surfaces which can be generated by the motion of an invariable curve whose points describe straight lines.

M. H. Sznyter (*Amer. Math. Monthly,* 1917, **24**, 113–9) establishes for the pentahedroid in a four-dimensional space metrical theorems similar to those found in ordinary solid geometry dealing with the tetrahedron.

In connection with papers of some years ago on the projective differential geometry of curved surfaces by E. J. Wilczynski, F. M. Morrison (*Amer. Journ. Math.* 1917, **39**, 199–220) has a paper on the relation between some important notions of projective and metrical geometry. He studies metrically for the first time certain configurations associated with a point on a surface which had been defined and investigated by Wilczynski from a projective point of view. By means of these configurations Morrison defines some new classes of special points on a surface and some new kinds of surfaces, and applies these notions to the minimal surface of Enneper.

ASTRONOMY. By H. SPENCER JONES, M.A., B.Sc., Royal Observatory, Greenwich.

Solar Prominences.—The publication of an important memoir by Mr. and Mrs. John Evershed dealing with solar prominences (Results of Prominence Observations, *Memoirs of the Kodaikanal Observatory*, vol. 1, pt. ii. pp. iv. + 72, with 11 diagrams and 12 plates) provides a convenient opportunity to review briefly our present knowledge of solar prominences, the more so as much of that knowledge is due to the observations of Mr. Evershed, first in his private observatory at Kenley

in Surrey from 1890–1906, and since then as director of the Kodaikanal Observatory. The results of the whole series of his observations and of those taken at Kodaikanal previous to his becoming director are discussed in this memoir

It is well known that sun-spots, with few exceptions, occur only between heliographic latitudes 15° and 35°, the mean latitude varying with the phase of the solar cycle. At the commencement of a new cycle of activity, small spots begin to appear in the high latitudes of the spot zones. As the cycle progresses the mean latitude decreases, and towards its end the spots die out near the equator. Rather different results hold for prominences; there are two distinct belts both north and south of the equator in which prominences occur predominantly. The low-latitude belts are in the same latitudes as the sun-spot zones, and vary in activity in a similar manner, drawing in towards the equator and gradually dying out as the cycle progresses. The high-latitude zones decrease in activity at sun-spot minimum, but do not then disappear, and as the solar activity increases they move towards the poles and die out there at about the time of sun-spot maximum. These prominences obviously cannot be connected in any way with sun-spots, and it is found that the majority of those in the low-latitude belts also have no connection.

Prominences vary enormously in shape, size and behaviour. Mr. Evershed has classified them into broad groups and finds that groups with certain characteristics are almost invariably associated with sun-spots, whilst those with certain other characteristics rarely ever are. Prominences in the form of rockets, small bright jets and arches, are usually, and metallic prominences (*i.e.* those whose spectra contain metallic lines) are almost always associated with spots. Young and active spots are most commonly associated with jets; small old spots are usually unaccompanied by prominences.

Large massive forms, long groups of prominences, pyramids and columns appear to be associated rarely, if ever, with sun-spots; these are the prominences which in spectroheliograms taken with calcium or hydrogen light appear as long, narrow, dark "filaments" projected on the disc. They are usually long-lived and may reappear for several solar rotations. It has been doubted whether filaments are actually the projec-

tions of prominences, but Mr. Evershed's observations leave little room for doubt; when a filament of wide extent is disappearing at the limb, the latitude of the prominence observed agrees with that of the end of the filament and the two change together.

Magnetic storms on the earth are most frequent when prominences reach the poles. As this, however, occurs at the time of sun-spot maxima, the two phenomena cannot be directly correlated. It can only be said that both are indications of great solar activity. It seems, however, that a sun-spot may only be definitely assigned as the cause of a magnetic storm when associated with an eruptive prominence. It is interesting to note also that the solar corona at the stage in solar activity when prominences reach the poles contains long streamers right round the disc, whilst when the high-latitude prominence zones die away there are only small tufts at the poles.

It was first remarked by Mrs. Maunder that sun-spots consistently predominate on the eastern hemisphere of the sun as compared with the western, and this was tentatively attributed to an extinguishing effect of the earth on solar activity. The same result is found to hold for prominences, and not only are the prominences on the east more numerous, but they are also more active and denser. The only cause to which it seems possible to attribute this is an earth influence, and the presence of such is apparently supported by the fact that the line-of-sight movements in prominences are more frequently away from the earth than towards it. The remarkable fact is that no influence of any other planet can be detected. The eastern preponderance recently has not been quite so marked, but for thirty years was very persistent; it remains to be seen whether there is a gradual oscillation or not. Another result found to hold both for sun-spots and prominences is a greater activity in the southern hemisphere than in the northern.

There is not much information available as to the speed of rotation of prominences. The indications at present are that they rotate at a faster rate than the chromosphere, and that the speed of rotation is slower in high latitudes than in low; the first result is supported by a determination of the speed of rotation of the corona made by Bosler from plates

taken at the 1914 eclipse, his value being still greater than the value found for prominences.

The study of motion in prominences is being developed, and has already yielded important results. The rocket type of sun-spot prominence appears to be caused by an intermittent explosive force in sun-spots, the matter falling back again on to the sun's surface. The eruptive prominences indicate the presence of powerful forces which may last for several hours, and which apparently neutralise gravity, for the matter is not seen to descend, but fades away at a great height. In *Kodaikanal Obs. Bull.* lv. 1917, Mr. Evershed describes a remarkable prominence which fortunately was observed both at Kodaikanal and by him in Kashmir, a complete series of observations being obtained. This prominence was the highest ever observed, reaching a height of 18′·5, or over half a million miles, before fading away. The motion was accelerated and radial from the base of the prominence, the greatest velocity being 457 kms. per sec.

Observation appears to indicate that prominences, even the largest, are always very tenuous. If this is so, they cannot possess a temperature in the ordinary sense, but are luminous on account of the absorption of solar radiation. Mr. Evershed indicates as the most important problems connected with prominences which await solution, the causes and nature of their changes in density and luminosity, and of their differences in these respects, and a knowledge of their average density.

The memoir is illustrated with numerous fine reproductions of photographs of typical prominences.

The following notes refer to some of the more important papers recently published :

History.—Knobel, E. B., On Frederick de Houtman's Catalogue of Southern Stars, and the origin of the Southern Constellations, *M.N.*, *R.A.S.*, **77**, 414, 1917.

Dreyer, J. L. E., On Tycho Brahe's Catalogue of Stars, *Observatory* **40**, 229, 1917.

The Solar System.—Lau, H. E., Die periodischen Veränderungen auf dem Mars, I. Teil, *Ast. Nach.* 4878–9 ; II. Teil, *Ast. Nach.* 4884–5 ; III. Teil, *Ast. Nach*, 4889–90, 1917.

Three lengthy papers dealing with observations, past and present, of the surface-markings of Mars.

NICHOLSON, S. B., The Ninth Satellite of Jupiter, *Proc. Nat. Acad. Sci.*, **3**, 417, 1917. The orbit is given, and from the magnitude and probable albedo the diameter is found to be about 15 miles.

STRUVE, G., Neue Bestimmung des Saturnsäquators und der Bahnlageelemente der inneren Saturnstrabanten, *Ast. Nach.* 4880, 1917. Observations by H. Struve at Poulkowa and more recent observations at Washington and Babelsberg are combined to deduce a definite position for Saturn's equator. The result agrees well with that obtained by H. Struve.

PICART, L., Sur l'Application de la Méthode de Lagrange-Cauchy au Calcul d'une Orbite définitive, *Bull. Astron.* **34**, 135, 1917. To determine an orbit from three observations at widely separated epochs the methods of Lagrange and Gauss are not suitable. The only method is that of Lagrange-Cauchy; this method has been adapted for calculation by Picart and the process of computation is illustrated.

SLIPHER, V. M., Spectral Evidence of a Persistent Aurora, *Lowell Obs. Bulletin*, 76, 1916. By spectroscopic evidence a permanent illumination of the night-sky by the aurora is proved.

HOUGH, S. S., A Determination of the Constant of Aberration, *M.N., R.A.S.* **77**, 484, 1917. The value is deduced from observations made with the Cape reversible transit-circle, using Gill's very stable meridian marks. A programme for obtaining the constant with greater weight is sketched out.

Orbits of Binary Systems.—RUSSELL, H. N., The Orbit of Equulei, *Ast. Journ.*, 710, 1917. The orbit of Krueger 60, *Ast. Journ.* 711, 1917.

COMSTOCK, G. C., The orbit of ζ Herculis (Σ 2084), *Ast. Journ.* 712, 1917.

YOUNG, R. K., The orbit of the spectroscopic binary, 2 Sagittæ, *Journ. R.A.S.C.* **11**, 127, 1917.

MCDIAMID, R. J., The Eclipsing Variable Star SS Camelopardalis, *Astroph. Journ.* **45**, 50, 1917.

The first two are of importance because the stars have reached positions in their orbits in which they are in rapid relative rotation, and the measures of the next few years should give very accurate orbits. The third is of interest because

no orbit represents the observations satisfactorily; there are either systematic errors in them or there is a third disturbing body. It is shown that discrepancies are well represented on the latter hypothesis.

Stellar Clusters.—SHAPLEY, HARLOW, Studies based on the Colours and Magnitudes in Stellar Clusters:

I. The General Problem of Clusters.
II. 1,300 Stars in the Hercules Cluster (Messier 13).
III. A Catalogue of 311 Stars in Messier 67.

These papers form respectively *Mt. Wilson Observatory Contributions*, Nos. 115, 116, 117; they are abridged together in *Astroph. Journ.* **45**, 118, 1917.

IV. The Galactic Cluster, Messier 11; *Mt. Wilson Conts.* 126, 1917; *Astroph. Journ.* **45**, 164, 1917.
V. Further Evidence of the Absence of Scattering of Light in Space, *Proc. Nat. Acad. Sci.* **3**, 267, 1917.
VI. The Relation of Blue Stars and Variables to Galactic Planes, *Proc. Nat. Acad. Sci.* **3**, 276, 1917.

It should be noted that the *Proc. Nat. Acad. Sci.*, Washington, contains only brief accounts of researches, the full details of which are published subsequently elsewhere. The full details of the last two papers have not yet been published. The above important series of papers will be reviewed together when completed. Connected with them is the following:

PEASE, F. G., and SHAPLEY, HARLOW, On the Distribution of Stars in Twelve Globular Clusters; *Mt. Wilson Conts.* 129, 1917; *Astroph. Journ.* **45**, 225, 1917.

Stellar Distributions.—GYLLENBERG, W., Distribution in Space of Stars of Spectral Type O, *Arkiv. för Math. Astron. och. fysik k. Svenska-vetensk* **11**, No. 28, 1917; *Lund Medd.* 75, 1917.

MALMQUIST. K. G., The Distances and the Velocity Distribution of the Stars of the Spectral Type A, *Arkiv. k. Svensk.* etc., **11**, No. 29, 1917; *Lund Medd.* 76, 1917.

LUNDALL, C. F., Preliminary Note on Distances and Velocities of Stars of Spectral Type F., *Arkiv. k. Svensk.*, etc., **11**, No. 30, 1917; *Lund Medd*, 77, 1917.

These are three similar papers by students of Prof. Charlier,

discussing by his method the distribution in space of these special type stars and finding the positions of the solar vertex and apex relative to them.

Dyson, F. W., A Statistical Discussion of the Proper Motions of the Stars in the Greenwich Catalogue for 1910, *M.N., R.A.S.* **77**, 212, 1917.

Stellar Photometry, Seares, F. H., Photographic Magnitudes of Stars in the Selected Areas of Kapteyn, *Proc. Nat. Acad. Sci.* **3**, 188, 1917. Preliminary Note on Distribution of Stars with respect to the Galactic Plane. *Proc. Nat. Acad. Sci.*, **3**, 217, 1917.

The first paper describes the method of determining and standardising the magnitudes. The second gives the result of the determination from counts of these magnitudes of the galactic condensation. A value much higher than the recent determination of Chapman and Melotte is obtained, and it is concluded that their counts omitted many of the fainter stars in the richer fields.

PHYSICS. By James Rice, M.A., University, Liverpool.

For some time past a considerable body of mathematicians have been engaged on a careful scrutiny of the concepts and axioms on which rests the whole body of mathematics, and in the March number of the *Physical Review* Dr. R. C. Tolman suggests that the time is ripe for a similar investigation in physics. As any branch of science develops, its considerations become more and more deductive, the ideal form of exposition being one in which all the important relations are derived from a small number of independent general principles and all the terms used are defined with the help of a small number of indefinables, which are certain entities not defined in the particular discussion undertaken, but assumed to be matters upon which for the purposes at hand there is general agreement. Dr. Tolman proceeds to discuss the nature of the quantities which occur in the equations of mathematical physics and to consider a set of indefinables for their definition in the hope that he may thereby help in the preparation for that more complete systematisation of mathematical physics which is undoubtedly coming.

Following Bertrand Russell, the author considers *magni-*

tudes as indefinable *terms* which are capable of entering into the *relations* of "*greater than*" and "*less than.*" Every magnitude bears a peculiar relation to some particular concept such that we say it is the magnitude of that concept; magnitudes are said to be of the same kind when they bear this relation to the same concept. Only magnitudes of the same kind can enter into the relations of greater and less. Thus we can speak of magnitudes of volume and magnitudes of temperature, and we can say that one magnitude of the concept volume is greater or less than another magnitude of volume, but cannot relate it in this way to a magnitude of the concept temperature.

Quantity is a magnitude which has been particularised by the specification of spatial or temporal conditions, so that, for instance, the statement of the temperature in a given beaker at a given time is a quantity. One quantity is greater than another quantity, when the magnitude of the former is greater than the magnitude of the latter. To speak of *equal* magnitudes would be meaningless; but we can speak of equal quantities—they are quantities having the *same* magnitude.

No reasonable ambiguity arises if we place the different kinds of quantities used by physicists in two general classes—those having *extensive* magnitude and those having *intensive* magnitude. The first class contains those quantities which have a certain additive nature so that a given quantity can be regarded as being the sum of a number of smaller quantities of the same kind; the second class contains those which have no such additive nature. Thus volume is typical of quantities of the first class; likewise mass, energy, entropy. We can decide that such quantities have extensive magnitude by the consideration that the simultaneous presence of two systems, each having a definite quantity of the kind in question, gives a larger system with twice the quantity. On the other hand, density, temperature, permeability, inductivity, are quantities having intensive magnitude. For instance, the consideration of two pieces of platinum at 100° C. gives a larger piece of platinum at 100° C., not 200° C. This classification may turn out to be ambiguous; it is, however, simple and would still have great value even if there should happen to be borderline quantities of such a nature that it was hard to apply the criterion.

In measuring quantities of the first class we may for this

purpose distinguish two groups. In the first group will appear those quantities which can be regarded as composed of a finite number of distinct and identical parts, one of these parts being naturally chosen as a unit of measurement, which will then consist in a process of *counting.* Quantities of this kind are often used in the natural sciences, as in the statement of a number of petals in a given species of flower, in the correlation of the pressure of a gas with the number of molecules it contains ; they are associated with a *discrete* series of magnitudes which can be put into a one-to-one correspondence with the *ordinal* numbers. The second group of quantities having extensive magnitude contains those which cannot be regarded as the sum of a finite number of parts ; in this case the magnitudes of a given kind form a continuous series and can only be put into a one-to-one relation with the whole series of real numbers. Volume is an example, as it does not accord with our present ideas of space to consider a volume as composed of a finite number of parts which could not be further subdivided.

The measurement of quantities of the second class, viz. those having intensive magnitude, must be effected by some device in which the magnitudes to be measured are put into a one-to-one correspondence with a series of quantities having extensive magnitude. Thus, in the case of temperature, we correlate magnitudes of temperature with lengths of a mercury thread, volumes of a gas, etc.

Nearly all the quantities of different kinds used by the physicist are capable of being defined in terms of the few remaining ones, with the aid of certain other indefinables which consist in the operations " multiplied by," " divided by," etc. Thus a quantity of velocity is regarded as a quantity of length divided by a quantity of time ; a quantity of area is a quantity of length multiplied by a quantity of length ; and we have the well-known dimensional symbols to represent this relation of derived to fundamental quantities in as succinct a manner as possible.

In choosing a set of fundamental kinds of quantity it is desirable to keep in mind the following considerations : the number of kinds shall be sufficient and not redundant ; all the kinds chosen shall have *extensive* magnitude and further the requirements of simplicity.

It appears, as was pointed out by Rücker thirty years ago,

that we need for the present well-established body of physics five kinds of fundamental quantity, the ones usually chosen being those of length time, mass, permeability and temperature. The reason for the sufficiency and necessity of five seems to be the fact that physics is at present considering five fundamentally different kinds of "thing," viz. time, space, matter, electricity and entropy—the latter being the "degree of run-downness of a system." We can, in fact, arrange the physical sciences in a hierarchy such that each successive member introduces the consideration of one additional kind of "thing." Thus geometry introduces space; kinematics introduces, in addition, time; mechanics, matter; electrodynamics, electricity; thermodynamics, entropy.

If we accept as a criterion for the choice of fundamental quantities that they should have extensive magnitude, we will have to reject temperature and permeability (or dielectric inductivity) from the customary list. The desirability of this criterion consists in the fact that we can then measure all our fundamental kinds of quantity by a simple process of fitting unit quantities together until we reach the magnitude of the quantity we are measuring; it would accord with the criterion of simplicity also on the ground that we have to measure all quantities having intensive magnitude by correlation with some quantity having extensive magnitude.

On these grounds Dr. Tolman then suggests that we should employ as fundamental quantities, length, time interval, mass, quantity of electric charge and entropy. No statements are needed to justify the choice of the first three. As regards the fourth, it is superior to the usual choice of permeability not only because it has extensive magnitude, but also because of greater psychological simplicity and a more direct relation to the fundamental "thing," electricity. It is interesting to note that experimental work seems to confirm the view that electric charge is in the first group of quantities having extensive magnitude—that is, it has *discrete* magnitude, and the most natural way of measuring a charge would be based on the *counting* of electrons.

Entropy, although justifiable on the ground that it has extensive magnitude, may not be justifiable on grounds of simplicity, since temperature certainly seems to most people the simpler idea. Entropy, however, does bear a simple rela-

tion to the kind of "thing" considered in thermodynamics, which is the degree to which a system has approached a state of quiescence or equilibrium, *i.e.* its "degree of run-downness," as Dr. Tolman calls it.

In addition to these fundamental quantities, we have also certain indefinable *operations* which enter into the definition of derived quantities; they are five in number, multiplication, division, differentiation and the two types of vector multiplication leading to the scalar and vector products of two vectors.

Dr. Tolman then proceeds to build up a table of derived quantities, using the well-known dimensional symbolism. Many of these are already commonplaces, but a few which bring out his particular choice of fundamentals may be given. Using e and S as shorthand symbols for electrical charge and entropy we have, for example:

Potential	$[ml^2 t^{-2} e^{-1}]$
Current	$[lt^{-1} e]$
Resistance	$[mlt^{-1} e^{-2}]$
Electric Field Strength	$[mlt^{-2} e^{-1}]$
Magnetic Field Strength	$[l^{-1} t^{-1} e]$
Magnetic Pole Strength	$[ml^2 t^{-1} e^{-1}]$
Permeability	$[mle^{-2}]$
Magnetic Induction	$[mt^{-1} e^{-1}]$
Temperature	$[ml^2 t^{-2} S^{-1}]$
Specific heat	$[m^{-1} S]$

Future developments of science—not merely physical science, but other branches which may come into the field of mathematical analysis—may require us to increase the number of fundamental kinds of quantity; thus we may shortly have to separate gravitational mass from ordinary mass as a quite different kind of thing, whose accidental proportionality to it in materials hitherto experimented upon has disguised an essential dissimilarity. Also the desirability of looking at phenomena from a new point of view may cause us to add to the number of fundamentals, just as already the desirability of viewing thermal phenomena from the statistical and microscopic standpoint rather than from the molecular, kinetic and microscopic point of view has led to the introduction of entropy. There appears to be little hope, however, of a decrease in the number of fundamental kinds of quantity.

PHYSICAL CHEMISTRY. By Prof. W. C. McC. Lewis, M.A., D.Sc., University, Liverpool.

Thermal and Photochemical Change.—Although very numerous reactions are known, which belong, on the one hand to the thermal type, and on the other to the photochemical type, very few are known in which the chemical change takes place at a measurable rate thermally as well as photochemically. Hydrogen peroxide decomposition is an instance of the latter behaviour; the oxidation of sodium sulphite is another case in point, and as this reaction possesses particular interest in view of the effects of various catalysts upon it, both positive and negative, it is proposed to consider the reaction briefly.

Bigelow (1) in 1898 carried out a very extensive investigation of the influence of catalysts upon the reaction occurring in aqueous solution at 20° C., the reaction proceeding thermally. Numerous difficulties were encountered, into the details of which it is impossible to go, in connection with the non-reproducibility of many of the results. For this reason many of the numerical values possess a relative and not an absolute significance. Marked effects were finally discovered to be attributable to the solvent, the water itself. Exactly analogous behaviour, it may be remarked, has been observed in the decomposition of hydrogen peroxide. By working with a given stock of water, however, it was possible to examine systematically the influence of a number of substances upon the rate of the reaction. All the organic substances examined by Bigelow exerted a negative catalytic effect, that is to say, they diminish the rate of the reaction. Ketones and esters of the aliphatic series were found to exert an almost inappreciable effect. On the other hand, benzyl alcohol, benzaldehyde, butyl alcohol and other alcohols, mannitol, aniline and other substances exert a marked anti-catalytic effect, benzyl alcohol being the most remarkable in this respect. The striking fact in connection with these catalysts is the minute amount which is capable of producing a measurable effect. Thus in the case of mannitol, a quantity so small that the solution was only 1/160000 normal with respect to the catalyst produced a detectable retardation. In a particular case in which there were 800 moles of sodium sulphite to one mole of mannitol, the velocity was reduced to one half of its value in water alone. It is inconceivable that such effects as these are brought about

merely by a diminution of the solubility of the oxygen in the solution. The actual mechanism is not at all understood. Such results remind one of heterogeneous catalysis, especially the catalytic effects of colloidal metals which can be poisoned by minute amounts of foreign material. In the present case, however, we are apparently dealing with homogeneous systems in which the degree of dispersion has attained the molecular limit. No suggestion as to a possible mechanism is made by Bigelow.

Bigelow's work was extended by Young (2), who showed that negative catalytic effects are produced by alkaloids upon the oxidation of sodium sulphite and the oxidation of stannous chloride.

The sodium sulphite reaction was taken up later by Titoff (3), the reaction being again carried out under thermal conditions. Titoff discovered a number of positive catalysts for the reaction, chiefly inorganic salts. In this connection copper sulphate occupies a unique position in virtue of its strong positive effect even at extreme dilution, the influence of one gram-molecule of the salt in ten million litres being measurable. Approximate proportionality was found to exist between the increase in speed and the concentration of the copper salt. The catalytic effect is attributed to the copper ion. When mannitol and copper sulphate are present simultaneously, the rate of the reaction depends upon the relative amount of each ; the net effect indicates mutual influence. Titoff also found that stannic and stannous chlorides exert the greatest negative catalytic influence so far observed. Acids exhibit the very interesting property of accelerating the reaction as long as the hydrogen ion content is small, but on further addition of acid the catalytic effect becomes negative and the reaction finally ceases.

The view put forward by Luther (4) as an explanation of negative catalysis is that it results from a partial destruction of some positive catalyst already present in the system. Titoff shows that this view leads us to expect that the observed velocity of reaction should be inversely proportional to the concentration of the negative catalyst, which is in general in agreement with experiment. Luther's explanation is probably true in certain cases, but by no means in all.

The above investigations refer to the oxidation of sodium

sulphite when the reaction proceeds thermally. The same reaction has recently been investigated photochemically by Mathews (5), the solution being exposed to the ultra-violet rays from a quartz mercury lamp.

In the first place it was discovered that uranium salts act as positive catalysts for the photochemical reaction in proportion to their concentration. Some idea of the normal rate of the reaction as carried out by Mathews in the absence of any catalysts will be gathered from the fact that a 0·2 normal solution of sodium sulphite was oxidised completely by the light in less than three hours. One very remarkable discovery is that copper sulphate, which Titoff found to be a very strong positive catalyst for the thermal reaction, exerts no measurable effect upon the photochemical reaction. Pyridine strongly inhibited the reaction when present in the proportion of 5 cc. to one litre of solution, whilst esters had a much weaker (negative) effect. Five drops of benzaldehyde to a litre of solution inhibited the reaction, whereas the same amount of glycerine had scarcely any negative effect. Bigelow had found that benzaldehyde and glycerine both acted as strong negative catalysts for the thermal reaction. Other substances examined, all of which exhibited a negative catalytic effect were, urea (very feeble effect), phenol, quinine-sulphate, and hydroquinone. In the presence of quinine sulphate and pyridine the solution acquires a green colour under the influence of light, and with hydroquinone it becomes opalescent.

It would appear from the above results that we have to do with selective absorption of light at certain regions of the spectrum. It is very necessary therefore to possess a complete knowledge of the absorption spectra of all the substances participating in the reaction. Mathews indicates that such measurements in the ultra-violet region are to be undertaken. It may be possible in this way to link up more closely the photochemical and thermal reactivities of various substances.

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4. Luther, *ibid.* **45**, 662, 1903.
5. Mathews and Dewey, *Journ. Physical Chem.* **17**, 211, 1913.
   —— and Weeks, *Journ. Amer. Chem. Soc.* **39**, 635, 1917.

ORGANIC CHEMISTRY. By P. HAAS, D.Sc., Ph.D., St. Mary's Hospital Medical School, London.

THE possibilities of the technical applications of acetylene have been more than once referred to in these columns, and a recent paper by Richard Meyer and Hans Wesche (*Berichte*, 1917, **50**, 422) indicates a further direction in which this hydrocarbon might be utilised. One of the authors has for some years past been making a study of the pyrogenic condensation of acetylene, a subject which was originally investigated by Berthelot in 1866. The method adopted has been to pass acetylene alone or mixed with hydrogen over copper gauze in tubes heated to 600° C., and in the earlier papers (1912, 1913, and 1915) the production of a number of different hydrocarbons was described. The present paper is concerned with attempts to prepare some of the phenolic and heterocyclic compounds found in coal tar. It has been found that acetylene reacts with hydrogen sulphide in presence of copper at 640–660° C. to form thiophen and thionaphthen. Acetylene methane and hydrogen sulphide gave thiotolen, thioxens, and thionaphthen. Acetylene and ammonia gave pyridine, pyrrole, aniline, benzonitrile, quinoline, anthracene, and fluorene. Acetylene and steam reacted at 500–600° C., forming only a small quantity of phenol. Acetylene steam and ammonia gave pyridine, aniline, phenol, and much hydrogen cyanide. Aniline and steam reacting at 650–700° C. gave a small quantity of phenol. Much of the work done has been carried out with a view to throwing some possible light on the origin of the various compounds occurring in coal tar. A paper by Zanetti and Egloff (*J. Ind. Eng. Chem.* 1917, **9**, 350) dealing with the thermal decomposition of benzene at temperatures varying from 500–800° C. is of some interest in this connection. The chief products of decomposition are diphenyl, diphenylbenzenes, carbon, and hydrogen, but no naphthalene or acetylene. Diphenyl begins to be formed at 500° C., but the optimum temperature for its formation is 750° C. Above this temperature diphenylbenzene, carbon, and hydrogen are formed. Copper exerts no marked action until the temperature exceeds 750°, when it favours the formation of carbon. Iron and nickel likewise favour the decomposition to carbon and hydrogen.

A new type of ammonium compound is described by Schlenk and Holtz (*Berichte*, 1917, **50**, 276). By shaking an ether

solution of potassium ditolylamine with tetramethylammonium chloride in an atmosphere of nitrogen the following reaction takes place:

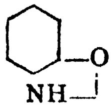
$$(CH_3C_6H_4)_2N.K + (CH_3)_4NCl = KCl + (CH_3C_6H_4)_2N.N(CH_3)_4.$$

The resulting compound ditolylaminotetramethyl ammonium crystallises from pyridine in greenish yellow leaflets and is hydrolysed by water to ditolylamine and tetramethylammonium hydroxide, and is therefore a salt in which the fifth valency of one nitrogen atom is satisfied by the valency of another nitrogen atom.

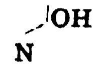
Datta and Chaterjee (*J. Amer. Chem. Soc.* 1917, **39**, 485) in their studies on halogenation describe cases of direct introduction of an iodine atom into the molecule of an aromatic hydrocarbon by acting in the presence of nitric acid, whereby the latter is reduced to oxides of nitrogen. A certain amount of nitration is liable to take place at the same time, especially in the case of the higher hydrocarbons. With the aliphatic hydrocarbons the tendency to hydrolyse is so great that the iodo derivatives are destroyed by the nitric acid as soon as they are formed, but it has been found possible under certain conditions to obtain small yields of iodo derivatives of some of the higher hydrocarbons. The following compounds were actually obtained: iodobenzene from benzene, *o*- and *p*-iodotoluenes from toluene, *o*- and *p*-iodoxylenes from xylene, etc. A small quantity of iodohexane was also obtained from hexane; naphthalene gave a mixture of iodo and nitronaphthalene. Datta and Prosad (*J. Amer. Chem. Soc.* 1917, **39**, 441) have also found that iodine can be introduced into phenols and nitrophenols by acting upon these substances with nitrogen iodide or a solution of iodine in potassium iodide in presence of ammonia; under the latter conditions the iodonitrophenols produced are obtained in the form of their ammonium salts. Treated in this way phenol gave tri-iodophenol, *o*-cresol gave di-iodocresol and *m*-cresol gave tri-iodo-*m*-cresol, while *o*-nitrophenol gave the ammonium salt of di-iodonitrophenol. A great many other examples are given, but for these reference must be made to the original.

In continuation of his work on the chemical constitution of the vitamines R. R. Williams now states (*J. Biol. Chem.* 1917, **29**, 495) that since the needle crystal form of 2 hydroxy-

pyridine (*cf.* SCIENCE PROGRESS, No. 44, April 1917, p. 634), has curative action in polyneuritis, it must have the constitution represented by the formula I. and not II.:

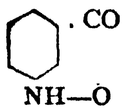


I



II

In other words it must belong to the class of pseudobetaines to bring it into line with methylpyridine, trigonelline, and betaine, all of which exert a curative effect on polyneuritic birds. For the same reason it is suggested that nicotinic acid prepared by the action of silver hydroxide on nicotinic acid hydriodide must have the constitution:



and further the curative action of the vitamine fractions of yeast and rice polishings must be due in part to the presence of this form of nicotinic acid or some simple derivative thereof.

From feeding experiments on pigeons Atherton Seidell (*J. Biol. Chem.* 1917, **29**, 145) has shown that the vitamine deficiency of an exclusive diet of polished rice may be made up by daily doses of 0·5–1·0 c.c. of a clear filtrate from autolysed brewers' yeast, and he has calculated that a grown pigeon requires less than 1 mgm. of vitamine daily; the diet of a pigeon and possibly of a man also would therefore require to contain about 0·0033 per cent. of vitamine.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., F.G.S., University, Glasgow.

Dynamical and Structural Geology

KOTO, B., The Great Eruption of Sakura-jima in 1914, *J. Coll. Sci. Imp. Univ. Tokyo*, 1916, **38**, 1–237.

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POWERS, S., Intrusive Bodies at Kilauea, *Zeitsch. f. Vulcanologie*, 1916, **3**, 28–33.

SCHUCHERT, C., Atlantis and the Permanency of the North Atlantic Ocean, *Proc. Nat. Acad. Sci.* (*Wash.*), 1917, **3**, 65–72.

SPENCER, J. W., Origin and Age of the Ontario Shore—Birth of the Modern Saint Lawrence River, *Amer. J. Sci.* 1917, **43**, 351–62.

ANDREWS, E. C., Shoreline Studies at Botany Bay, *J. and Proc. Roy. Soc. of New South Wales*, 1916, **50**, pt. 1, 165–76.
BECKER, G. F., Mechanics of the Panama Canal Slides, *Prof. Paper* 98-*N*, *U.S. Geol. Surv.* 1916, pp. 253–61.

THE Sakura-jima eruption of 1914, described by Koto, is remarkable, not only for its great size and intensity, but also for lateral eruptions which poured huge lava-flows into the sea, and converted an island into a peninsula. These flows are interpreted as due to horizontal satellitic injection of laccolites into the flanks of the volcano, which gave rise to subordinate lateral vents.

Oinouye describes the varied phenomena of the eruption of Usu in 1910. The outbreak was preceded by numerous earthquakes and roaring noises. Then came explosions opening V-shaped craterlets from which numberless red-hot bombs were ejected. A remarkable feature was the extrusion of a plug forming a new mountain.

The great outflow of lava from Mauna Loa (Hawaii) from May 19–31, 1916, brought to a close the eruptive period of that volcano 1914–16. There can be little doubt but that Mauna Loa and the lava lake of Kilauea are connected. The latter is now regarded as a decadent volcano of earlier origin, acting as a gas pressure-gauge adjacent to, and partly buried by, the younger and more vigorous Mauna Loa.

Powers describes the intrusive bodies known to occur near the summits of the Hawaiian basaltic volcanoes. These are believed to represent the fillings of lava tubes. Other intrusive masses have rather the form of volcanic necks.

In his paper on Atlantis Schuchert combats the views of Termier that the whole region of the Azores, Canaries, and Cape Verde islands has recently been submerged beneath the waters of the Atlantic. The subsidence of so great an area (210,000 square miles) would have resulted in a lowering of the eustatic strand-line of all oceans by 15 feet; but there is no sign of the terraces which should have been left along the ocean margins.

Regional and Stratigraphical Geology

HOLMQUIST, P. J., Swedish Archæan Structures and their Meaning, *Bull. Geol. Inst. Upsala*, 1916, **15**, 126–48.
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HENDERSON, J., Geology and Mineral Resources of the Reefton Subdivision, *New Zealand Geol. Surv. Bull.* No. 18, 1917, pp. 232.
STEPHENSON, L. W., and CRIDER, A. F., Geology and Underground Waters of North-Eastern Arkansas, *Water-supply Paper* 399, *U.S. Geol. Surv.* 1916, pp 315.
CAPPS, S. R., The Chisana-White River District, Alaska, *Bull* 630, *U.S. Geol. Surv.* 1916, pp. 130.

There has come to hand this quarter an unusually large number of important memoirs on stratigraphical and regional geology, which illustrate phases of geological work in many parts of the earth. Holmquist has supplied English-speaking geologists with a very useful summary of recent work on the Swedish Archæan. He shows that the Archæan granites and gneisses are intrusive into the porphyry-leptite bedded formation, and do not form a basement on which the oldest sedimentary rocks were deposited—a development in line with American and Finnish work on similar rocks. The porphyry-leptite formation of lavas and sediments is the oldest known Archæan group, and is correlated with the Keewatin of North America.

Frech's exhaustive memoir on the geology of Asia Minor contains a full account of the stratigraphy and structure of the Anatolian Mountains, and of the palæontology of the Taurus. A large part of the memoir, however, is devoted to a comprehensive general account of the geography, geology, and vulcanology of Asia Minor treated as a single geological unit.

The area of West Central Sinai described by Ball is of extreme tectonic complexity. The foundation rocks are Archæan gneisses, granites, diorites, and porphyries, on which are laid down rocks belonging to the Carboniferous, Cretaceous, Eocene, and Miocene periods. The only mineral resources of commercial value are manganese iron ores, occurring within the Carboniferous limestone.

Foye's work on the Fiji Islands has disclosed an "oldland" of slates and red sandstones in Viti Levu, intruded by a batholith of gabbro, diorite, and granite, which, after erosion, was covered by volcanic rocks. After further erosion coral conglomerates, marls, claystones, and coral limestones were

deposited. Vanua Levu has batholithic gabbro, followed, after erosion, by lavas and volcanic ash. The Lau Islands consist of volcanic rocks and limestone.

The rocks of the Reefton Subdivision of New Zealand consist of sharply folded greywackes and argillites, probably of Lower Palæozoic age, overlaid first by Devonian, then by Tertiary rocks of Eocene and Miocene age. The older rocks have been invaded by granitic intrusions. The economic products are gold-quartz veins, alluvial gold gravels and conglomerates, and coal.

Petrology

SEDERHOLM, J. J., On Synantectic Minerals and Related Phenomena, *Bull. Comm. Geol. Finlande*, 1916, No. **48**, pp. 148.

CALKINS, F. C., A Decimal Grouping of the Plagioclases, *J. Geol.* 1917, **25**, 157-9.

IDDINGS, J. P., and MORLEY, E. W., A Contribution to the Petrography of the Island of Bawean, Netherland Indies, *Proc. Nat. Acad. Sci. (Wash.)*, 1917, **3**, 105-9.

HOLMES, A., Picrite from the Ampwihi River, Mozambique, *Geol. Mag.* (6), 1917, **4**, 150-7.

TYRRELL, G. W., Trachytic and Associated Rocks of the Clyde Carboniferous Lava-Plateaus, *Proc. Roy. Soc. Edin.* 1917, **36**, pt. 3, 288-99.

Sederholm's work on synantectic minerals deals with structures such as reaction-rims, corona-minerals, kelyphite, myrmekite, etc., found in igneous rocks and occurring at the mutual boundaries of minerals. These mineral associations are termed *synantectic*. The structures are believed to be of primary igneous origin in granitic rocks, but are referred to the last stages of consolidation when residual fluids and gases played an important part. In the mafic rocks synantectic structures are believed to be of secondary or metamorphic origin.

Calkins has constructed a diagram showing the surprising diversity of usage of various authorities in subdividing the range of the plagioclase felspars. He proposes a decimal grouping, so that the term albite covers the range from Ab_{100} to Ab_{90}; oligoclase, $Ab_{90}-Ab_{70}$; andesine, $Ab_{70}-Ab_{50}$; labradorite, $Ab_{50}-Ab_{30}$; bytownite, $Ab_{30}-Ab_{10}$; and anorthite, $Ab_{10}-Ab_{0}$. This seems to be a sensible and useful suggestion.

The rocks of Bawean, described by Iddings and Morley, are highly alkalic and potassic lavas which belong to vicoite and phonolite. One of the vicoites falls into a hitherto unnamed

subrang (II. 7, 2, 3) of the American Quantitative Classification, which is consequently named *baweanose*.

The rocks of the past and present flows of Sakura-jima, Japan (see Koto B, reference under Dynamical and Structural Geology) are, for the most part, hypersthene-, and hypersthene-augite-andesites, occasionally also containing olivine as an accessory. Numerous bombs of various gabbroid rocks were also ejected. Bombs and lavas from Usu, Japan (see Oinouye, reference under Dynamical and Structural Geology) are also hypersthene-augite-andesites. These rocks appear to be overwhelmingly predominant among the lavas of the present and recent circum-Pacific volcanoes.

The Mozambique "picrite" described by Holmes is a dyke in gneiss; and is almost identical with the picrite-basalt lavas of Hawaii, and with the rocks called "felspathic picrite" by Lacroix (see SCIENCE PROGRESS, July 1917, p. 32).

Economic Geology

HORWOOD, C. B., The Gold Deposits of the Rand; London (C. Griffin), 1917, pp. xx, 369.

YOUNG, R. B., The Banket: a Study of the Auriferous Conglomerates of the Witwatersrand and the Associated Rocks; London (Gurney & Jackson), 1917, pp. 125.

WATERHOUSE, L. L., The South Heemskirk Tin Field, *Tasmania, Dept. of Mines, Geol. Surv. Bull.* No. 21, 1916, pp. 450.

BROOKS, A. H., Antimony Deposits of Alaska, *U.S. Geol. Surv. Bull.* 649, 1916, pp. 67.

GREGORY, J. W., The Geology of Phosphates, and their Bearing on the Conservation of Mineral Resources, *Trans. Geol. Soc. Glasgow*, 1917, **16**, pt. 2, pp. 116-63.

HUME, W. F., Report on the Oil-fields Region of Egypt, *Survey Dept. Egypt*, 1916, pp. 103.

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HILL, J. M., Notes on Some Mining Districts in Eastern Nevada, *Bull.* 648, *ibid.* 1916, pp. 214.

Mr. Horwood is a strong upholder of the infiltration as opposed to the placer theory of the origin of the huge gold

deposits of the Rand. He believes that the numerous diabase dykes in the Rand were the means of opening up the channels by which the gold-bearing solutions ascended. A new point advanced by Horwood relates to the fact that the Rand bankets are really gold-silver deposits, as the bullion contains 10–12 per cent. of silver. This tells against the placer theory, for placer gold is always exceptionally pure.

Prof. Young, on the other hand, is a supporter of the placer theory of origin, and has arrived at his conclusions from a close petrographical study of the banket and its metasomatism. He points out that the metasomatic changes which have been appealed to in support of the infiltration theory, took place subsequent to the cementation of the rock by quartz, and therefore after the banket had lost its original permeability. He considers that the placer theory has been greatly strengthened by the recent stratigraphical work of E. T. Mellor of the Transvaal Geological Survey.

Waterhouse's description of the South Heemskirk tin-field in Tasmania contains a very full account of the igneous, stratigraphical, and economic geology of an interesting district. The ore deposition was associated with intense igneous action in the Devonian, the dominant tin ores being introduced with the granites, and the subordinate nickel ores with the basic intrusions.

While antimony ore is known at sixty-seven localities in Alaska, the prospects for profitable antimony mining under normal conditions is not encouraging. The great war demand for the metal, however, has stimulated production, and has led to the publication of Brook's memoir, which summarises present knowledge of the Alaskan antimony deposits.

In his interesting Presidential Address to the Geological Society of Glasgow, Prof. J. W. Gregory reviews and classifies the phosphate deposits of the world, including therewith an account of the phosphatic lenticles in the Scottish Torridonian sandstone. He shows that the history of coal, iron, and phosphate mining in Britain illustrates the truth of the economic principle of using an asset while you can. From an examination of the world's phosphate production statistics, Prof. Gregory concludes that there is no reasonable danger of a phosphate famine as predicted by some alarmists ; " and we are no more blameworthy for using as much as we need than

were the Phœnicians because they worked out the richest deposits of alluvial tin in Cornwall regardless of the convenience of Chicago and the requirements of the tinned meat trade."

The oil-field region of Egypt is near the Gulf of Suez, and consists of a series of marls, salt, gypsum, and reef formations of Miocene and Pliocene age. According to Hume's detailed report, the most conspicuous oil indications are found in connection with overfolded anticlines along both shores of the Gulf of Suez. The richest supplies of oil occur in dolomitic limestones associated with the Miocene gypsum deposits.

The new memoir of the Geological Survey of Scotland is the first of a series designed to present the economic geology of the Central Coal-field. It deals principally with the coals, and provides much new information as to the records of bore and shaft journals, and other mining material collected during the recent revision of the coal-field. Several maps are given, showing the isopachytic lines for certain of the coal-seams.

BOTANY. By E. J. SALISBURY, D.Sc., F.L.S., East London College, University, London.

Ecology.—R. H. Boerker has carried out a number of experiments upon the germination and early growth of tree seedlings under varying conditions of light, water content, and soil texture (Ecological Investigations upon the Germination and Early Growth of Forest Trees. Lincoln, Nebraska, 1916). In all some twenty-seven species were experimented upon. It was found that in most cases shade accelerated the inception of germination, a feature that may be attributed to the increase of soil moisture which accompanies the diminution of light. In addition the decreased illumination is accompanied by a prolongation of the germination period, and the final percentage of seeds that germinate is usually higher in the shade than in the light.

Inadequate soil moisture was found to not only delay germination and cause a decrease in the percentage of seedlings, but to diminish the length of the germination period. It is in harmony with these results that the more xerophyllous species begin germinating first and have the shortest germination period, whilst the reverse is the case with mesophyllous species. In regard to soil texture, well aerated sandy soil was found to

yield the highest number of seedlings and to show the longest germination period. Size of seed also appears to be a factor influencing the percentage of germinating seeds, since this latter is higher in the case of large seeds (e.g. *Pinus ponderosa* and *Pseudotsuga taxifolia*) than in the case of small ones.

The most complete account yet published dealing with the ecology of terrestrial algæ is that by J. B. Petersen (*Mém. de l'Académie Roy. des Sciences et des Lettres de Danemark*, I. XII. 1915). As a result of his observations he concludes that diatoms are to be encountered on most types of bare earth. Diatoms are, however, often absent from the soil of woods and forests and the drier parts of heaths, especially where there is a covering of decaying leaves. Such situations are usually acid, and perhaps the poverty of diatoms is related to this feature. The fact that on the wetter parts of heaths several species occur might be due to the greater dilution of the humic acids; the author, however, expresses the opinion that chemical reaction has little influence, although he finds that the richest diatom flora is encountered on garden mould and arable land where the acidity is usually relatively low. The most interesting fact is that the aerial diatoms are not only distinct from the aquatic, but are, without exception, of small size. They are mostly members of the sub-family Raphideæ, and the author attaches importance to the fact that all can exhibit movement, and so perhaps wander to the more humid situations during periods of drought. The aerial Chlorophyceæ appear to be distributed in relation to the character of the surface, and chemical reaction. Especially characteristic of acid soils are *Zygogonium ericetorum* (cf. *Fritsch. Ann. Bot.* Jan. 1916), *Mesotænium violascens* and *Coccomyxa* spp. On alkaline soils *Mesotænium macrococcum* and species of *Hormidium* and *Vaucheria*. Other associations recognised are those of dead wood, stony soils, tree trunks, etc. The epidendrous forms are particularly abundant on the smaller branches, a feature attributed to the diminished competition with lichens.

The influence of the physical character of the substratum is illustrated by the difference in the algal flora from the trunks of various species of trees. The absence of algæ from the rain-tracks upon the trees is an interesting and surprising feature.

Genetics.—Miss Saunders has studied the inheritance of

doubleness in the flowers of *Meconopsis*, *Althæa* and *Dianthus* (*Journal of Genetics*, 1917), in all of which the double condition is exhibited in various degrees, so that every stage, from a single to a completely double flower, can be obtained. In *Meconopsis* the doubleness results from varying degrees of petalody of the andrœcium and gynœcium, extreme forms being completely sterile. Both doubles and singles breed true. The former behave as dominants and the latter as recessives. In *Althæa* the offspring which result from crossing high grade doubles with singles are low grade doubles constituting a heterozygous race. The F2 generation yields three types in the proportions of one high-grade double, two heterozygous low-grade doubles, and one single. In the genus *Dianthus* the author has experimented upon the Carnation and the Sweet William. In the former the double type is dominant, whilst in the latter it is the single type which is dominant. The flowers of the Sweet William appear to present two kinds of double flowers. In the one this condition is the result of petalody alone, and such flowers exhibit fifteen petals, of which ten replace the stamens. The second type is the outcome of petalody combined with splitting, and as a consequence twenty or more petals are often present.

General.—Dr. Boycott (*The Naturalist*, Jan. and Feb., 1917) has investigated the occurrence of manganese in various land and fresh water snails, and incidentally gives some interesting data as to the amount present in various plants upon which the snails feed. These show in general a very much higher manganese content for water plants than land plants. The quantities given are in parts per 10,000 of dried material. Marrow and ash leaves gave only 0·2, grass 0·6—1·1, whilst beech leaves contained as much as 8·6–10·7. Jadin and Anstruc (*Comptes Rendus*, 1912) in 80 species belonging to 32 families found from 0·014–7·6. In most of the aquatics examined, the manganese content was much higher and ranged from 1 part per 10,000 in *Potamogeton crispus* to 77 in the Water Buttercup. Algal slime gave from 20 to 377, whilst an aquatic moss yielded as much as 690. The manganese content of various Agarics ranged from 0·2 to 0·7.

In 1914 Prof. Bottomley described some experiments which indicated that when peat is decomposed, by incubation with a mixed culture of aerobic soil organisms, certain substances

are produced which greatly accelerated the growth of maize seedlings in water cultures, even though only added in small amounts. Experiments have now been performed (*Proc. Roy. Soc.* B., vol. lxxxix. 1917, pp. 481–507) to test the effect of extracts of peat, thus treated, on the growth of *Lemna minor*. When the aqueous extract of "Bacterised peat" was added to a Detmer culture solution the rate of growth of the *Lemna* was twenty times more rapid than in the control, whilst the increase in weight was augmented 62 times. A less marked effect was obtained by the addition of water extract freed from humic acid.

Miss Mockeridge (*ibid.* pp. 508–533) has tested the effect of these extracts upon the activities of nitrifying and denitrifying bacteria. She arrives at the conclusion that the activities of the former are accelerated, whilst those of the latter are depressed. Both investigators found that the alcoholic extract and the phosphotungstic fraction produced similar results.

Selected List of Recent Literature

(Papers of which abstracts have been given are not included)

Bower, F. O., Studies in the Phylogeny of the Filicales, VI. Ferns showing the Acrostichoid condition with special reference to Dipterid Derivatives, *Ann. Bot.* January 1917.

Brenchley, W. E., The Effects of Weeds on Crops, *New. Phyt.* May 1917.

Bristol, B. M., Life-history and Cytology of *Chlorochytrium grande*, *Ann. Bot.* January 1917.

Farrow, E. P., On the Ecology of the Vegetation of Breckland, III. General Effects of Rabbits on the Vegetation, *Journ. of Ecology*, March 1917.

Gano, L., Ecology of Florida, *Bot. Gaz.* May 1917.

Lotsy, J. P., Qu'est-ce qu'une espèce? *Archives Néerlandaises de Sci. Naturelles*, t. iii. 1916.

Marshall, E. S., Critical Notes on some Britannic Saxifrages, *Journ. Bot.* June 1917.

Shantz, H. L., Plant Succession on Abandoned Roads in Eastern Colorado, *Journ. of Ecology*, March 1917.

Small, J., Floral Anatomy of some Compositæ, *Journ. Linn. Soc.* May 1917.

PLANT PHYSIOLOGY.

By I. Jorgensen, Cand. phil. (Copenhagen), D.I.C. (Plant Physiology Committee), Imperial College of Science and Technology, London.

Carbon Assimilation.—Carbon assimilation, which for a few years has not occupied a prominent position in plant physiological researches, has again attracted the attention of many workers.

Willstätter and collaborators, whose important work on the chemistry of the pigments of the green leaf has attracted world-wide attention, have followed up this work by investigations on the physiological aspect of chlorophyll in a series of preliminary papers. (R. Willstätter and A. Stoll, (1) "Über die chemischen Einrichtungen des Assimilationsapparates," *Sitzungsb. d. k. Preuss. Akad. der Wiss.*, 1915, 322–46; (2) "Über die Assimilation ergrünender Blätten," *Sitzungsb. d. k. Preuss. Akad. der Wiss.*, 1915, 524–32; (3) "Untersuchungen über die Assimilation der Kohlensaure (Erste vorläufige Mitteilung): Über die Beziehungen zwischen Chlorophyllgehalt und assimilatorischer Leistung der Blätter," *Ber d. deutsch. chem. Ges.* **48**, 1540–64, 1915).

Willstätter's investigations have been carried out much on the same lines as the extensive researches of F. F. Blackman and H. T. Brown in this country, but with the improvement that, besides determining the quantitative relations of carbon dioxide, temperature and illumination as these authors have done, the amount of chlorophyll is estimated quantitatively by the refined and accurate methods published by Willstätter in earlier papers. Assuming with F. F. Blackman that the principle of limiting factors is valid for the processes of carbon assimilation he uses a high concentration of carbon dioxide (5 per cent.), a moderately high temperature (25° C.) and high light-intensity (a 3,000 candle-power Osram lamp at distances varying from 15 to 35 cm. from the leaf chamber). Under these conditions neither light nor carbon dioxide is a limiting factor. The amount of carbon dioxide assimilated can then only depend on internal factors and the temperature.

Willstätter varied the chlorophyll content by choosing leaves in different stages of development; spring, summer and autumn leaves, leaves poor in chlorophyll (yellow varieties), etiolated leaves and chlorotic leaves.

The first important point brought out is that the assimilation number (the ratio between the amount of carbon dioxide assimilated per hour and the amount of chlorophyll) is not constant, thus disclosing that other internal factors come into play. A study of the influence of temperature and light on the assimilation of leaves of green and yellow varieties of the same species convinced Willstätter that carbon-assimilation consists of two processes, a photochemical process and an enzymatic

process. The reaction of the chlorophyll system being photochemical, it may be assumed to have a temperature coefficient not far removed from unity, while the enzymatic process has a temperature coefficient of from 2 to 3. Temperature variations do not influence the assimilation in yellow varieties, while decrease in light decreases the assimilation. Willstätter therefore concludes that in this case the enzymatic system is more developed than the chlorophyll system, which thus controls the rate of reactions. In the green variety (*i.e.* the normal leaf) variations in light intensity are without influence on the assimilation, while temperature variations influence the rate of assimilation considerably, this being explained by the greater development of the chlorophyll system in this case, while the enzymatic system limits the rate of the reaction.

Another important fact is brought out by a study of the assimilation numbers of etiolated leaves becoming green. Miss Irving, in some experiments on etiolated leaves (*Ann. of Bot.*, 1910) found not only that etiolated shoots possessed no power of assimilation, but that shoots that had developed a considerable green colour did not possess the power. Willstätter contends that these results were due to Miss Irving's experimental arrangement, in which the plants were only supplied with their own respiratory carbon dioxide and were exposed to the feeble light from a north window. Willstätter used a much stronger light intensity (48,000 lux) and 5 per cent. carbon dioxide. Under these circumstances he finds that not only is there an appreciable assimilation, but that also the assimilation numbers of etiolated leaves are very high, thus indicating that the amount of chlorophyll is limiting assimilation. In Miss Irving's experiments light and carbon dioxide were probably limiting factors.

Finally, Willstätter, like so many great chemists before him, dogmatises as to the actual processes concerned in carbon assimilation, but the considerations which lead him to assume that carbon dioxide and chlorophyll form a dissociable compound are not worth recording here, as the experimental basis is very questionable, and he himself admits that only a few experiments could be carried out owing to the exhaustion of his stock of chlorophyll.

Altogether the present trend of plant physiology seems to aim less at getting the processes of the plant pictured by means

of simple chemical reactions than was formerly the case. Thus, for instance, the formaldehyde hypothesis of carbon assimilation seems at last to have lost the prominent position it has so unworthily occupied for nearly half a century. H. A. Spoehr ("Theories of Photosynthesis in the Light of some new Facts," *The Plant World*, **19**, 1–16, 1916) has published a carefully reasoned criticism of this theory, particularly in regard to the formation of formaldehyde from carbon dioxide and water under various conditions. He shows that what evidence there is could be used with much greater force in support of a formic acid theory of carbon assimilation, but he does not consider that the putting forward even of a formic acid theory would be at all justifiable. Jorgensen and Kidd ("Some Photochemical Experiments with Pure Chlorophyll and their Bearing on Theories of Carbon Assimilation," *Proc. Roy. Soc.* B, **89**, 342–61, 1916) have repeated with pure chlorophyll the experiments made by earlier investigators on the formation of formaldehyde in a system consisting of chlorophyll, water and carbon dioxide exposed to light. It was found that formaldehyde is only produced when oxygen is present, and then only as the result of a thorough disintegration of the chlorophyll molecule. Further criticism of the various theories of carbon assimilation and a review of the whole subject is to be found in the monograph of the subject by Jorgensen and Stiles (*New Phytologist* Reprint No. 10, London, 1917).

An attempt to revive the old theory of Liebig and others who supposed that organic acids were formed in carbon assimilation, has recently been made by A. B. Steinmann ("Studien über die Azidität des Zellsaftes beim Rhabarber," *Zeitsch. f. Bot.* **9**, 1–59, 1917). He has made a study of the acidity of the cell-sap of leaves and leaf-stalks of *Rheum*, and shows that the content of acid runs parallel to that of carbohydrate in the organs of assimilation. He contends, therefore, that in an acid-forming non-succulent plant like rhubarb, the acid is a product of assimilation.

The study of the products of assimilation in normal plants has been made the subject of several recent researches. In order to test and amplify the classical observations of Brown and Morris on the distribution of the products of assimilation in leaves, W. A. Davis, A. J. Daish and G. C. Sawyer have made analyses of the carbohydrates in leaves of mangold

collected in the field at two-hourly intervals throughout a twenty-four hour day, at three different season of the growing period (" Studies of the Formation and Translocation of Carbohydrates in Plants," I and II, *Journ. Agric. Sci.* **7**, 255–326, 327–51, 1916). For this work the methods of carbohydrate estimation when a mixture of carbohydrates is concerned had first to be worked out. This they succeeded in doing, and analyses were made of starch, sucrose, maltose, glucose, fructose, pentoses and pentosans. In some supplementary work with potato (Studies III., *Journ. Agric. Sci.* **7**, 352–84, 1916) " soluble starch " was estimated as well. The chief conclusion of these workers is the same as that of Brown and Morris, in that they regard sucrose as the primary sugar of carbon-assimilation from which hexoses arise later for translocatory purposes. On the other hand, they disagree with Brown and Morris in that they find pentoses present in measurable amount, while maltose is consistently absent from the leaves examined. Starch is never present in the leaves of mangold, but occurs in large quantities in potato leaves.

Further results of Davis, Daish and Sawyer's researches are that both sucrose and hexoses increase in quantity in the leaf during the middle hours of the day, and then fall off in amount during the night. As the season progresses the sugars increase in amount, but the sucrose to a much greater extent than the hexoses.

A recent contribution by Gast (*Hoppe-Seyler's Zeitsch. f. physiol. Chem.* **99**, 1–53, 1917) also attempts to repeat and amplify Brown and Morris's observations. Gast's analytical methods are somewhat similar to those of Davis, Daish and Sawyer, but his conclusions are based on the analyses of the leaves of five species collected early in the afternoon, and in the early morning. His results completely confirm those of Brown and Morris, even to the absence of pentoses and the presence of maltose in leaves. He points out with much justice that these results do not indicate that sucrose is the first produced sugar in the leaf, but that a good case can be made out for regarding cane-sugar as the first *recognisable* product of assimilation. Thus in some cases (e.g. *Cucurbita, Canna*) no glucose and very little fructose could be determined during the day.

In a critical and very important study by A. Ursprung

("Über die Stärkebildung im Spektrum," *Ber. d. deutsch. bot. Ges.* **35**, 44–69, 1917) the question of the relative assimilation in various parts of the spectrum is dealt with, as far as the assimilatory activity is expressed by the formation of starch. The experiments of Timiriazeff, Engelmann, Bonnier and Mangin and others are severely criticised, and it is found that any wave-length between 760 $\mu\mu$ and 330 $\mu\mu$ is capable of inducing starch formation.

PALÆOBOTANY IN 1916. By MARIE C. STOPES, D.Sc., Ph.D., University College, London.

THE influence of the war on the output of palæobotanical work was very apparent this year, and the difference between 1916 and 1915 is much more marked than that between 1915 and 1914. In the general account published in SCIENCE PROGRESS last year of the 1915 harvest of palæobotany it was pointed out how surprising (in spite of the war) were the quantity, quality, and handsomeness of production; the same cannot be said for 1916. There are only two large memoirs to record, both hailing from America. An important paper, forty-six pages long, on the Devonian of Norway, though of great interest and value, would not in an ordinary year have been physically noticeable on a table on which was spread the European output for the year; but that this year it is so is an indication of the slenderness of the year's publications.

Nevertheless there are some notable additions to the science, and these will be dealt with under the same arrangement of subjects as was used last year. In addition to the papers mentioned, I have the titles of about thirty others of less moment, or which have not yet reached me though this article was delayed in the hope of their arrival.

General Palæobotany.—The lamented death of the great Prof. Zeiller called forth from Dr. Scott an obituary, published in the *Proceedings of the Linnean Society*, in which a useful record of his work was given. M. H. Douvillé also did honour to the master (*Bull. Mens. Assoc. École sup. Mines*, pp. 18–25). The deaths of Zeiller and the other leading palæobotanists noted last year have influenced the output of work in the same sense as has the war.

Antevs published a general discussion of the climate of the

Palæozoic and Mesozoic epochs (*Geol. Fören. Stockholm Förhandl.* pp. 212–19) in a work criticising the too ready acceptance of the existence or absence of growth rings in petrified woods as an indication of a seasonal or a uniform climate. He pointed out that by analogy with some of the living types the failure of growth zones is no certain proof that the climate was uniform. The whole subject is one which wants critical and detailed observation, and is of general interest because the climates of the past epochs have been involved in discussions of the position of the poles, the existence of glacial epochs, and other general geological theories.

One of the three or four notable products of the year was that on FUELS of various ages and types which were dealt with in a most valuable memoir by Stevenson ("Interrelations of the Fossil Fuels," *Proc. Amer. Philos. Soc.* vol. lv. pp. 21–203). Peats and Tertiary coals received principal consideration in this first part of the work, which surveyed the subject from many points of view and brought together an immense mass of evidence about the various plants forming the fuel-deposits, their mode of deposition and other subjects. This work showed the fallacy of the generally accepted conclusion that true peat is confined to the temperate zones. Any plant, apparently, can, under suitable conditions, become a peat-maker—"the hyacinth, introduced into Florida, where it threatened to ruin the navigable rivers, has become a peat-producer of no little importance." Stevenson gave useful accounts of the coals of the Tertiary of all parts of the world, everywhere linking what he had to say with whatever palæobotanical evidence his wide research made available.

Stratigraphic Palæobotany.—In this connection the outstanding features of the year were the discoveries in the DEVONIAN, an epoch of surpassing interest but about which there is usually little to say. Dr. Halle, in his paper on "Lower Devonian Plants from Röragen in Norway" (*K. Svensk. Vetenskap Handl.* vol. lvii. pp. 1–46, 4 pls.) broke new ground. In his own words, "The important point is that this flora is a very good representative of the type of land-vegetation characteristic of the *Lower* Devonian, as contrasted with the well-known floras of Kiltorkan, Bear Island, etc., which belong to the Upper Devonian. We are dealing here with the remains of the very oldest land-flora at present known;

and it may be stated at once that there is a far greater difference between this flora and that of the Upper Devonian than between the latter and the Lower Carboniferous." The beds in which the relatively well-preserved fossil impressions were found was probably a fresh-water deposit, the contained plants having travelled no great distance. In addition to new species Halle gave most useful descriptions of species he identified with Dawson's original *Arthrostigma gracile* and *Psilophyton princeps*. Perhaps no genus has been more abused by uncritical and insufficiently experienced palæobotanists than Dawson's *Psilophyton*, and so Halle's remarks were most useful and timely. As throughout the strata older than the Tertiary the very existence of the Bryales is doubtful, particular interest attached to numbers of well-preserved, stalked sporogonia containing spores, which were remarkably moss-like in appearance.

In Scotland also peculiar Devonian plants came to light, and at the meeting of the British Association at Newcastle a committee presented its report on the Plant-bearing Cherts at Rhynie, Aberdeenshire (*Rep. Brit. Assoc. Sci.* 1916, pp. 206–16), the plants of which were reported on by Kidston. The plants were petrified in masses in a chert about 8 ft. thick of Old Red Sandstone age. The chert represented the infiltration of an ancient peat bed by silica. The peat appeared to have been formed entirely of two plants, the more crowded and numerous (*Rhynia*) composed of rootless rhizomes with tapering aerial stems bearing cylindrical sporangia but no leaves.

Work on the CARBONIFEROUS was remarkably scanty in 1916. Arber gave a comprehensive account of the fossil flora of S. Staffordshire (*Phil. Trans. Roy. Soc. Lond.* B, vol. ccviii. pp. 127–55, pls. II.–IV.), separating the floras of the red-grey Unproductives and the grey Productives. Useful and exhaustive lists of localities were given, and several forms well illustrated. A new "genus" was created for the external surfaces of calamite impressions, but as it covered almost the same ground as Grand' Eury's old term *Calamodendrofloyos*, its creation would be difficult to justify. The conclusion of the paper supported Kidston's view that the whole productive coal measures of S. Staffordshire belong to the Middle coal measures.

Two new foliage impressions from the TRIAS of Pennsyl-

vania were described by Wherry (*Proceed. U.S. Nat. Mus.* vol. li. pp. 327–9, pls. XXIX.–XXX.). In the CRETACEOUS Berry discussed the age of the Morrison formation (*Bull. Geol. Soc. Amer.* vol. xxvi. pp. 325–42) in which occurred the stumps of *Cycadella*. In this paper Berry concluded that " presently we may expect some modern Huxley to enunciate diastrophic homotaxis as opposed to diastrophic synchroneity."

The UPPER CRETACEOUS deposits of North America afforded material to Knowlton, who illustrated good foliage impressions from the Foxhills Sandstone (*U.S. Geol. Surv. Profess. paper* 98 H, pp. 85–93, pls. XV.–XVIII.) from this generally rather sterile deposit. In this, as well as dicotyledons and conifers, he recorded " countless thousands of specimens " of an alga, *Halymenites major*. Berry discussed the age of the Cretaceous flora of Staten Island and Martha's Vineyard (*Journ. Geol.* vol. xxiii. pp. 608–18), deposits which are notable palæobotanically as the source of the gymnosperms described some years ago with wonderful internal detail by Jeffrey and Hollick.

The TERTIARY, also of the States, was the subject of one of the two *large* memoirs of the year. Berry published a monumental work on the Lower Eocene (*U.S. Geol. Surv. Profess. paper*, 91, pp. 481, pls. CXVII.), one of the most important works on the Tertiary which has appeared for many years. The bulk of the text and the illustrations dealt systematically with the angiospermic foliage impressions, which, as in most deposits of this age, formed the large majority of the available remains. The first 160 pages dealt with the stratigraphy, outcrop, distribution, character, ecology, and correlation of the " Wilcox flora." Berry took a wide and sound view of the demands of his subject, and pointed out the importance of correlating what is known of the fauna, general conditions, etc., of the area. " In work on deposits that teem with the remains of marine life, as do many of the Tertiary formations of South-eastern North America, it is possible to arrive at very close approximations of the temperatures of the coastal waters. It may be safely assumed that boreal or temperate floras did not flourish in proximity to tropical marine faunas and that plants reflected their environment in the past as in the present." Berry discussed the difficulty of specific determinations from leaves alone, but pointed out how much neglected leaves have been by recent

botanists, and how the geologic indications of environment and other evidence assists a competent palæobotanist to form useful judgments. In this work over 300 species of this Wilcox flora were described, and it is therefore one of the largest fossil floras known from any single area. Berry elaborately compared this flora with the known floras of similar and neighbouring age, and thus throws into prominence the lamentable neglect of our own Tertiary floras. As Berry said, " No richly fossiliferous European plant horizons exactly equivalent to the Wilcox have as yet received monographic study. The Eocene of the south of England is rich in fossil plants at horizons that I consider equivalent to the Wilcox, but comparisons are unfortunately limited to the long lists of nomina nuda published by Ettingshausen,"—because our British collections have never yet been described.

In the *U.S. Geol. Surv. Profess. papers* 98 E and F, Berry considered the physical conditions indicated by the fossil floras of the Alum Bluff and the Calvert formations, the latter represented by a small flora present in diatomaceous beds of Miocene age.

Family Histories and Anatomy.—To the ANGIOSPERMS have been added many species and records in the Tertiary papers referred to above. Papers dealing with particular members of the angiospermic family were few in 1916. Nagel is reported to have published a monograph on the Betulaceæ in the ***Fossilium Catalogus***, Berlin, but I have not been able to see a copy. Berry contributed one or two semi-popular papers on angiosperms, and also recorded a Tertiary Nutmeg from Texas (*Amer. Journ. Sci.* vol. xlii. pp. 241–5) represented by its pericarps in sandstone. The same author described a new species of petrified palmwood (*Amer. Journ. Sci.* vol. xli. pp. 193–7) which, being of Cliffwood (Cretaceous) age, is one of the few really early records of this family.

The HIGHER GYMNOSPERMS.—Stopes described (*Ann. Bot.* vol. xxx. pp. 111–25, pl. IV.) a new well-petrified Cretaceous wood which showed the medullary ray pitting and wall thickening characteristic of the Abietineæ, while the tracheids had the typical multiseriate pits of the Araucarineæ. A new genus, ***Planoxylon***, was founded for this fossil, which came from New Zealand and is particularly interesting as an Australasian record because no living forms with abietinean ray structures are

endemic in this area at present. Coniferous woods from the American Potomac were described by Sinnott and Bartlett (*Amer. Journ. Sci.* vol. xli. pp. 276–93) from a locality, which though rich in various plant remains, has yielded only coniferous and no dicotyledonous woods. The fossils were described as species of *Podocarpoxylon* and *Paracupressinoxylon*, the latter representative of "that group of small-leaved Mesozoic conifers which are preponderantly araucarian in their affinities." Lindley and Hutton's classic specimen of a well-preserved pinus-like cone underwent modern re-examination and re-description by Dutt (*Ann. Bot.* vol. xxx. pp. 529–49, pl. XV.) under the name *Pityostrobus macrocephalus*. The cone shows not only well-preserved anatomical details, but is one of the few specimens from the later floras with ovular detail.

Cycadophyta.—The great event of the year was the appearance of the second volume of Wieland's already classic memoir on the American fossil Cycads (*Carnegie Publication* 34, pp. 277, pls. LVIII. 97 text figs.). This volume, like its predecessor, is magnificently illustrated, and the beautiful text figures are printed so well on such fine paper that the volume is a work of art. It is grievous to think that under present (or even pre-war) conditions no British scientist can hope to get his country to publish his work in a corresponding style. This volume contains fewer new facts than the first, and is largely a bringing together of scattered data and general considerations of the group, its allies and their interrelations. In the body of the work the wonderful "Hermosa Cycadeoid" (*C. Dartoni*), with its numerous ovulate and young cones, is fully described and magnificently illustrated. Even in such a series of splendid photographs, pl. 46 stands out as a wonderful presentation of a number of cones in section. In the general part of the work almost all the problems involved in a consideration of the groups are either dealt with at length or touched upon, and the position of the author is always interestingly presented, though if considered in detail might sometimes rouse lengthy discussion. The unique position of the group of the Bennettitales makes these fossils of such moment to palæobotanists that this new volume could only be treated adequately in a special review.

Of the cycadophyta in a wide sense a species of *Nilssonia* was recorded from the Cretaceous of W. Queensland by Walkom

(*Mem. Queensl. Mus.* vol. v. pp. 231–2, pl. XXIV.) represented by a foliage impression previously described as a *Pterophyllum*. Bassler (*Amer. Journ. Sci.* vol. xlii. pp. 21–6) claimed to have identified a "Cycadophyte," the *Plagiozamites* of Zeiller, in an American deposit, which makes it "the earliest occurrence thus far recorded of the plant group." His identification seems entirely unjustified, however, and, judging from the text figure given, his plant is simply a *Rhacopteris*. The arguments and conclusions in the paper are therefore invalidated, and will tend to confuse rather than elucidate the science of Palæobotany.

PTERIDOSPERMS.—This group, about which so much has been recently written, has been neglected this year. Kubart is reported to have published a consideration of *Anachoropteris pulchra* Corda (*Anz. Akad. Wiss. Wien*, vol. liii.), but I have not been able to see the paper.

PTERIDOPHYTA.—A specimen of *Annularia*, remarkably similar to the well-known European *A. stellata*, was described from a block with specimens of *Glossopteris* (Walkom, *Mem. Queensl. Mus.* No. 5, pp. 233–4, pl. XXV.). This association is of particular interest in this locality; the only *Annularia* previously recorded from the Permo-Carboniferous rocks of E. Australia is Feistmantel's species from the Greta Coal Measures.

The CHARACEÆ, so long dependent on scattered and desultory observation, were taken seriously in hand by Reid and Groves, who have read several papers on the subject. By careful, slow etching of the matrices with acid, as well as from sections, good results have been obtained. Beautiful photographs of hitherto obscure details offered a promise of much to come in the future about this puzzling and geologically persistent family. (See Preliminary Report on the Purbeck Characeæ, *Proceed. Roy. Soc. Lond.* B, vol. lxxxix. pp. 251–6, pl. VIII.)

ALGÆ.—Davis, in a short paper without illustrations, recorded the discovery of large numbers of Eocene algæ in a petroleum-yielding shale in Colorado (*Proc. Nat. Acad. Sci.* vol. ii. pp. 114–19). Blue-green algæ, chlorophyceæ, and others were stated to be present in greatnumbers, and to be represented by still living genera such as *Spirulina*, *Pediastrum*, and others.

ZOOLOGY. By CHAS. H. O'DONOGHUE, D.Sc., F.Z.S., University College, London.

[As several of the American journals for the period under review have not yet reached this country they have unfortunately had to be omitted.]

Protozoa.—The papers include "The Case of *Trichomonas,*" by Hadley (*American Nat.* vol. li. April 1917).

Invertebrata.—The papers include "Occurrence of a Holothurian new to the Fauna of Bermuda," by Crozier (*Ann. and Mag.* May 1917).

Keibin in "Recherches sur les Anthomyides à Larves carnivores" (*Parasit.* vol. ix. May 1917) gives a long and detailed study (occupying a complete part of the journal) of the larvæ of certain Diptera made with the objects of investigating their economic rôle and also of adding to our morphological and histological knowledge of the group, and in both points he has been very successful. The larvæ are to be found in all sorts of situations, but whatever their habitat they feed carnivorously and often act as destroyers of certain vegetable-feeding larvæ, and what is more important, of the larvæ of the domestic fly and of *Stomoxys calcitrans.* All the carnivorous species have not been examined in the same detail, as they cannot all be obtained with certainty. Intermediate forms—sometimes saprophytic, sometimes carnivorous, according to conditions—are known, and appear to indicate how the carnivorous forms have arisen. In addition to attacking other larvæ they also attack one another and have developed a strong cuticle partly in order to meet this. They have naturally a well-developed bucco-pharyngeal armature. The paper is well illustrated by figures and fifteen plates.

Other papers include: "The Identity of the two Amphipods, *Ampelisca eschrichtii* and *A. macrocephala,* Liljeborg, considered from an Antarctic Point of View," by Chilton; "Description of a new British Terrestrial Isopod (*Trichoniscoides scabrous* n.sp.)," by Collinge, both in *Jour. Zool. Research,* vol. ii. May 1917; "A New Tuberculate Terrestrial Isopod from New Zealand," by Chilton (*Ann. and Mag. Nat. Hist.* April 1917);

"South African Talitridæ," by Stebbing (*Ann. and Mag. Nat. Hist.* April 1917); "Notes on Myriapoda—V. On Cylindroiulus (Lencoiulus) nitidus (Verhoeff)," by Brade and Birks

(*ibid.* May 1917); "Notes on Collembola—Part IV. The Classification of the Collembola; with a List of Genera known to occur in the British Isles," by Shoebotham (*ibid.* June 1917);

"New Species of Indo-Malayan Lepidoptera," by Swinhoe (*ibid.* April and May 1917); "Descriptions of New Pyralidæ of the Subfamilies *Hydrocampinæ*, *Scopariana*, etc.," by Hampson (*ibid.* May and June 1917); "On New Weevils of the Genus *Mecysmoderes* from India," by Marshall (*ibid.* May 1917); "The Khapra Beetle (*Trogoderma khapra* sp.n.), an Indian Grain-Pest," by Arrow (*ibid.* June 1917);

"Notes on Fossorial Hymenoptera—XXVII. On New Species in the British Museum" (*ibid.* April 1917), "Notes on Fossorial Hymenoptera—XXVIII. On new Ethiopian Species of *Bembex* in the British Museum" (*ibid.* June 1917), both by Turner; "Descriptions and Records of Bees—LXXV.," by Cockerell (*ibid.* June 1917); "On Fabricius' Types of Odonata in the British Museum (Natural History)," by Campion (*ibid.* June 1917).

Vertebrata.—The papers include: "A Revision of the Clupeoid Fishes of the Genera *Promolobus*, *Brevoortia*, and *Dorosoma* and their Allies" (*ibid.* April 1917); "A Revision of the Clupeid Fishes of the Genera *Sardinella*, *Harengula*, etc." (*ibid.* May 1917), both by Regan; "Description of a new Lizard and two new Frogs discovered in West Africa by Dr. H. G. F. Spurrell," by Boulenger (*ibid.* May 1917);

"Further Studies in the Peritoneum and Intestinal Tract in Monotremes and Marsupials," by Mackenzie (*Jour. of Anat.* vol. li. April 1917), is a continuation of the author's previous studies on the same subject (*ibid.* vol. li. pt. i). It is claimed that the diversity met with in these two groups throws light on the significance of certain of the bands in the human intestine, the Duodenal (right lateral) fold, the left lateral (Lienomesocolic) fold, and the mesial fold.

A very extensive literature has grow up on the subject of the Pituitary Body particularly in the mammals, but in spite of it all practically nothing was known about the lower mammals. In "The Development of the Hypophysis Cerebri, Pre-oral Gut and Related Structures in the Marsupialia" (*Jour. of Anat.* vol. li. April 1917), Parker gives a very careful and detailed account of these structures in various marsupials, particularly in *Bettongia*, *Macropus*, *Dasyurus*, *Perameles*, *Trichosurus*, and *Phascolaretos*. Although in the

main concerned with these forms, a full and useful summary and discussion of other vertebrate types is also given. It is clearly shown that in the pituitary body of the adult, the pars tuberalis is derived from Rathke's pouch, the pars distalis from outgrowths from its walls or the ingrowth of connective tissue into these walls, and the pars neuralis from a hollow outgrowth of the floor of the diencephalon. A remarkable fact came to light, namely that Seessel's pocket forms a part of the hypophysis in *Phascolaretos cinereus* and *Phascolomys Mitchelli*, but not in other forms, *e.g. Dasyurus*, and the significance of this is not yet clear. The characteristic chromophilic and chromophobic cells are differentiated *in situ* before the adult condition is reached.

Other papers include: "On the Skeleton of an Adult Female Small Finner or Piked Whale (*Balænoptera acutorostrata*, Lacèpede)," by M'Intosh (*Jour. Zool. Research*, vol. ii. May 1917); "Some Notes on Three-toed Sloths" (*Ann. and Mag. Nat. Hist.* April 1917), "A new Vole from Palestine" (*ibid.* June 1917), "On the small Hamsters that have been referred to *Cricetulus phæus* and *Campbelli*" (*ibid.*), all by Thomas; "The Lemurs of the Hapalemur Group," by Pocock (*ibid.* April 1917); "Notes on some of the Viscera of an Okapi (*Okapia Johnstoni*, Lankester)," by Burne (*Proc. Zool. Soc.* April 3, 1917).

General.—Longley contributes "Studies upon the Biological Significance of Animal Coloration—II. A Revised Working Hypothesis of Mimicry" (*Amer. Nat.* vol. li. April 1917), the first part of which is printed in the *Journal of Experimental Zoology* and has not yet reached this country. The author criticises at some length the various theories of mimicry, pointing out that most of them are unsatisfactory. It is suggested "that the coloration of typical members of each group is a combination of hues well suited upon the average to render them inconspicuous in such places as they commonly frequent. If this be so, the initial step towards the production of new cases of mimicry might be any one of many variations in mode of nutrition or reproduction which would lead representatives of the first family to spend their lives after the manner of the second." From this similarity, at first only general, the action of natural selection is all that is required to produce mimics.

An investigation of "Biological Enigmas and the Theory of Enzyme Action" by Troland (*ibid.* June 1917) has resulted in a critical and somewhat polemical paper which is in the main an expansion of the author's statement: "It has for some years been my conviction that the conception of *enzyme action*, or of *specific catalysis*, provides a definite, general solution for all the fundamental biological enigmas: the mysteries of the origin of living matter, of the source of variations, of the mechanism of heredity and ontogeny, and of general organic regulation." In truth a most accommodating and inclusive solution.

Other papers include: "Sources of Anatomical Literature," by Moodie (*Amer. Nat.* vol. li. April 1917); "Linkage in Maize: Aleurone and Chlorophyll Factors" (*ibid.*); "Nucleus and Cytoplasm as Vehicles of Heredity," by Dunn (*ibid.* May 1917); "Mutation in *Didinum nasutum*," Mast (*ibid.* June 1917); "The Method of Evolution from the View-point of a Geneticist," by Shull (*ibid.*).

ANTHROPOLOGY. By A. G. THACKER, A.R.C.Sc.

IN mentioning the recent rebellion in Nyasaland in these notes last April, I referred to the importance, both theoretical and practical, of the study of race-psychology. In the *Eugenics Review* for April 1917 (vol. ix. No. 1) there is a short but very arresting contribution to the subject by the Rev. A. T. Bryant, under the title "Mental Development of the South African Native." The author has had prolonged experience of the natives, both male and female, young and adult, and it may be mentioned that the article shows none of that bias in favour of theories of race-equality which characterises some Christian missionaries and philanthropists. The natives discussed are the Bantus, not the Hottentots. The essay consists mainly of a comparison of the mental powers of Bantus and Europeans. Mr. Bryant thinks that up to the time of puberty the Bantu boy is rather in advance of the European boy, though he is careful to point out that this may only be due to the fact that in the higher departments of mentality, such as the power of reflection, wherein the European specially excels, all boys display but little development up to that age. At puberty, however, the author found that not only is the mental develop-

ment of the Bantu boy arrested, but that actual retrogression takes place, with the result that the adult white man shows a marked and presumably innate superiority to the adult Bantu man. Physiologists will probably have something to say upon the causes of this strange retrogression before very long. Mr. Bryant's most startling observation has yet to be mentioned. He states that in the female Bantu, both as regards young girls and as regards adult women, he was unable to find any inferiority as compared with the European female. Unfortunately, the author does not develop this particular thesis, and he does not institute any direct comparison between Bantu women and Bantu men. But if the Bantu woman be really the mental equal of the white woman, is she not in advance of her own men? Further, if the average of the Bantu women is equal to the average of European women, it is very possible, though not a necessary consequence, that the range of variation in the females of the two races is equally great. Now it will not be disputed that the mental powers of a considerable minority of white women—probably about 10 per cent.—are, even in those departments, such as ratiocination, which as Romanes pointed out long ago constitute the special masculine province, appreciably superior to the powers of the average white man, whilst a small minority of women soar far above the ordinary man. Are we to believe that the Bantu nations have possessed intellects such as these? It may be so; the intellects may have existed and have yet been unable to make themselves felt owing to adverse social conditions. The idea must not be scouted merely because to most of us it happens to be unexpected.

The Journal of the Royal Anthropological Institute for the first half of 1916 (vol. xlvi.) is more than usually full of interesting material. The first paper is Prof. Keith's Presidential Address, which is entitled "On certain Factors concerned in the Evolution of Human Races." This is a dissertation on what the author calls "clannishness," as a psychological factor tending to produce that isolation which was necessary to the evolution of distinct races from an original supposedly homogeneous mankind. Prof. Giddings has used the term "consciousness of kind" to express the attraction of like animal to like animal, and this combined with the instinct of gregariousness produces the clannishness to which the author

refers,—the instinctive love of a man for his own people. Mere clannishness would become developed, in the author's opinion, into " race-instinct " and finally into " inter-species aversion." In this connection, Prof. Keith mentions the separate races of the three great apes which are now known to exist; and he describes the numerous and very small social groups into which the hill-tribes in Borneo and the aborigines in central and other parts of Australia are split up. The longest contribution to this number of the *Journal* is an article by H. J. Fleure and T. C. James on " Geographical Distribution of Anthropological Types in Wales," in which many instructive details are given. Harold Peake also writes an interesting, though highly speculative, essay on " Racial Elements concerned in the First Siege of Troy." This capture of Troy took place about a thousand years before the ever-famous siege described in the *Iliad.* This city, known to archæologists as Hissarlik II. (the city of Laomedon being Hissarlik VI.), was almost completely destroyed, and the author comes to the conclusion that the conquerors and destroyers were members of the aggressive Nordic Race, whilst the conquered defenders were a brachycephalic people. Other papers in this number of the *Journal* are: " Notes on the Anthropometry of some Central Sudan Tribes," by P. A. Talbot; " Evolution in Maori Art," by H. D. Skinner; " The Evolution of the Earliest Palæoliths from the Rostro-Carinate Implements," by J. Reid Moir; and " Some Votive Offerings to the Venetic Goddess Rehtia," by R. S. Conway.

The magazine *Man* for the first three months of 1917 contains several interesting articles, including " The Transport of the Coconut across the Pacific Ocean," by Charles Hedly (in January), " Trans-Pacific Migrations," by Professor Hrdlicka (in February), and " The Evolution of the Rostro-Carinate Implement from the Primitive Kentian Plateau Implements," by J. Reid Moir (in March).

ARTICLES

PERTURBATIONS IN MODERN PHYSICAL PHILOSOPHY

WITH SPECIAL REFERENCE TO THE MOTION OF MATTER THROUGH ETHER, AS OPPOSED TO THE PRINCIPLE OF RELATIVITY.

BY SIR OLIVER LODGE, F.R.S., D.Sc., Sc.D., LL.D.,
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IT appears to be becoming necessary for a conservative physicist to criticise certain popular expositions of the Theory of Relativity, and especially the philosophical complications which have been based upon that theory, with the result that in the minds of philosophers and general readers, and in the minds of some physicists, the foundations of Physics appear to have been uprooted, and even the abstractions of space and time interfered with in a confusing manner. I wish to argue that no such conclusions really follow from the premises, that Newtonian Dynamics is supplemented by modern discoveries but by no means superseded, and that the existence of the Ether of Space is not discredited but rather upheld.

Let it be understood that in so far as precise physical measurements are made more complex by the electrical theory of matter, in combination with the finite speed of etherial wave-propagation, and by the corresponding changes in shape and in effective mass of bodies, no one has any complaint nor any feeling but one of congratulation on the enlargement of the boundaries of physical knowledge.

Real complications of that sort are wholesome and invigorating, and the deductions based upon them belong to genuine physics. Moreover, in so far as certain other formulæ associated with the Principle of Relativity are used for purely physical purposes to enable calculators to reach conclusions by a short cut, I have nothing to say against that negative principle, except that it is rather a blindfold method which I

think will soon be superseded, when the fashion has passed. It may be even regarded as comparable in utility to the second law of thermodynamics, which has been used by philosophers and theologians, with doubtful security, to justify conclusions in eschatology. But if technical theorems or handy formulæ are employed to confuse elementary ideas, to deny straightforward dynamical conceptions, and to justify the deduction of portentous consequences about space and time, so as to complicate not merely details of measurement but the whole of our physical conceptions, then it is time to protest.

The postulate that axes of reference can be shifted and chosen arbitrarily without making any real difference to phenomena is a perfectly reasonable one, and in Einstein's hands, in combination with the fundamental FitzGerald-Lorentz factor $(1 - v^2/c^2)$ representing the connection between matter and ether, has enabled certain deductions to be simply made.

Whether a body moves through stationary ether, or whether ether streams past a stationary body, makes no difference. Granted that, and granted also that light travels at a constant speed through free ether, and we have all that is needed for the original Einstein deductions; which really depend for their interest not on the assumption of trivial axioms but on the ingenious employment, throughout, of the above factor. Recently we have learnt that he has pushed the liberty of choosing co-ordinates further, and by endowing his standard with acceleration, say the acceleration due to gravity at any particular place, and again by consistently using the Lorentz factor for all moving matter, he appears to have made astronomical deductions of interest and importance. For by this means gravitational influence can be made to disappear from a specification of motion, and an accelerated body can move with reference to a suitably accelerated standard as if under no force.

All honour to the ingenuity of the conclusions thus deduced, in so far as they are confirmed by observation; but let us not stultify ourselves by thinking that the body is really subject to no force, and freed from acceleration, because of some mathematical artifice adapted to co-variant or relativist treatment. Use a falling stone, or a cloud of falling stones, as the

spacial frame of reference, and extensive modifications will be introduced into the equations of motion for a projectile or a cyclone,—or, for the matter of that, a railway train. That fruitful points of view can be gained by aid of so artificial a procedure is remarkable, but that the scheme of nature is really improved by any such intricate device is almost unthinkable. We have no right to complicate the philosophy of nature, or to confuse metaphysicians with analytical refinements about those pure abstractions space and time, merely because of an admitted practical difficulty about employing material objects for their precise measurement. The real complication belongs to the material objects, and depends on recently discovered properties of matter; those properties should not be masked, and their study evaded, by arbitrarily complicating the framework of reference. The procedure of Procrustes, who fitted the occupant to the bed instead of *vice versa*, has in this application some merit.

Concerning the Newtonian idea of absolute motion: It is infinitely unlikely that any piece of matter is at absolute rest. For motion is one of the attributes of matter; no particular velocity is more likely than another; and only one precise velocity is zero. But on the other hand it is unlikely that the whole ether of space is in a state of locomotion—in any sense that we can attribute to the words. We may venture to assume that the ether of space, if of infinite extent, is at rest: *i.e.* we may take it as our definition or standard of rest. Strain and stress belong to the ether, not motion. When a piece of matter is strained, *i.e.* altered in shape or size, the configuration of its particles is altered, they are simply moved into fresh positions, but all stress between them is in the connecting mechanism, *i.e.* in the ether. Motion is a fundamental property of *matter*. We have no reason to attribute motion to the continuous medium in which matter moves. For if the ether were moving as a whole, no conception of such movement could be made—nor would it make any difference to anything that happened in it. And it is unnecessary and gratuitous to suppose, without any evidence, that it is streaming in such a way that different parts are going in different directions. Ether is the receptacle of static energy, as matter is of kinetic energy, and the straightforward assumption about it is that it is stagnant except for such intense internal con-

stitutional ultra-microscopic whirl as may be required to explain its elastic properties. The burden of proof rests on those who deny the simple postulate of absence of gross locomotion in the ether. In so far as the elastic stress concerned in potential or static energy has to be explained on kinetic principles, it is perfectly legitimate to postulate a circulatory or vortex structure for the ether, such as can account for its optical elasticity and its electric and magnetic relations. But such sponge-like vorticity is of the kind known as stationary motion, and does not appeal to us as motion at all, only as structure.

Motion relative to the ether of space is, therefore, the nearest approach to the idea of absolute motion that we are able to form ; and it is already possible to show that we know *something*, though not very much, about this motion.

The fact that the effective or apparent mass of a body is a function of its velocity squared, so that an expression for it contains the factor $(1-v^2/c^2)^{-\frac{1}{2}}$, and therefore has an increased value as the velocity of light is approached, is the fact we have to argue from.

On the strength of that I assert that the solar system is not moving through the ether at any excessive speed near that of light ; at least that there is no component of such great speed in the plane of the ecliptic. For if there were, the earth's mass would vary according to its position in the orbit : and this ought to be perceptible as a definite perturbation, causing the orbit to revolve in a hypocycloidal manner.

That an effect would be perceptible can be shown in elementary fashion, thus : Suppose the sun's proper motion in the plane of the ecliptic to be within 1 per cent. of the velocity of light, so that $(1-v^2/c^2)^{-\frac{1}{2}}$ would equal 7, then the whole solar system's apparent mass would be 7 times what it would be if stationary.[1] It may be said that there would be no means of detecting a uniform effect of that sort.

[1] The simple factor for inertia-increase $m/m_0 = (1 - v^2/c^2)^{-\frac{1}{2}}$ is a fairly close approximation to a more complicated expression, such as is quoted in my book on "Electrons," pp. 133 and 225,

$$\frac{m}{m_0} = \frac{3}{8(1 - \cos 2\theta)}\left((2 - \cos 2\theta) - (2\cos 2\theta - 1)\frac{2\theta}{\sin 2\theta}\right)$$

where $\sin\theta = v/c$. This is supposed to be more correct, but the simple expression $m = m_0 \sec\theta$ will do for the illustration in the text. The completer expression gives 6·4 instead of 7, when $\sin\theta = {\cdot}99$, and it always works out a trifle less than

Let that pass for the present. But now reckon the earth's mass, at opposite points in the orbit, when it is moving parallel to a component of the sun's way at the known rate $10^{-4}c$. On one side of the orbit the denominator of the above fraction would be $\sqrt{(\cdot 0202)}$, and on the other side $\sqrt{(\cdot 0198)}$; so that the difference due to the extreme variation in the earth's apparent mass in the course of six months would be 7 per cent. of the real mass, or $\frac{1}{100}$th of the average effective mass, of the earth. Such a difference in the centripetal acceleration would have conspicuous effects, on the reasonable hypothesis, argued below, that the extra or spurious mass is not subject to gravity. For the sun's perturbing force on the moon is just about one-hundredth of the earth's main attraction[1]; and as a result Newton deduced an apsidal progression at the rate of $1\frac{1}{2}°$, if not $3°$, per revolution (*Principia*, Book I. section ix.).

Hence we can say that the solar system is not advancing through the ether at anything near the velocity of light. It is something if we can definitely make such an assertion as that on physical grounds. It shows that motion through the ether is at least not meaningless.

Again, when some spurious or extra inertia is actually being observed experimentally, in electrons or α particles flying in a vacuum, the aspect of the vacuum tube should make some difference, if the solar system were travelling at an enormous speed. Probably most people would agree that it is wholly unlikely that the visible cosmos is careering through space at any immense velocity: but why can we say it is

sec θ. A good approximation, when θ is moderate, is $1 + \frac{2}{5}\theta^2 + \frac{19}{105}\theta^4$. Another still closer one is $\frac{\tan\theta - \theta}{\frac{1}{3}\theta^3}$. See Alfred Lodge's *Diff. Cal.* p. 184.

Historical Note. Heaviside's expression for the coefficient of $\frac{1}{2}v^2$ in the value of kinetic energy for a charged sphere was first given in *Phil. Mag.* April 1889. (See Heaviside's Electrical Papers, Vol. II., p. 514, or my book on *Electrons*, p. 225.)

J. J. Thomson's expression for the coefficient of v in the value of momentum is contained in his *Recent Researches in Electricity and Magnetism*, p. 21, published in 1893.

[1] The sun's pull on the moon is greater than the earth's pull, the ratio being $S/E : (R/r)^2$, but the perturbing effect of the sun's pull is much less; for the ratio of the sun's perturbing force to the earth's controlling force is

$$2S/E : (R/r)^3 = 6 \times 10^5/400^3 = 1/100.$$

unlikely? According to the doctrine of relativity, as held by a school of theorists who deny the existence of an ether, we ought not to be able to say anything about it: we ought not to be able to deny that we were travelling a million times faster than light, except that both assertion and denial would be considered meaningless.

Of a revolving body we can assert that it is not spinning very fast, we can even say at what absolute rate it is spinning, because that kind of motion involves acceleration—centripetal acceleration; and no one appears to doubt that acceleration can be observed. So it is possible to observe and measure what may be called absolute rotation. For instance, the period of rotation of the earth or of Jupiter can be deduced from its shape. But then rotation of a piece of matter involves relative or differential motion of its parts, and the question of absolute motion does not really arise. But with pure translation, we have no criterion that the speed is not infinite, save and except the theoretical deduction—not accepted by relativity theorists—that if a piece of matter moves in a quite isolated manner and obedient to the first law of motion, then the fact or the supposition that it is moving through a stagnant medium, the ether, compels certain imaginable even if not readily observable consequences to arise. Yet these may in due time be looked for.

It is becoming known that the Principle of Relativity is acquiring merit and establishing its position by some brilliant deductions made by Professor Einstein.

One relates to the influence of a gravitational field upon a ray of light. No such effect is yet known, but if it can be observed, and if in amount it agrees with the calculated value, it will be a splendid triumph. Exactly how much of the theory will by that possible discovery really be established will then have to be considered. Meanwhile we can await the result of the proposed astronomical observations in May 1919.

The other deduction is an explanation of the outstanding discrepancy between gravitational theory and observation in the progression of the perihelion of the planet Mercury, its amount being 43 seconds of arc per century. Now, this progression is undoubtedly real, and it behoves us to consider what are the physical facts upon which its supposed recent explanation is really and solidly based.

I wish to argue that the real fact lying at the base of the explanation is the varying mass of the planet as a function of its speed through the ether, and is not dependent on the theory of relativity as such. I contend that this extra or spurious inertia due to motion does not really appertain to matter, but to the medium in which it moves, and that therefore it is extremely unlikely to be subject to gravitation.

A sphere moving through a perfect fluid is known to have its mass apparently increased by 50 per cent. of the mass of fluid displaced. A cylinder still more. This apparent mass is really due to a distribution of fluid pressures. Virtually more matter has to be accelerated, but the effect *in no way alters the floating or sinking forces acting on the body.* Nor does it encounter any resistance such as dissipates energy.

Extra inertia due to motion through the ether is not so simple as that, it is more like an approach to the critical speed (easily attained by sharp edges) at which fluid finds it difficult to get out of the way; but however it be explained in detail the extra inertia is something belonging to the ether, not to matter, and therefore ought not to influence the body's weight. The ether must be assumed to be the vehicle of gravitation, but not to be affected by it—at least not until it is constitutionally contorted into the peculiar condition of an electron. By this contortion or strain, whatever its nature, a stress appears to be set up which *is* the gravitational field. Parenthetically one may remark that this stress thus set up is no new thing, electrons cannot be manufactured; their tension simply exists, inversely as the distance, throughout space.

So I argue that in the extra inertia due to motion through ether we have mass or virtual mass without weight, and that an increased inertia of this kind must diminish acceleration in much the same way as a lessening of gravitational pull would diminish it. Thus the centripetal acceleration acting on a planet will not only be slightly less than in Newtonian theory, but will vary with its position in the orbit; and this must give rise to a definite perturbation.

In an article in the August number of *The Philosophical Magazine* for the present year I reckon the actual effect of extra and fluctuating mass upon the orbit of a planet, and show that it introduces a revolution of the orbit in its own

plane or a progression of the apses. And this without any reference to the theory of relativity.

To summarise briefly the result of the calculation referred to :—

If the sun's proper motion through the ether is taken as about two-thirds that of the earth in its orbit and as directed to an apex whose latitude or inclination to the ecliptic is about 60° (somewhere near Vega), *i.e.* if the cosmic star group containing the Sun is regarded as fixed, then the apse progression of Mercury comes out only 2″·4 per century ; which though not hopelessly of the wrong order of magnitude is much too small. But if the whole system were advancing in a certain direction, viz. towards longitude 294°, with three times the earth's orbital velocity, as one of Kapteyn's star-groups, then, while there would be nothing excessive in the apsidal progressions of other bodies in the solar system—for the components acting on the earth and Venus would be small—the revolution of the orbits both of Mercury and Mars would come out in accordance with observation.

NEWTON AND THE COLOURS OF THE SPECTRUM

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Every student of Optics knows that when Sir Isaac Newton discovered the spectrum he divided it into seven colours, namely, red, orange, yellow, green, blue, indigo and violet, and most students of Optics have at the same time wondered why indigo was included in this list of colours. For if we look along the spectrum from the one end to the other, screening off everything except the particular strip we are examining, we see in succession red, orange, yellow, yellow-green, green, green-blue, blue, blue-violet, and violet, but between the blue and the violet there is nothing sufficiently different in kind to warrant the introduction of a new name; the one colour merges gradually into the other, and the intermediate shades are clearly mixtures of blue and violet. Indigo, as we know it from the water-colour paintboxes, is an inky blue, a blue inclined to black, not one inclined to violet. Indeed the late Prof. S. P. Thompson gave it as his opinion in his lectures that indigo was inclined more to green than to violet, and in this opinion, I think, every one will concur. What did Newton understand by the colour indigo, and why did he attach such importance to it?

There is no doubt, I think, that Newton introduced his seventh colour partly because his own colour vision was slightly abnormal, partly from a fancied analogy between the spectrum and the musical scale, influenced no doubt by Kepler's doctrine of harmonies. Also I think it highly probable that it was on account of this fancied analogy that he regarded the dispersion of all transparent substances as the same, and did not attempt to correct the chromatic aberration of lenses. It is always interesting to consider the mistakes made by eminent men, especially when they are so important for the history of science as this one was. So I propose in this paper to give an account

of the evidence bearing on the matter. It is little known, at least to the English-speaking peoples, because Newton's *Opticks* has not been printed in English since 1730, and because the part of the *Lectiones Opticæ* bearing on the subject has never been printed in English. Newton's *Opticks* is for sale at present only in the German translation.

We shall first of all consider Newton's colour vision. This explanation has been put forward by Edridge-Green. In his theory of colour vision he does not accept the usual classification of red-blind, green-blind, etc., but divides colour vision into seven classes—monochromic (totally colour-blind), dichromic, trichromic, tetrachromic, pentachromic, hexachromic, and heptachromic—according as the subject sees one, two, three, four, five, six, or seven distinct colours in the spectrum. According to his theory normal colour vision is hexachromic; the normal individual sees the six colours, red, orange, yellow, green, blue, violet. The normal individual sees, of course, intermediate shades between these colours, but these six colours appear to be primary. The heptachromic class see indigo as a primary colour, and have a decidedly better colour perception than the hexachromic. It is not merely a matter of colour nomenclature; the heptachromic really see something at this region in the spectrum, which the hexachromic do not see. Newton, according to Edridge-Green, was a heptachromic.

I was at first somewhat sceptical about this explanation, because Newton states in a passage to be afterwards quoted in full that his eyes were not so critical for distinguishing colours as the eyes of his assistant; also I gathered from Edridge-Green's papers that heptachromics occurred rarely, and the probability of both Newton and his assistant being heptachromic seemed to me too small. But out of eighteen cases—members of the staff and advanced students of the university—that I have recently examined I find that three at least quite definitely see indigo as a separate colour, and there is a fourth case doubtful. The others are normal. So that the peculiarity seems to be fairly common.

The examination was made at first by projecting a spectrum about eight inches long on a screen. The student under examination was asked whether he saw a colour between blue and violet with as much right to a special name in the spectrum as orange has, if so how would he describe it, and what were

its limits. The spectrum was that of a carbon arc with a somewhat sudden increase of brightness in the violet, and I was afraid this increase of brightness might be misleading. So those who said they saw indigo were taken to a spectroscope, shown a very good continuous spectrum, and asked again to mark the limits of indigo; also two of the four were shown monochromatic patches, and asked to say rapidly whether these were blue, indigo, or violet, an attempt being made to confuse them.

I had no difficulty in convincing myself, somewhat to my astonishment, that the four students referred to have a much better power of discriminating hues in this part of the spectrum than I have, and my colour vision is normal. They all objected to the word indigo, and chose dark blue as a more suitable name for the new colour; they all said it was liker blue than violet, and their readings for the boundary between it and blue were, in $\mu\mu$, 457, 455, 465, 465, 465, the first two readings being by the same observer, and all the readings being made rapidly and without hesitation. At this part of the spectrum I see only blue merging into violet, and I find it extremely difficult to say where the one begins and the other ends, getting anything between 437 and 460 $\mu\mu$.

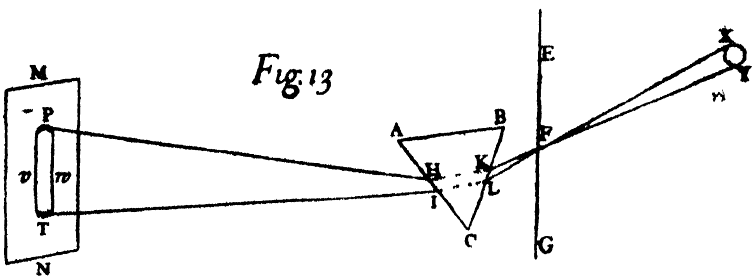
In his *Opticks* Newton refers to his assistant in connection with the discrimination of colour, and in the *Lectiones Opticæ* speaks of himself as relying on the judgment of others. He would naturally choose others with a keen sense of colour, but he would not, I think, have committed himself so definitely had he not seen indigo to some degree himself. I am therefore inclined to class him as a doubtful heptachromic and his assistant as a decided heptachromic.

Before describing Newton's experiments I should like to dismiss a suggested explanation which has been frequently offered to me by students, namely, that indigo was introduced to make out the seven colours, that Newton was anxious to have seven colours in his list on account of some mystical significance attaching to that number. The words of Helmholtz[1] are, I think, quite conclusive on this point, namely, that to the observer with normal colour vision orange-yellow, yellow-green, and sea-green, *i.e.* green-blue, are at least as different from their neighbours in the spectrum as indigo is

[1] *Handbuch der Physiologischen Optik*, 2nd edition, p. 288.

from blue and violet. In making out a list of the seven most important colours they would have prior claims to indigo.

It was in the year 1666 that Isaac Newton carried out at Cambridge the experiments on the decomposition of white light by a prism which were to inform us as to the true nature of colour. As source of light he used the brightest of all possible sources, namely, the sun. Its rays were admitted into a darkened room through a hole in a shutter. In the first experiment on the spectrum described in the *Opticks*, *i.e.* expt. 3, Bk. I. Part 1, this hole was circular and about ¼ in. in diameter. The room thus acted as a pinhole camera, and an image of the sun in its natural colours would under ordinary circumstances fall on the opposite wall of the room. If, however, a glass prism was placed inside the room close up to the hole, with its refracting edge horizontal and pointing downwards, so as to receive the rays, they were refracted, and the image of the sun appeared higher up on the wall, as is shown in the figure,



which is taken from the *Opticks*. At the same time it changed its appearance. Instead of appearing as a single yellow disc, when it was received on a sheet of white paper it became a vertical strip with straight sides and semi-circular ends. The strip was 18⅓ ft. from the prism. Its breadth was 2⅝ in., just the same as the diameter of the image would have been had the prism been taken away. If the prism was rotated about a horizontal axis, the image moved down and then up the wall, and its length altered, being a minimum when the image was as far down as possible. The whole length then was 10¼ in. and the length of the straight sides was 8 in. The strip was coloured, the upper end being violet, the lower end being red, and the intermediate portion being blue, green, and yellow.

In this way Newton discovered the prismatic spectrum,

and a large part of the first book of his *Opticks* is devoted to proving what is a commonplace now, namely, that the white light of the sun was decomposed by the prism into light of different colours, and that each of these colours had its own definite refrangibility. The prisms he used had a good dispersion and gave a spectrum of a length of 2°, and on pp. 59–60[1] he describes a determination of the indices of refraction of two of them by a quadrant (an anticipation of Hadley's sextant, but of somewhat different design), which gave results $\frac{8047}{5188}$ and $\frac{8157}{5262}$ or in round numbers $\frac{31}{20}$.

With the arrangement shown in fig. 13 owing to the diameter of the pinhole being $\frac{1}{8}$ in. the spectrum would not be sharp, but would have a penumbra $\frac{1}{8}$ in. deep round its boundary. Nevertheless on account of its simplicity this arrangement seems to have been used oftener than any other. In expt. 5, p. 28, however, we find Newton taking steps to remove the penumbra, which he did by placing a lens at the hole in the shutter. The dark room then acted like an ordinary photographic camera, the lens throwing a sharp image of the sun on the opposite wall.

Even with a lens at the hole the spectrum is not pure, for every colour forms its own image of the sun. Consequently we have a series of coloured discs, red, orange, yellow, green, blue, violet, all with their centres at different points on the same straight line and all overlapping. The overlapping is greatest on the line of centres and least at the edges of the spectrum. The edges are, of course, defined by the two common tangents to all the discs. The spectrum is thus very impure in the middle, but increases in purity towards the edges. On pp. 46 and 47 we find Newton discussing the impurity of his spectra. He ascribes it to the diameter of the sun not being small enough, and suggests placing an opaque body with a round hole in the middle of it without doors, at a great distance from the prism in the direction of the sun. The coloured images then produced inside the darkened room would not correspond to the whole disc of the sun, but only to that portion of it seen through the round hole of the opaque body. He suggests using a lens with this arrangement, the lens to be placed close to the prism and to throw a sharp image of the round hole in the opaque body on the wall of the room. And then, having

[1] The references are to the first edition.

reached this stage, he points out that the opaque body is not necessary; the hole may be quite as well made in the shutter itself and focussed by a lens on the other side of the room. So he arrives at the ordinary arrangement in use to-day.

We find this arrangement described in expt. 11 (p. 47) and represented in fig. 24. F is a round hole in the shutter, MN

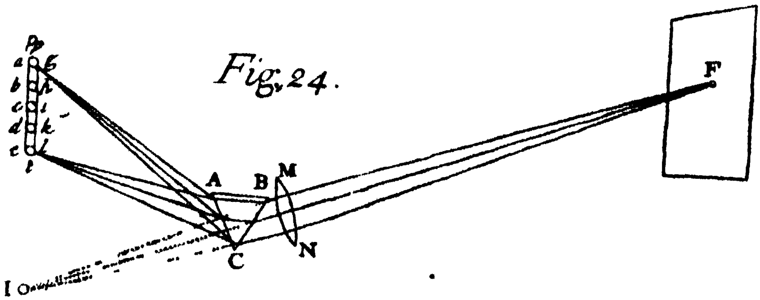


Fig. 24.

is a lens at a distance of about 10 or 12 ft. from the window, which throws an image of the hole on a sheet of white paper at I, which is at a distance varying between 6 and 12 ft. from the lens, according to the lens used. The introduction of the prism causes the formation of the spectrum pt. The diagram shows a circular hole at F, but on p. 49 we find:

> "Yet instead of the circular hole F, it is better to substitute an oblong hole shaped like a long Parallelogram with its length Parallel to the Prism ABC. For if this hole be an Inch or two long, and but a tenth or twentieth part of an Inch broad or narrower; the Light of the Image pt will be as Simple as before or simpler, and the Image will become much broader, and therefore more fit to have Experiments tried in its Light than before."

The use of a slit is again described on p. 81 (expt. 1, Bk. I. Part 2), this time without a lens at all.

We have thus the modern arrangement of slit, lens, and prism, and the question arises, why did Newton not discover the Fraunhofer lines? With a slit $\frac{1}{20}$ in. broad and a spectrum 8 in. long they should be visible when looked at from a suitable distance, if a modern prism were used. Newton's failure seems partly to have been due to his prisms. He apparently polished them himself, and the glass was of poor optical quality; one prism he mentions as having "Veins, running along within

the Glass from one end to the other." Also it seems that he did not work so often with the slit and lens arrangement for producing spectra as with the pinhole or ordinary camera methods; the motion of the sun in the sky would cause less trouble with the two latter arrangements. Besides, the years 1665–6 were a time of such great mental activity on the part of Newton—they saw the discovery of the binomial theorem, differential and integral calculus, to say nothing of the work on gravitation—that he must have experimented at high speed. So he missed the lines, and it was reserved for Fraunhofer to discover them 150 years later.[1]

In addition to the sun, we know from p. 26 of the *Lectiones Opticæ*, published in Latin after his death, but not from his *Opticks*, that Newton examined also the spectrum of Venus, using the "ordinary camera" method. He says that the spectrum was not very bright but still easily visible, comments on its extreme narrowness, and states that he believes it would be possible to observe the spectra of the stars of the first magnitude, especially Sirius, in the same way. From a subsequent page we infer that this experiment had been successfully tried.

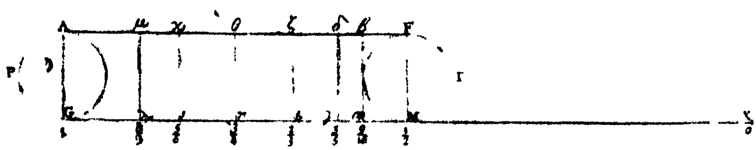
So much for Newton's experimental arrangements. We come now to discuss his nomenclature for the colours of the spectrum.

When first he describes the spectrum (pp. 22 and 23 of the *Opticks*) there are only five colours mentioned, namely, red, yellow, green, "blew," violet, and again on p. 87 he refers to the spectrum as showing the usual colours on a piece of paper, namely, red, yellow, green, blue, violet; blue this time is spelt in the modern way. This, I think, is the natural description to be expected from a man with normal colour vision who sees a bright spectrum spread out as a whole before him for the first time; only when the spectrum is examined strip by strip,

[1] The question as to why Newton missed the Fraunhofer lines has been considered by G. Griffith (*Brit. Ass. Rep.* 1885, p. 941) and by Alexander Johnson (*Trans. Roy. Soc. Canada*, **9**, p. 49, 1891). Both authors point out that it was not due to his using a circular aperture instead of a slit, and that the textbooks are wrong on this point. Griffith states that the arrangement was good enough to show the lines, but that possibly part of the work was left to an assistant, and that the latter missed them. Johnson repeated Newton's experiment under similar conditions, and saw plainly ten Fraunhofer lines when the slit was $\frac{1}{10}$ in. wide (the greatest width mentioned by Newton). Newton's arrangement of a circular hole $\frac{1}{10}$ in. in diameter was found by Johnson to show four lines very distinctly.

the neighbouring colours being screened off, are the intermediate colours visible. On p. 92 we find the first mention of indigo; on account of its importance for our purpose I reproduce the entire passage together with its accompanying figure:

Fig 4



"When I had caused the rectilinear line sides AF, GM, of the Spectrum of Colours made by the Prism to be distinctly defined, as in the fifth Experiment of the first book is described,[1] there were found in it all the homogeneal Colours in the same order and situation one among another as in the Spectrum of simple Light, described in the fourth Experiment of that Book. For the Circles of which the Spectrum of compound Light PT is composed, and which in the middle parts of the Spectrum interfere and are intermixt with one another, are not intermixt in their outmost parts where they touch those rectilinear sides AF and GM. And therefore in those rectilinear sides when distinctly defined, there is no new Colour generated by refraction. I observed also, that if anywhere between the two outmost Circles TMF and PGA a right line, as $\gamma\delta$, was cross to the Spectrum so as at both ends to fall perpendicularly upon its rectilinear sides, there appeared one and the same Colour and degree of Colour from one end of this line to the other. I delineated therefore in a Paper the perimeter of the Spectrum FAPGMT, and in trying the third Experiment of the first Book, I held the paper so that the spectrum might fall upon this delineated Figure, and agree with it exactly, whilst an Assistant whose Eyes for distinguishing Colours were more critical than mine, did by right lines $\alpha\beta$, $\gamma\delta$, $\epsilon\zeta$, etc., drawn cross the Spectrum, note the confines of the Colours, that is of the red M$\alpha\beta$F, of the orange $\alpha\gamma\delta\beta$, of the yellow $\gamma\epsilon\zeta\delta$, of the green $\epsilon\eta\theta\zeta$, of the blue $\eta\iota\kappa\theta$, of the indico $\iota\lambda\mu\kappa$, and of the violet λGAμ. And this operation being divers times repeated both in the same and in several Papers, I found that the Observations agreed well enough with one another, and that the rectilinear sides MG and FA were by the said cross lines divided after the manner of a musical Chord. Let GM be produced to X, that MX may be equal to GM, and conceive GX, λX, ιX, ηX, ϵX, γX, αX, MX, to

[1] *I.e.* the "ordinary camera" method is used.

be in proportion to one another, as the numbers 1, $\frac{8}{9}$, $\frac{5}{6}$, $\frac{3}{4}$, $\frac{2}{3}$, $\frac{3}{5}$, $\frac{9}{16}$, $\frac{1}{2}$, and so to represent the Chords of the Key, and of a Tone, a third Minor, a fourth, a fifth, a sixth Major, a seventh, and an eighth above that Key. And the intervals M*α*, *αγ*, *γε*, *εη*, *ηι*, *ιλ*, and *λ*G, will be the spaces which the several Colours (red, orange, yellow, green, blue, indico, violet) take up."

This account in the *Opticks* was, of course, published in 1704, thirty-eight years after the experiments were made, and eight years after Newton became Warden of the Mint, and represents his final view of the matter. The parallel passage in the *Lectiones Opticæ*, or Optical Lectures, which according to their preface would have been published by Newton about 1671, had it not been for his dislike of controversy, is much fuller, and shows us his views in course of formation.

The passage begins on p. 239[1] and describes how the spectrum was received on a piece of paper, and the boundaries of the different colours as well as their regions of greatest purity marked on the paper, the "ordinary camera" arrangement being used. Then on p. 240 the results of this investigation are given under five heads:

(1) The boundary line between blue and green divides the length of the spectrum into two equal parts, blue (cæruleus) and violet (violaceus) composing the one part and green (viridis) and red (rubeus) the other. The colour between blue and green is referred to as thalassinus—sea-green.

(2) A line drawn across the spectrum at the richest part of the green divides the spectrum in the ratio 3 to 5.

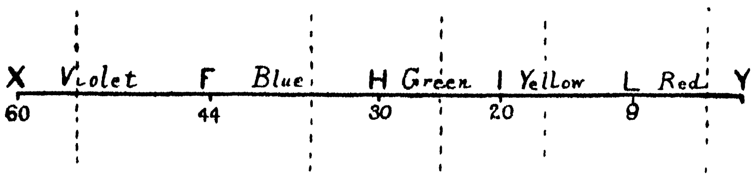
(3) The length of the green is one-sixth the length of the spectrum.

(4) The boundary between blue and violet (this time purpureus is used for violet) or the most perfect indigo (indicus) is $\frac{1}{12}$ of the length of the spectrum distant from

[1] The reference here is to the Latin edition published in 1729, two years after Newton's death. The lectures were delivered at Cambridge in 1669, and the Latin original was deposited at the time it was read in the archives of the university. The lectures are divided into two parts. The second part, from which the matter here cited is taken, relates to the doctrine of colours, and was recast and improved by Newton in the *Opticks*. The first part is preparatory to the second, and contains much less in common with the *Opticks*. It was published by itself in an English translation in 1728.

the boundary between red and yellow (flavus) or the most perfect orange (citrinus).

(5) The distance from indigo to orange is divided by the boundary of the green and blue in the ratio 2 to 3, the line of division being nearer the indigo.



He states that he arrived at these results not so much trusting to his own perception, but relying on the judgments of others (*aliorum judiciis fretus*). He next exhibits the results in a diagram, fig. 38 of the *Lectiones Opticæ* ; the figure above is a somewhat simplified form of this diagam. The diagram shows the whole spectrum divided into sixty parts, and the numbers represent the distances of the points of separation of the colours from the red end. The diagram gives only the five " insigniores colores," as they are referred to. So far orange and indigo have no more claim to importance than sea-green has.

We come now to the reason for their addition to the other five. The purest green lies midway between H and I in the position shown by the dotted line (in this passage in the original all the letters in the text have accidentally been shifted one place to the left, but it is easy to make the necessary correction), but the purest violet, blue, yellow, and red do not occur in the middles of their respective spaces. The purest blue and yellow are nearer the green, and the purest violet and red nearer the ends of the spectrum in the positions shown by the dotted lines. Since the intervals between the purest blue and violet and between the purest red and yellow are about one-third greater than the intervals between the purest green and blue and purest green and yellow, in order to divide the image more elegantly into parts (*quo imago elegantius in partes inter se proportionales distinguatur*) it is advantageous to admit two other colours into the number of the five more important colours, namely, orange between red and yellow and indigo between blue and violet.

Then Newton says he has investigated the limits of the

additional colours, and finds that the spaces occupied are connected with the lengths of a vibrating string giving the notes of the octave. This is illustrated by a figure which is practically the same as the fig. 4 from the *Opticks*, which we have reproduced. Only here the statements are rather more definite; the notes of the octave are referred to as sol, la, fa, ut, re, mi, fa, sol; the spaces $\mu\kappa\iota\lambda$ and $\delta\beta\alpha\gamma$ of fig. 4 are said to give the two semitones, while the three spaces in the middle occupied by blue, green and yellow, and the end spaces occupied by violet and red, give the five tones. Also the purest part of each colour is now said to be in the middle of its space.

In the next section Newton states that owing to its not being possible to observe the limits of the colours sufficiently accurately, they may really differ slightly from the positions given above, and he consequently suggests another distribution; put "eleven mean proportionals" between the ends of the spectrum and let $\alpha\beta$, $\gamma\delta$, $\epsilon\zeta$, $\eta\theta$, $\iota\kappa$, and $\lambda\mu$ in fig. 4 be respectively the second, third, fifth, seventh, ninth, and tenth of these proportionals, *i.e.* multiply XM in fig. 4 by $\sqrt[12]{2}$ to the second, third, fifth, seventh, ninth, and tenth powers, and this distribution also satisfies the observations within experimental error. To compare the two distributions he supposes OM = 180; then according to the first one the limits of the colours are at

360, 320, 300 270, 240, 216, 202½, 180,

and according to the second

360, 321, 303, 270, 240, 214, 202, 180.

Then there are two extremely interesting sentences comparing the merits of the two distributions which may be freely rendered as follows:

"I really prefer the former one, not because it agrees with experiment better, but because it may perhaps suggest analogies between harmonies of sounds and harmonies of colours (such as are not wholly unknown to the painters, but which I have not yet studied sufficiently myself—qualium pictores non penitus ignari sunt, sed ipse nondum satis perspectas habeo). Such a connection appears more plausible if we note that the affinity between the extreme red and violet, the ends of the spectrum, is of the same kind as between the first and last notes of the octave."

Any one comparing the accounts given in 1669 and 1704 must see that in the earlier year Newton believed that an analogy between the spectrum and the musical scale might contain possibilities of development, but that this belief was not realised. What was the belief due to? It is probable that Kepler's *Harmonices Mundi* had something to do with it. This is the book which is memorable as having announced to the world Kepler's third law, that of the relation between the planetary periods and distances, and which must have been known to Newton about 1669 owing to his explanation of the third law. Miss Clerke, the astronomer, in the biography of Kepler in the *Encyclopædia Britannica*, characterises the book as an extraordinary production, and states that its main purport was "the exposition of an elaborate system of celestial harmonies depending on the various and varying velocities of the several planets, of which the sentient soul animating the sun was the solitary auditor." Some of the reasoning certainly does appear extraordinary; Kepler not only makes the Deity anthropomorphic but also a mathematician. "Cum autem Deus nihil sine Geometrica pulchritudine constituerit" (p. 194, Bk. V.) is one of the arguments. But I think it is possible to regard the work more sympathetically than this.

Any one wishing to "explain" the universe in Kepler's time had not the analogy of the pendulum, the ideas of force and acceleration, electrons, or the mathematics of wave motion or any of the rest of the modern imagery to his hand. He was restricted to the properties of the geometrical figures and to the laws of harmony, *i.e.* of vibrating strings, which had been discovered by Pythagoras well nigh 2,000 years before. A physical law became necessarily a "harmony" because the first physical laws discovered were those of harmony. Pythagoras, who was the earliest mathematical physicist, and who probably saw the possibility of nature obeying mathematical laws, lived at a time when ideas were not clearly distinguished from things themselves. So the metaphor ran away with his followers, and by a peculiar confusion of thought it became necessary, for example, for the planets actually to give out musical notes as they revolved in their orbits. But there was enough truth seen in Pythagorism to keep it alive, and it had, of course, an immense tradition. So it is not to be wondered at that Kepler sought to elaborate and restate it. His attempt

undoubtedly led to the discovery of his three laws, and I think that his views, together with the popular way of referring to colours as "harmonising," must have had some weight with Newton; Newton's own theological writings show he was not averse to mystical speculation. Kepler's ways of thinking appear foreign to physicists nowadays, because his contemporary Galileo inaugurated the experimental method and revolutionised scientific thinking.

At the present day there is much confused thinking about the harmony of colours and the psychological effect of colours. Apart from the eloquent utterances of the art critics, which fill pages, but which melt away into a few bald, often mutually contradictory sentences in the hands of the physicist who tries to reduce them to concrete, definite statements, there are those who believe in the possibility of "colour music," *i.e.* that by witnessing the projection of colours in rhythmic succession on a screen we are able to experience the same enjoyment that we do in hearing music. For this purpose instruments called colour organs have been devised. Then there are some who believe that colour in itself has a psychological effect, that blue is cold, yellow sickening, green restful, red maddening, etc. A curious instance about which one would like to hear more is given in support of this in an article by Maurice Leblanc on the mercury arc lamp in the *Journ. de Phys.* (4) **4**, p. 416, 1905. He states that in a certain department of the Lumière factory at Lyons red lights were employed. They had a bad psychological effect; the staff became "ingouvernable." Green lights were then substituted with the best results. Also we have a musical composer, Herman Darewski (*Pearson's Mag.*, Dec. 1916), who regards colour and tone as "being brothers and sisters" and who states he finds composition facilitated by wearing coloured glasses. Deep blue and deep mauve, for example, make him depressed, grass-green soothed, pale pink fanciful, purple doleful, etc. All the above reduces, I think, to the simple facts, that by education and training we find certain combinations of colours harsh and other combinations pleasing, and that colours suggest definite things to us; red suggests blood or a fire, orange sunshine, green suggests green fields and trees, bluish-white ice or moonlight, etc. Beyond this statement there is nothing definite to go upon.

It will be noticed that in the passage cited above Newton

used a circular aperture, and consequently obtained a spectrum very impure in the middle, but pure towards its upper and lower edges ; yet he says that if " a right line was cross to the Spectrum, so as at both ends to fall perpendicularly upon its rectilinear sides, there appeared one and the same Colour and degree of Colour from one end of this line to the other." In order to test this I fitted an iris diaphragm to a spectroscope and examined a continuous spectrum, adjusting the diaphragm so as to give the spectrum the same dimensions as Newton's had. At certain points the colour did not appear the same on the same vertical, but I was not sure how far the result was vitiated by internal reflection in the spectroscope. So I focussed a carbon arc on a circular aperture, and by means of a lens and large flint prism produced a very bright steady spectrum about 8 inches long, and of the same shape and degree of purity as Newton's. I saw then that in the middle the yellow overflowed into the orange, and the orange into the red. But others whom I asked gave contradictory answers. Thus it appears that Newton's statement as to the hue being the same on the one transversal is fairly true.

I next attempted to reproduce his measurements on the boundaries of the colours, using the same carbon arc as source, and projecting the spectrum on a screen. His prism gave a spectrum 2° long. I used in succession a crown prism giving $1\frac{1}{2}°$, and flint prisms giving $2° 49'$ and $3° 36'$, and in addition to my own observations had measurements made by my best " heptachromic." But in vain ; we could not get anything like an agreement between his results and ours. Our yellow was, for example, always much narrower than his, which occupied $\frac{8}{70}$ of the whole length of the spectrum.

It is interesting to note that in his analogy between the colours and intervals in the musical scale Newton gives the red end of the spectrum the shortest wave-length. Also the scale he uses does not agree with the diatonic scale ; the latter would, however, fit the observations just as well. The essential feature is that the widths of the orange and indigo strips are the semitones, and the widths of the other five colours the tones.

As is well known, Newton believed that the dispersion of a medium was proportional to its refraction, and that chromatic aberration was an insuperable obstacle to the development of the refracting telescope ; consequently he turned his

attention to the development of the reflecting telescope. We know from the *Opticks* he worked with a hollow prism filled with water, which should have given a spectrum only half as long as his glass prisms did, and Lucas in Liége, who worked with a crown glass prism, insisted that his spectrum was only three times the angular diameter of the sun in length, not five times as Newton found it. Why Newton should have persisted in error has always seemed strange, and Glazebrook has suggested in his biography in the *Encyclopædia Britannica* that possibly the water in Newton's hollow prism contained sugar of lead in solution ; this would increase the length of the spectrum, and make it liker the spectrum given by the glass prisms. We know that Newton sometimes worked with sugar of lead solutions.

But was the whole affair not merely a matter of strong preconceived ideas ? The determination of the variation of index of refraction with colour follows closely on the comparison of the spectrum with the musical scale, both in the *Opticks* and the *Lectiones Opticæ*. If the arrangement of the colours in the spectrum bore a fixed relation to the musical scale, it was obviously independent of the material of the prism. Thus, being indebted to Kepler for the idea that there must be a simple numerical law, and taking his stand on his own measurements, Newton perhaps believed that there were theoretical reasons demanding that the dispersion of all media should be the same. And the experimental evidence forthcoming was not strong enough to make him give up this belief.

DISPERSOIDOLOGY AND THE THEORY OF VON WEIMARN[1]

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SINCE Graham laid the first foundation stones of the structure, many significant additions have been made to our knowledge of the chemistry and physics of colloids. The invention of the ultra-microscope by Siedentopf and Zigmondy led to the investigation of Brownian movement by Smolukhovski, Svedberg, and Einstein, and the demonstration of the actual existence of molecules by Perrin. The theory recently elaborated by v. Weimarn is an important extension of the main framework of the science. Although Graham originally distinguished between crystalloid and colloid as distinct classes of substances, it has gradually become evident that these terms should rather be applied to differing states of matter, since, under suitable circumstances, many typically crystalline substances can be obtained in a colloid state. And it further appears that this condition is characterised by the development of a relatively enormous amount of surface, to which the distinctive properties of colloids are traceable.

V. Weimarn's researches completely demonstrate this view, and his theory defines the main conditions which determine the form in which a given substance shall occur. So that, provided the necessary circumstances can be realised, it should be possible to obtain a given substance in either desired condition. The study of colloids therefore develops increasingly into a consideration of specific surface, and the modifications of physical and chemical properties produced by its variation. For even chemical affinity appears as a function of the grain-size of solid and liquid bodies, as may be exemplified by the fact that gold in the finest state of subdivision is soluble in hydro-

[1] P. P. v. Weimarn, *Zur Lehre von den Zuständen der Materie.* Leipzig, 1914. 2 Bde.

chloric acid. Since all material bodies have at least three kinds of surface, that which separates them from other objects, their atomic surface and that of the electrons of which the atoms are composed, the study of the manifestations of surface energy embraces a universal field, to which v. Weimarn has given the name of "Dispersoidology."

Like most other fundamental precepts of science, v. Weimarn's theory is capable of simple expression. The form in which a substance is deposited from the gaseous or liquid state is conditioned by the number, N, of condensation centres produced, as determined by the equation:

$$N = K\frac{P}{L},$$

where P is the mass of the body about to be deposited, in excess of that which will remain in solution, and L is its solubility, or partial pressure, in the particular state of subdivision concerned. K is a factor which is proportional to the degrees of aggregation of the substances taking part in, or present during, the reaction, and to the viscosity of the reaction medium. Of these, solubility is the quantity which has the greatest influence on the form of the precipitate, and is, in fact, the decisive factor. Thus bodies like soda and copper sulphate, which are easily soluble, occur naturally in large crystals; while, at the other extreme, compounds which are almost insoluble, such as aluminium hydroxide and silicic acid, result in the gelatinous form.

For the production of the colloid condition it is essential that, in the medium chosen, L should be exceedingly small. Then, by varying the value of P, v. Weimarn has shown that five distinct states are produced:

I. Corresponding to solutions which deposit no precipitate for a long time, until after some years the solid phase appears in the form of macrocrystals.

II. Corresponding to solutions which deposit macrocrystals in a short time.

III. In which a precipitate separates in the form of crystalline growth figures (crystalline skeletons and needles).

IV. In which the precipitate appears in the form of plastic masses (so-called amorphous precipitates, curdy and gelatinous masses) which are composed of particles so small as to appear spherical under the microscope and ultra-microscope.

V. The state corresponding to the separation of a precipitate of which the individual grains cannot be differentiated with the aid of our present-day optical instruments. (Homogeneous gels and the first products of their decomposition).

The influence of concentration upon the form of the precipitate may be explained by the consideration of some examples which, with many others, have been fully worked out by v. Weimarn and his pupils.

The solubility, L, of barium sulphate corresponds to about N/20,000. In order to obtain, at need, a large value of P, the barium sulphate is produced by the double decomposition, $MnSO_4 + Ba(CNS)_2 = BaSO_4 + Mn(CNS)_2$, from the mixing of a litre each of solutions of equal molar strength. By changing the concentration from N/20,000 to N/10,000, no separation of barium sulphate is observed during many months. From N/5,000 to N/1,000, a precipitate falls in a short time in the form of macro-crystals. The progress of the reaction in the neighbourhood of the lower limit of concentration is thus described. In the course of the first two weeks an opalescence appeared in the liquid, though no separation of precipitate on the base of the vessel could be observed. After about a month an extraordinary fine deposit of precipitate appeared which might easily have been taken for dust. A month later the mass of the precipitate on the base of the flask was somewhat greater. After six months from the commencement of the experiment the liquid was poured off, the bottom of the flask cut out and examined under the microscope. With a magnification of 500, the particles were seen to consist of practically perfect crystals of apparently 2—2·5 cm. length and a width of from 0·5–1 cm.

With a concentration of N/3,000, the opalescence appeared in from 6–8 hours, and the precipitate had mostly separated within 24 hours. A concentration of about N/2,000 showed an opalescence in from 2–3 hours, and gave a precipitate within

10–12 hours; while changing the concentration from N/1,000 to N/500 reduced the period before the appearance of an opalescence from 3–5 minutes to one second, and the time of separation of the precipitate from within 2–3 hours to ½–1 hour. The precipitate obtained with a concentration of N/1,000 appeared, under a magnification of 15,000, to consist of a few parallelepipedal barium sulphate crystals (of which the largest were about 5 × 3 mm.) and of aggregates of the smallest particles which appeared spherical under the highest magnification. After the expiration of six months, the smaller particles had disappeared, owing to their greater solubility, and the precipitate was seen to consist almost entirely of right-angled crystals, which under a magnification of 500 appeared to be 2–5 mm. long and 1–3 mm. wide.

The third stage commences with a concentration of about N/600. As the strength of the reacting solutions increases, the number of perfect crystals quickly falls, until, at a concentration of N/100, the precipitate consists of compact star-like skeletons and fine needles. At N/10 the star-like growth figures disappear, and the precipitate (from reacting drops) consists entirely of needles of a maximum length of 3 mm. under a magnification of 1,500. When 0·75N/1 is reached the precipitate has become so fine-grained and mutually entangled that its needle form can hardly be discerned under the higher magnification. This is the commencement of the fourth stage. The formation of so-called amorphous precipitates begins in the neighbourhood of N/1. The precipitate appears to consist of flocks of different sizes as well as a fine-grained precipitate. Under a magnification of 1,500, the flocks are seen to be composed of spherical grains so small as to appear as points.

The fifth stage begins at about 3N/1 with the appearance of translucent gelatinous flocks. At 4N/1, at a temperature of 100°, a partially transparent coarse-celled gel is formed which quickly falls in flocks. At 5N/1 the gel is formed at 20°, as well as 100°, and is more permanent. When the concentration of 7N/1 is reached the single cells of the gel remain transparent for many hours. But after about 24 hours the gel falls into a fine flocky white precipitate, which is very plastic. The results are summarised as below :

Concentration of reacting substances.	$\frac{P}{L}$	Form of precipitate.
N/20,000–N/7,000	0–2	I. Macro-crystals appear after many years.
N/7,000–N/600	2–32	II. The precipitate appears as complete crystals within a short time.
N/600–0·75N/1	32–14,000	III. The precipitate occurs in the form of growth figures (skeletons and needles).
0·75N/1–3N/1	14,000–60,000	IV. The precipitate appears in different forms —ball-like, curdy, flocky, and gelatinous. These are so-called amorphous precipitates, which appear to consist of spherical grains under the microscope.
3N/1–7N/1	60,000–140,000	V. The precipitate separates in the gelatinous form, and, during the first part of its existence, cannot be differentiated under the microscope.

By the inoculation of supersaturated solutions, v. Weimarn demonstrated that even the amorphous precipitates are essentially crystalline in nature.

The most permanent sol, or colloid solution, of barium sulphate results at a concentration of N/3,000. But its stability is not great, and most of the precipitate is deposited within 24 hours. Ultra-microscopic examination showed that the number of sub-microns was small compared to those in a typical sol of the same concentration. Consequently their size increased rapidly, owing to the greater amount of nutrient material, with a corresponding loss of Brownian movement and eventual precipitation. To prepare more permanent sols of barium sulphate, it was evidently necessary to increase the number of crystallisation centres in relation to the nutrient material, and so reduce their rate of growth. This was done by further reducing the solubility by mixing solutions containing 50 per cent. of alcohol. The reaction between $CoSO_4 . 7H_2O$ and $Ba(CNS)_2 . 3H_2O$ was employed, with concentrations varying from N/10 to N/500. The resulting sols were extraordinarily permanent. One containing 0·25 per cent. $BaSO_4$ had not deposited any precipitate after seven years. By mixing solutions containing progressively more alcohol from 1 to 50 per cent. it was shown that the ultra-microscopic particles must consist of the finest crystalline needles of $BaSO_4$. It appears, therefore, that from the first stage to the last the disperse phase is crystalline.

The number of crystallisation centres may also be increased

by adding to the reaction medium some neutral substance, which increases its viscosity while taking no part in the chemical change. This progresses more slowly than before, and, by reason of the internal friction, the Brownian movement of the resulting particles is diminished, so that they more readily form centres of crystallisation, while their smaller sphere of movement limits them to a lesser size. A third method for the preparation of sols consists in the employment of reacting solutions of a high degree of aggregation. The greater the degree of aggregation, the less the Brownian movement, and the smaller also the number of particles which will have to combine together to form the slow-moving centres of crystallisation, which will be proportionately increased in number. Thus a sol of the metal barium, prepared by Bredig's method, may be treated with dilute sulphuric acid with the formation of a sol of $BaSO_4$ and escape of hydrogen.

Aluminium hydroxide, as it usually occurs, is a typical representative of the colloid state. This is due to its great insolubility and to the production of associated basic salts as intermediate products during its formation. Small macroscopic crystals of this substance may be obtained by the slow union of extraordinarily dilute reagents, or by artificially increasing the solubility. The former method has long been known to mineralogists. In the case of a difficultly soluble substance like barium sulphate, of which the solubility is about 10^{-4}, we have seen that macro-crystals are produced from the mixing of two litres of very dilute solutions after some years. In the case of a body whose solubility is only about 10^{-10}, the number of crystallisation centres will be so much greater that there will be insufficient nutrient material for them to grow into crystals. So that, to obtain crystals, very large quantities of extraordinarily dilute solutions must be used. When however micro-crystals have been obtained, if they are put into a super-saturated solution obtained in any way they will grow to macroscopic size. This is the basis of the first method. The second method was employed by v. Weimarn, who made use of the greater solubility of aluminium hydroxide at the boiling point. Half a litre of $N/2,500$ $AlCl_3 . 6H_2O$ was poured into the same volume of $N/2,500$ NH_4OH solution, and the mixture boiled until an opalescence was clearly visible. The heating was then discontinued and the

solution cooled to room temperature. In 10–12 days a fine-grained precipitate of $Al_2O_3 . 3H_2O$ had been deposited, which, under a 15,000 magnification, was observed to consist of spear-shaped needles 3–4 mm. long.

The stage of so-called amorphous precipitates begins with aluminium hydroxide at about N/500. At N/1 coarse-celled gels are formed. And at 3N/1 the mixture sets to a solid gel.

By application of similar principles a typical crystalloid, such as common salt, can be obtained in the colloid form. For this purpose it is merely necessary to choose a medium in which the solubility is sufficiently small. By pouring 4 c.c. of a saturated solution of NaCl in methyl alcohol into a mixture of 30 c.c. propyl alcohol and 10 c.c. ether, a precipitate in the form of growth figures is obtained. If a mixture of equal parts of propyl alcohol and ether is employed, the precipitate appears amorphous under a 300-times magnification. With 30 c.c. ether and 10 c.c. propyl alcohol a voluminous precipitate of fine-grained amorphous structure is obtained. Finally, by pouring 4 c.c. of the saturated methyl alcohol solution into 40 c.c. ether, a strongly opalescent sol is obtained, with a characteristic reddish-brown colour by transmitted light, and which deposits only a minimal precipitate in 24 hours, remaining otherwise unchanged.

These researches explain the natural occurrence of large crystals of insoluble bodies either by deposition after many years from enormous volumes of very dilute solutions, by the gradual diffusion of the reacting solutions into gels, or by the slow crystallisation of the latter.

PREHISTORIC CLASSIFICATION

By W. J. LEWIS ABBOTT, F.G.S., F.R.A.I.

There can be no doubt of the justification of the classification of the enthusiastic young prehistorian Lubbock, at the time of its introduction. When Prestwich invited him over to see the iron-stained weapons in the river-drift of the Somme, they were so obviously different from the "bleached" whitened cylindrical polished specimens found at or near the surface in this country, with which he had become familiar; and moreover they were indisputably associated with the bones of the extinct mammalia, in a gravel of great antiquity; whereas the polished bleached and fresh specimens were so closely associated with the existing order of things, that it was obvious the two were separated very widely from each other, in a manner as immediately to suggest an "older" and a "newer"—a palæolithic and a neolithic age. Further, the great difference between polished specimens of the surface and the more or less rudely chipped ones of the drift suggested the terms "polished" and "rough stone ages." But unfortunately the words "palæolithic" and "neolithic" were only extreme terms; palæolithic was confined to the river-drift specimens, and neolithic [1] was the equivalent of "polished" stone, but was vaguely made to include all specimens found at the surface. There was, however no "scientific frontier," between the two states, and with closer observation the "no man's land" grew into an ever-widening "buffer state." Soon everything found at or near the surface—especially if it had suffered any of these alterations which occur to flint on the surface of the earth—came to be regarded as neolithic. With this practice in operation, the hiatus became an impassable ocean, to bridge which many attempts were made. As time went on it became more and more apparent that archæologically, geologically, and palæontologically the two were separated by a vast interval

[1] *Prehistoric Times*, 1872, p. 3.

of time: a certain school then sprung up which suggested that the palæolithic and neolithic might be separated and bridged over by the glacial period (during which time it was claimed Britain was uninhabited). But the then existing predisposition towards the modernity of man would not allow the idea. (Comparatively recent as the glacial period might have been, it was altogether too long ago for man to have been here; and when implements were found in the Thames valley, to make man post-glacial it was asserted that the glacial deposits stopped on the Essex hills and at Finchley, and never entered what is now the Thames valley. It counted nothing that the glacial deposits at Finchley were not terminal moraines, but chalky boulder clay, and when this was even traced under Highgate into the Thames valley, underlying redeposited London clay, the old prejudice was so strong that the old ideas still held the field! So firm was this hypothesis maintained that papers describing chalky boulder clay with northern erratics and fossils in the valley of the Thames were refused to be printed by the Societies! Since then the sections at the Admiralty and in the Lea valley have shown how wrong were these contentions.)

I think it was my old friend Westropp who first suggested to bridge over the gap, by the introduction of a "mesolithic" age, his classification was as follows[1]:

Barbarous	Palæolithic	Rough flints.
Hunting	Mesolithic	Flint flakes, flints chipped into shape.
Pastoral	Neolithic	Implements ground at edge, or all over, and polished.
Agricultural	Bronze	Arrow- and spear-heads, swords and celts.
State	Iron	Celts, spears, swords.

Ingenious as this and its further subdivisions might have been in the light of the knowledge he possessed, this latter was altogether too small for so embracing a subject. The next, I think, to advocate a mesolithic age was my old colleague John Allen Brown[2]; most of his specimens, however, we should now regard as "surface palæoliths," of which some of us have large collections.

The question naturally asked was how it was that all the palæolithic implements were found in the gravels, and how

[1] *Prehistoric Phases*, p. xxiv.

[2] "Continuity of the Palæolithic and Neolithic Periods," *Jour. R.A.I.* vol. xxii. pp. 66–98.

they got there. I remember a hypothesis being advocated that man arrived here in a cold period, when the rivers were frozen over, and that he made air-holes in the ice for the fish to come to, when they were speared, and through which the implements got lost in the rivers and mixed with the gravels. Even now there are many people who have not fully realised the chronological value of implements in a deposit. That they are fossils no one can deny, and they must be treated as such ; but they differ greatly as chronometers when compared with the mollusca that may lie beside them. When one sees the delicate little double valves of the mollusca still unseparated, and a faunal assemblage which have a common habitat, we may be sure they are in their original home, and were contemporaneous with the deposit. As we examine the fauna of a particular river-valley at the different altitudes we are able to trace the history of the latter: whatever its depth may be we can see that the valley has been excavated in (say) pleistocene times ; widely separated as its banks now are, time was when the valley was filled by the country rocks which at one time formed the surface of the earth, upon which man and beast lived ; from the commencement of that excavation the stream has been carrying seaward the soluble and transportable materials, ever encroaching upon plateau and plain, cutting back subsequent and obsequent " land-grabbers," and constantly drawing into its vortex rocks, stones, and earth. But although its operations may have been extensive they were nevertheless limited, and vast areas of surface have never been denuded of their surface materials. Naturally during the whole of this time the primæval hunters have camped beside its waters, and sat upon its banks and worked flints into implements which in many cases have been preserved practically as man left them. In other cases they have been whirled along in the waters and mixed with others belonging to long anterior ages. Sometimes an easier-wearing stratum has been encountered at some distance, and the stream has gone into the deeper channel and left the deposit (and perhaps the flaking floor) as a terrace, on the sides of the valley. The same thing, of course, occurs with the alteration of land altitudes, and base levels of erosion. I submit there are two very obvious facts that are not usually recognised.

Firstly, that the area of old land surfaces that have not

been destroyed by subaerial denudation in the process of valley formation is much greater than is sometimes realised. Secondly, that very generally implements are older—very often very much more so—than the deposit in which they are found. The first question that naturally suggests itself is, where are those uncaptured surface palæoliths? They must be on or near the surface still, and, having been subject to the same processes as the other flints associated with them, will be subject to the same set of surface changes. But the antiquity of anything found at the surface, especially if alteration processes have not been very operative, or if the specimens have become "whitened" (cretaised or porcellanised), is precluded by the ideas that have associated themselves with the term "palæolithic." It is therefore certain that the term palæolithic cannot be confined to the iron-stained (jasperised) specimens of the drift; nor can it be excluded from surface finds: consequently the terms palæolithic and neolithic, rough and polished stone ages, in the light of more recent discoveries are misleading, and should now be relegated to the pension list. It is true an attempt might be made to limit and define the term neolithic, but it would always be confused with its old equivalent, polished stone age (despite the fact that polished weapons belong to metal ages), or to things not found in an old river-drift. Further, so much more material from so many different sources has so enlarged our knowledge of late years, that we require a far more extensive and comprehensive scheme of classification. Probably the first real light that was thrown upon post-miocene succession came from the palæontological evidence. In their exhaustive researches on the pleistocene and holocene mollusca, Messrs. B. B. Woodward and A. S. Kennard have shown from time to time that the palæo-neolithic gap could be largely filled up by the mollusca of various sections, that the jump from the extinct to the wholly recent did not really exist, and that an orderly consecutive chronological series could be established from the pliocene to the recent. Then came the splendid work of Hinton on the vertebrates, telling of the same orderly succession. During recent years the science of lithoclasiology has developed, and this, in showing the natural history of the flint-worker's art, establishes this same orderly sequence.

It did not require many years of either practical field work

or philosophic reasoning to realise that the acheulian and chellian industries—the constituents of Lubbock's palæolithic period—did not represent man's first essays at flint working, and accordingly Gabriel de Mortilet proposed the term "eolithic" for certain flints which he considered bore evidence of man's work. But the idea of an intelligent being at so early a geological date as the deposits in which the flints were found received but little favour with many prehistorians. Later John Allen Brown reintroduced the term eolithic with more success for the worked flints from the S.E. plateaux. It was when the writer was first showing him some of these plateau things that he exclaimed "we have here the dawn of the stone age—the eolithic period." He was then reminded that it was already a preoccupied and presumptuous term; but he persisted in its application, and many have since followed in his lead.

Unfortunately the date of the plateau deposits in which the first evidence of intentional chipping was found has not been universally accepted; naturally the implements occur in beds which have generally been relaid several times. That the first of these are of pliocene age the writer has recently tried to show,[1] and fortunately most of his co-workers have accepted the assignment.

Within the last ten years an epoch-making series of worked flints have been found in East Anglia by Messrs. Reid Moir, Clarke, Underwood, and others: from the positions in which these are found they appear to belong to three horizons:

III. Below the Norwich Crag (the norwichian).
II. Below the Red Crag (the ipswichian).
I. Below the White Crag (the suttonian).

There are so many facts pointing to their belonging to three separate ages, which cannot be gone into here, that I think we may regard them as such. The suttonian would thus be pre-coralline, perhaps equivalent to the Lenham-Diestian of Kent, Surrey, and Sussex, antedating the invasion of the eastern counties by the Coralline Sea. We are then confronted with the questions: What is the age of the oldest

[1] "The Pliocene Deposits of S.-E. England," *Proc. P.S. E. Anglia*, vol. ii. part ii. pp. 175–94.

implementiferous deposits on the plateaux? Were they laid down by consequent streams which formed down the newly elevated Lenham-Diestian sea-bottom? and were the implements found in them of contemporary age, or were the worked flints surface-products of miocene times? The fact that in the deepest deposit that has been opened—the stanstedian—freshly chipped specimens are found associated with very hard worn ones, rather perhaps points to the possibility of the oldest specimens dating to the close of the miocene age. I am, however, inclined to regard the plateau worked flints as post Lenham-Diestian.[1]

I would like to call special attention to several points raised in the *Proc. R.A.I.*, vol. xli. pp. 460–5. It is there shown, firstly, that an orderly sequence of flint-working can be established for the plateau worked flints. It is pointed out (p. 464) that many important factors may supervene: *inter alia* that implements of a given shape need not necessarily have had a monogenitic origin, that evolution of forms need not have followed on the same lines in different places, and that at any particular time all men would not have been in the same manipulative stage. If these factors did not hold good I fear that we should have to place the three older groups of the plateaux below those of the sub-Crag.

In the same paper (*op. cit.*) a nomenclature for the various forms of flint-working is suggested. They are a lot of more or less ugly, loud-sounding words, but I submit they will improve upon acquaintance, and to every student who has taken up the science of lithoclasiology they are indispensable: a glance at these distinctive features in a system of classification should convince one of their diagnostic value. Of course, when one speaks of "the age of mammals" or "the age of reptiles" one does not mean that every animal was a mammal in one case and a reptile in the other, but that they were the distinctive features of the respective ages; and similarly these various classes of methods of working are the distinctive features of the various ages, and as such have a high classificatory value.

Up to this point we have not touched upon the palæontological side of the question; to this we will now turn. Quite early in the study of prehistoric man the famous Neanderthal skull was brought to light. Unfortunately its exact horizon was

[1] "Pliocene Dep." *op. cit.*

not known; but from its gorilloid characters, it was assumed upon the evolutionary hypothesis that it represented the lowliest and therefore the oldest type of human whose works had been discovered. The finding of this species, however, associated with a mousterian industry, and in stratigraphical superiority to the chellian and acheulian, revealed its existence at a much later date than was expected. Whatever might have been the date of its first appearance it was evident it had lasted down to post-acheulian times, and although lithoclasiologically absolutely distinct, it was faunistically associated with it without any break.

It is not so, however, in its relation to the superimposed aurignacean: here we have modern man separated by a prodigious fauna, of arctic habitat, and wearing the facies of both the past and the future.[1] The lithoclasiological break is just as pronounced. Obviously we have here a break so large that it must be very pronounced and conspicuous in any system of classification.

It cannot be supposed for a moment that every man living at this period was either morphologically or culturally mousterian, and we find here two and probably three well-marked cultural stages, and although the lower had a number of forms of implements which do not pass higher, it still had badly made bouchers, that associate it with the acheulian. The other two divisions have both connecting and separating forms, so that the three should be grouped in one division, say as a lower, middle, and upper.

From the aurignacean upwards (with the exception of the solutrian, with its very fine clinoclastic work) there is no break, they belong to one great division; but we must not call it either palæolithic or neolithic: it is the division of the *new men* and we may therefore call it the neoanthropic: and the antecedent division, being that of the old men, the palæanthropic. The upper part of the neoanthropic will coincide with the holocene, the various cultures of which have not yet been thoroughly worked out, defined, or named, but the existence of some of them is indicated by their lithoclasiology. The lower part will lie in the pleistocene.

[1] "The Ossiferous Fissures of the Valley of the Shode," *Q.J.G.S.* vol. l. pp. 188–210, and vol. lv. 419–29; "Pleistocene Vertebrates," *Proc. S.E.U.S.S.* 1908, pp. 96–113; *Pal. Soc. Mono.* 1909.

The palæanthropic will occupy the rest of the pleistocene, and extend into the cromerian pliocene.

All over this country and the continent there are to be found large quantities of meroclastic implements: whether they should all be assigned to one period it is difficult to say; the writer does not consider they should. They are just rough flints partly chipped. Generally (though not always) a simple rude point or cutting edge is put on, with little attempt at shapening: those from some localities show very bold free-struck mesoclastic work, in others it is more mioclastic. Many years ago the writer first referred to these as archæoliths. They occur probably in the greatest numbers beneath the chellian, and in that position were called strepyan, by my old colleague Rutot, from being found at Strepy. Below these Rutot recognises three other industries, mesvenian, mafflien, and rutelian: each of these industries finds a counterpart in this country, although not superimposed as Rutot found them in Belgium. Some of the mesvenian worked flints he has sent me are similar to the heteroclastic fawkhamian things of our plateaux. They show round-hammer free-struck parallel flaking mixed with edge flipper-work. The flints from the two underlying industries are lithoclasiologically greatly inferior to the skilfully worked norwichian things, or even to the ipswichian, and are probably equivalent to the older pliocene plateau industries.

In pithicanthropus, and eoanthropus we are introduced to two very different kinds of simian characters, the one relating to the cranial vault, the conformation of the forehead, and brow-ridges, and the others to a chinless ape-like jaw, all of which characters are protoanthropic. What other treasures await discovery we cannot say, but these two genera justify establishing another division—the protoanthropic. There can be no doubt that the anthropoidæa resorted to the use of clubs and stones long before they reached the large-brained stage of eoanthropus, and as we learn more of them we shall be able to subdivide this oldest division.

I submit that one part of the facts enumerated here calls for a revision of our classification, and the other suggests the lines upon which this should be based. One cannot for a moment claim that the classification as here set out is perfect and complete. It is only submitted as a basis, and it may

PREHISTORIC CLASSIFICATION

Geological.	Anthropopic.		Older (former) archæological divisions.	With newer additions and correlations.	Lithoclasiology.	British localities.
HOLOCENE.	Neoanthropic.	Upper	Iron.			
			Bronze (and copper).			
					Brachyclastic.	
					Clinoclastic.	
					Mesoclastic.	
				Last Hastingian.	Microclastic.	Hastings, Blackdown.
PLEISTOCENE.		Lower	Azilian.	Azilian.		Oban, Culbin Sands.
			U.	Kemsingian.		Kemsing.
			M.		Dolichoclastic.	
			L. Magdalinian.			
			U.		Clinoclastic.	
			L. Solutrian.	First Hastingian.	Microclastic.	Hastings Kitchen Middens.
			U.			
			M.			
			L. Aurignacian.	Fairlightian.	Brachyclastic.	Fairlight, Hastings.
	Palæanthropic.	Upper	U.	Cissburian.	Hemihedral.	Cissbury, Grimes Graves.
			M.	Ebbsfleetian.	Dolichoclastic.	Southfleet.
		Middle	L. Mousterian.	Preswichian.	Megaclastic.	,,
			U.	Oldburian.	Clinoclastic.	Ightham.
			M.			
			L. Acheulian.		Mesoclastic. Essentially Holohedral.	E., S.E., and S. River-valleys.
		Lower	Chellian.		,,	,,
Cromerian.				Upper Ashian?	Holoclastic.	Hill Gravels.
				Strepyan?	Meroclastic.	,, ,,
PLIOCENE.	Protoanthropic.			Fawkhamian.	Heteroclastic.	Fawkham, Piltdown.
				Meridionalis Gravels.	Holoclastic.	Dewlish, Hastings, St. Leonards.
Norwich Crag.				Norwichian.	,,	Norwich, Whitlingham.
Red Crag.				Ipswichian.	,,	Ipswich, Thorington, etc.
				Mesvenian?	Meroclastic.	
				Mafflien?	,,	
				Rutelian?	,,	
Coralline Crag.				Lower Ashian.	Edge-worked.	N. and S. Downs.
				Stanstedian.	,,	Kent Plateau.
				Suttonian.		Sutton.
MIOPLIOCENE. Lenham Diestian.						

be subject to whatever alterations and additions further and closer study may necessitate.

Firstly, we have the geological horizons, or ages, commencing with the mio-pliocene Lenham-Diestian. Then the pliocene, pleistocene, and holocene periods respectively.

Next the anthropic divisions: protoanthropic, palæanthropic, and neoanthropic.

Of the first we are scarcely in possession of sufficient knowledge at the present to make subdivisions. There are several more points which must first be decided. But the industries are well marked. The second I submit can well be divided into a lower, middle, and upper. The lower to include the pre-chellian; the middle the chellian and acheulian, with their subdivisions; and the upper the mousterian with its subdivisions.

The neoanthropic may naturally be divided into two; a lower and an upper, the line being drawn at the top of the passage industry, the azilian, just below the holocene.

Although a vast amount is known of the upper neoanthropic, a perfect sequence has yet to be worked out; certain well-marked lithoclasiological industries are indicated in the last column. Nothing but a vast amount of co-operative work will enable us to form a satisfactory classification. I feel convinced that the adoption of some such a one as is here offered will remove a vast amount of difficulties which now hamper alike the prehistorian and the palæontologist; and offer a good working basis in harmony with recent discoveries.

POPULAR SCIENCE

THE NATURE OF SUN-SPOTS

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A SUN-SPOT consists generally of a large dark area called the umbra, in which at times darker patches can be detected, called nuclei. The umbra is surrounded by a filamentous area of much greater extent, in wavy and broken patches, called the penumbra. Between the umbra and the penumbra is a boundary region of lighter hue, forming the inner edge of the filaments of the penumbra. Photographs on a large scale, showing much detail, as those for instance taken by Father Chevalier at the observatory of Zô-Sè in China, confirm the reality of a real increase in light at the interior border of the penumbra of a sun-spot. But they also show that this maximum of luminous intensity does not occur at the extremity of the filaments. These extremities are not in the form of luminous club-heads projecting over the umbra, as figured in Langley's hypothetical drawing of a typical sun-spot, but of tapering points, which curve over towards the umbra, and diminish gradually in brightness. The arrangement of these filaments is in the form of undulating layers, lighter in the regions that are more raised, and darker in those that are at a lower level. A really large sun-spot is of enormous size. For instance, the group, easily visible to the naked eye, which crossed the sun's disc between February 4 and 15, 1917, in south latitude 15·6° and longitude 10·2°, extended over 18° in longitude and 6·5° in latitude, while its surface area was 26 units, in terms of the 1/5000 of the visible disc. Roughly, this disturbance affected an area of 3,500 millions of square miles, exceeded indeed by the spot of February 7, 1905, which covered 3,800 millions of square miles.

The life-history of a group of sun-spots generally follows

a definite progression of changes. At first a few small spots appear in bright compact faculæ, or luminous white clouds, the faculæ preceding the appearance of the spots. These spots then coalesce into two main spots, a leader or western spot of the group, with a trailer or eastern spot, with many subsidiary spots bridging the gulf between the two main spots of the group. It is in this complex region between the spots that, in the earlier days of the life-history of a group, the greatest display of energy takes place. In this region spectroscopic observations show many reversals and distortions in the hydrogen lines. Meanwhile the faculæ are still very bright and cling to the spots. The subsidiary spots bridging the gulf between the leader and tail spot will first disappear. The tail or following spot will next break up, leaving the leader spot to attain the comfortable rotundity of middle age. Finally, this spot, growing ever smaller and smaller, will disappear as a few insignificant dots. The faculæ, which in the early days of the life of the spots clustered round the group in dense luminous clouds, will extend in branching lanes to cover an immense area of the solar surface.

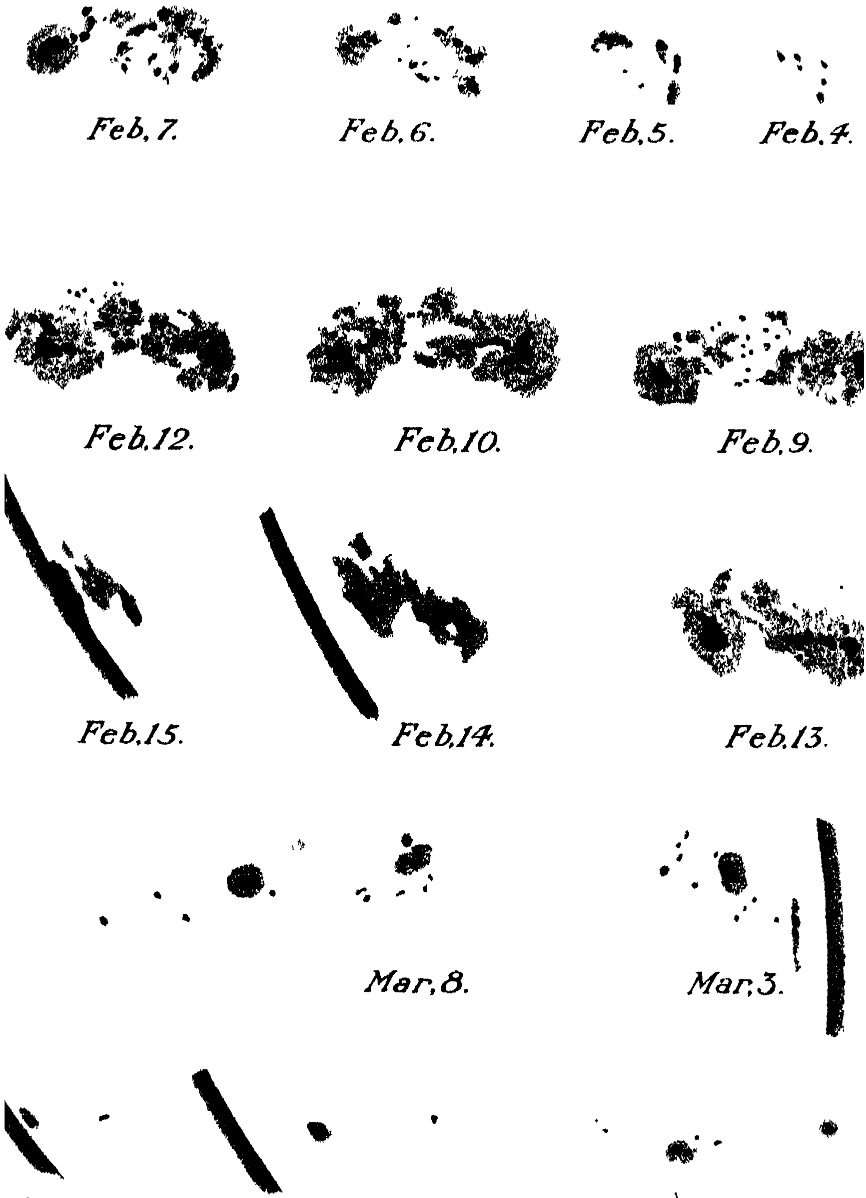
From a detailed study of some 3,500 drawings of solar spots made at Stonyhurst, it was possible to divide all spots into five normal types. But the chief type, of which the others are but phases, was found to be the double spot formation. These groups have subsequently received from Prof. Hale the name of bipolar groups, and they exhibit opposite magnetic polarity. In the northern hemisphere and in middle solar latitudes, the direction of rotation for the preceding spot or western spot is counter-clockwise, and for the eastern or following spot clockwise. This direction of rotation corresponds in the preceding spot to that of a terrestrial tornado. The opposite direction obtains in the southern hemisphere, as is the case also in terrestrial tornadoes. But in spots in high latitude these directions are reversed.

Sun-spots appear to be dark, the umbræ and particularly the nuclei seeming to be very black, but in reality they are intensely bright—far brighter than any terrestrial source of illumination. They are composed of uprushes of metallic vapours, in which vanadium, titanium, and iron are very evident. The rapid expansion and consequent cooling relatively to the bright solar surface cause them to absorb the

solar light, and hence to appear dark. The temperature of the sun's surface is about 6,000 degrees on the absolute Centigrade scale, and that of sun-spots, as is indicated by a lowering temperature sufficient to allow of the formation of chemical compounds, such, for instance, as titanium oxide, magnesium hydride, and possibly superheated steam, is about 3,500 degrees on the same scale. Hence they exercise an absorbing effect on the higher temperature vapours of the solar core.

In the year 1769 Dr. Wilson of Glasgow broached the idea that sun-spots were depressions in the solar surface. His demonstration was founded upon the appearance of the penumbræ of spots, seen in perspective, as the spots travel from east to west across the visible solar hemisphere. When the spot is near the east limb, the penumbra only appears on the side turned towards the limb, and appears to be wanting on the other side. As the spot approaches the centre of the disc the westerly edge of the penumbra gradually comes into sight. As the spot moves towards the west limb carried round by the rotation of the sun, the eastern part of the penumbra begins to disappear, so that when it is near the west limb, the western side of the penumbra is alone seen. The interpretation of these phenomena is that the spot is a saucer-like depression, with the umbra corresponding to the bottom of the saucer. But there are relatively few round symmetrical spots which can be observed as tests of this theory. Moreover, when the spots are very near the sun's limb, the penumbræ are not seen at all, but only a foreshortened dark line, presumably representing the umbra. The umbra also, when a large spot is on the limb, seems to block out the view of the edge of the sun, and would be better described as an elevation above the level of the general surface of the sun, than as a depression below it. This was noticeably the case when the big spot of February 15, 1917, was crossing the sun's western limb. Visually a sun-spot is a shallow depression represented by the penumbra, in which rises an elevation seen as the umbra.

There are relatively very few spots in which a gyratory or cyclonic motion can with certainty be detected. Nevertheless there are such instances observed, as for instance the rotation of the umbra and penumbra of a spot around a nucleus described by Dawes, who recorded in one case a rotation of 100 degrees in six days, and on December 23, 1851, observed in



Life-history of the large sun spot group, February 4 to March 14, 1917.

Stonyhurst College Observatory.

a spot a rotation of 70 degrees in one day. As observed at Stonyhurst, the sun-spot of April 16–21, 1882, revolved through 90 degrees in four days. From his spectroscopic observations of movements in the penumbræ of spots, Mr. Evershed also comes to the conclusion, that if a rotatory movement exists in some spots, it is not a constant and regular feature of spots. Yet some spots, by the swirling appearance of their umbra and penumbra, vividly suggest a cyclonic or rotatory motion in the spot.

A spectro-heliograph is a modification of the spectroscope, by which it is possible to photograph the sun's surface and envelopes in monochromatic light, either for instance in the red light of a hydrogen ray, or the violet light of a calcium ray. Also it is possible to photograph the clouds of calcium and hydrogen which exist above and around sun-spots at different levels, so that a picture may be obtained of the sun in light corresponding to relatively low-lying calcium, and to calcium at a higher level. These spectro-heliograms, as they are called, in calcium light, show that a sun-spot is covered and surrounded by extensive clouds of luminous calcium vapour. Many photographs of these flocculi, for so they are named, both in calcium and in hydrogen light, have been made by M. Deslandres and by Prof. Hale. It is found that the calcium flocculi, which may be likened to the cumulous clouds in the earth's atmosphere, exhibit no well-defined linear structure. But the hydrogen flocculi of the higher regions of the solar atmosphere exhibit a very definite structure, and appear, in relation to the sun-spots they surround, as cyclonic whirls, right-handed or left-handed as the case may be, which are highly suggestive of lines of magnetic force. On June 3, 1908, a great cloud of hydrogen, which had been photographed earlier on May 29 and succeeding days at Mount Wilson as hanging on the edge of a vortex structure, was suddenly sucked into the spot at a velocity of about sixty miles a second. Hence we derive the genesis of the idea, that sun-spots are analogous to tornadoes or water-spouts in the earth's atmosphere, with the trumpet-like end upwards.

Let it be remarked, however, that these observations, and the deductions from them, concern only the higher regions of the gases and vapours above a sun-spot.

When a sun-spot is observed by means of the spectroscope,

its radiation is dispersed into a band of general darkening longitudinally crossing the lines of the photospheric spectrum. This band varies in intensity in different spots, being in large spots generally very black, and in small spots of a decidedly lighter tint, the difference being due, no doubt, to the varying quantity and depth of the materials contained in the spots. If the materials in a sun-spot were simply opaque to light, this variation in tint might be accounted for by the sparseness or denseness with which the particles were packed together. The effect produced, however, seems not, at least wholly, to be due to the stoppage of light by opaque particles, because with highly dispersive spectroscopes the dark band is seen to be discontinuous, crossed by bright spaces. With very powerful instruments the band of general darkening has been resolved into a series of fine lines, and bands, or congeries of lines, indicating a gaseous constitution.

Besides this almost continuous band of general darkening, the Fraunhofer lines are variously affected in a sun-spot. The most general characteristic is, that, where they cross the spot, they gradually widen though the penumbra to the umbra, in which part of a spot the widening effect is most pronounced, and then gradually narrow so as to present a sort of spindle shape. Not all the Fraunhofer, or dark, lines of the solar spectrum are so affected when they cross a spot, but only some. Taking the lines due to any given metallic vapour, iron for instance, some lines will be widened in this manner and some not. Some, too, will be weakened where they cross a spot. And speaking of metallic lines in general, some will be obliterated in a spot, some will be thinned, as, for instance, the lines of hydrogen; some will be reversed, if indeed it is a true reversal of the line, and not rather a doubling of a line, which is single in the photosphere. Other lines, though weakened in intensity, will be widened; other lines will extend as widened far beyond the limits of the spot; while others will have a sort of fuzzy fringe attached to them, and be confined as spot bands to the umbra.

In addition to these variations in the line spectrum of a spot, there is a whole series of bands made up of a great number of lines forming flutings. These bands may become so prominent in a big spot, as, even with a large dispersion, to present the appearance of a local strengthening of the general

darkening, or quasi-continuous band, at the background of the line and fluted spot spectrum. The appearances presented by this complicated spectrum would seem to be for the most part due to the absorption of a mass of metallic vapours and gases of great density, the varying density or thickness of the strata being denoted by the variations in the general darkening of the background of the spot spectrum.

The lines that are most widened in the sun-spot spectrum are due to vanadium, titanium, and silicon, and it is generally the fainter lines of their normal photospheric spectrum that are the most affected by widening, an indication of the low level of a sun-spot. Next in order of widening, at least in the region of the red and yellow parts of the spectrum, which has been the portion especially studied at Stonyhurst, come calcium, sodium, nickel, manganese, iron, while cobalt, scandium, and chromium are represented by well-marked lines. It is noteworthy that most of the elements enumerated form the first half of the first long period in the chemical periodic system. This would seem to indicate that the level of sun-spots is that of the metals with atomic weight about 50, for it is conceivable that, as we advance outwards from the solar photosphere through a mixture of metallic vapours and gases, there should be a layer or stratum in which vapours of the metals connected by any one period of the periodic law should be most abundant.

Titanium also appears as titanium oxide in the banded spectrum of sun-spots, in which, too, are represented magnesium hydride and calcium hydride, with possibly a much more familiar compound in the form of superheated steam. A banded spectrum of chemical compounds is generally indicative of a lowering of temperature. Hence it is argued that sun-spots are at a lower temperature than that of the photosphere. To the contrary, however, it is urged that a banded spectrum may be produced by pressure alone, and, moreover, some compounds, though titanium oxide is probably not among them, are formed with an absorption and not with an evolution of heat. If, however, appeal be made to the laboratories of the stars, titanium oxide in particular, as Prof. Fowler has shown, is responsible for a whole series of characteristic bands in the spectra of stars of Type III., such as *a* Herculis. It is generally supposed that Type III., or red stars, are at a lower temperature than the yellow stars of Type II., which give a spectrum

like that of the sun's photosphere. Moreover, in the variable star Mira Ceti, which belongs to Type III., the titanium oxide bands are much weakened at brighter maxima of the star's light; and the photosphere of the star is probably hotter at such maxima, as the hydrogen bright-line spectrum shows the blue radiation Hβ, which is not seen when the titanium oxide bands are more prominent.

Again it is generally supposed, though it has not been proved, that the electric spark represents a higher temperature than that of the arc, and especially of the flame of the arc. The lines that are widened in sun-spots are those which appear in the arc flame, while those weakened are the so-called enhanced lines which appear in the electric spark spectrum of an element. But the argument is inconclusive, as some experiments of Dr. Hemsalech show the enhanced lines in a flame at a lower temperature than the flame which gives the arc lines. It may be doubted whether these differences of behaviour in the lines of an element in arc and spark are due at all to temperature, but rather to a difference in the condition of electrical excitation. Even in experiments with the electric furnace, the heating is produced by electrical means, and the resulting spectra of the metals heated in the graphite chamber may be conditioned, as in the arc itself, by the mode of the production of the heat. On the whole, however, opinion at present tends to the view that the vapours in sun-spots are at a reduced temperature, being at a temperature of about 3,500 degrees on the absolute Centigrade scale, while that of the photosphere is at a temperature of 6,000 degrees on the same scale. Hence sun-spots, though intensely bright in themselves, appear to be dark, as the cooler vapours constituting them absorb the more intense radiations of the sun's photosphere.

The spectra of sun-spots, as observed at Stonyhurst for more than thirty years, are substantially always the same, the same lines being always picked out for widening, or weakening, or obliteration, and for the other effects to which lines are subjected in the spot spectra, and also to the same degree, except, perhaps, that the intenser iron lines are more apparent in the minimum than in the maximum period of sun-spots.

The discovery of the magnetic field in sun-spots, made by Prof. Hale with the magnificent and highly dispersive apparatus at Mount Wilson, is one of the most brilliant and important

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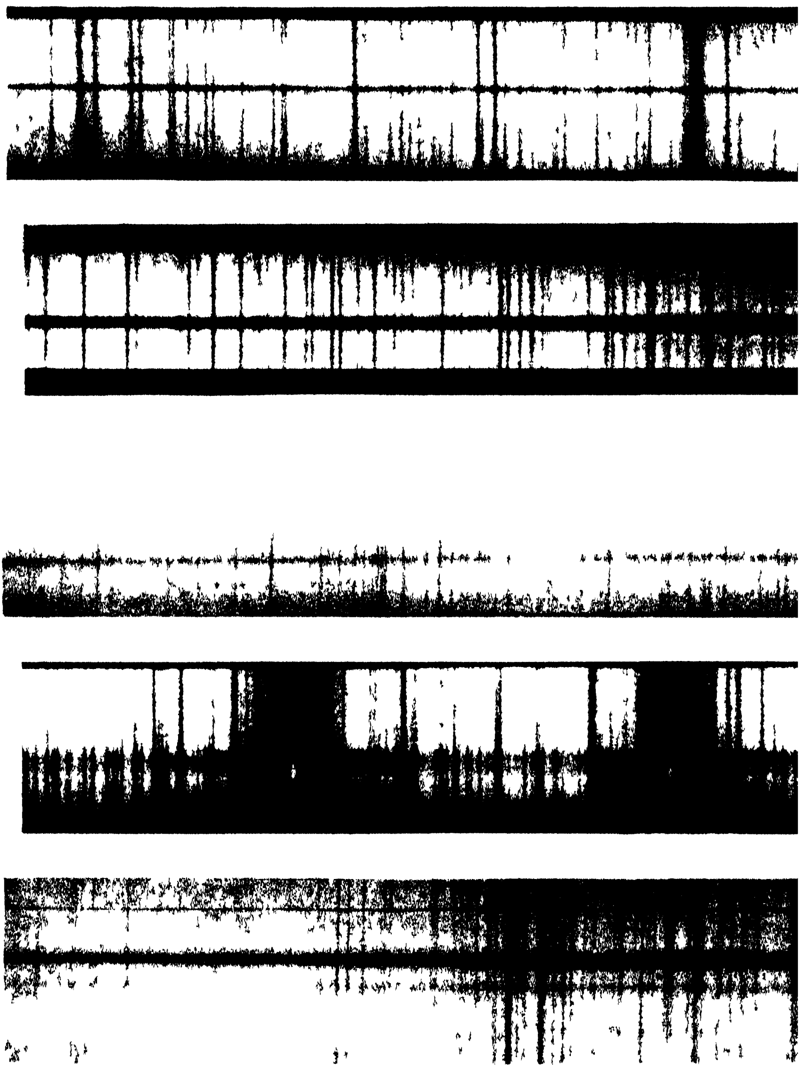


Spectra of sun-spots

1 Region of the D lines 2 Reversals in the H and K lines 3 Region λ5455-56 4 Region λ
5 Radial velocities in a sun-spot region (Evershed)

made in recent years. In the year 1896 Prof. Zeeman had demonstrated a new relation between light and magnetism, by proving that the emission and absorption of luminous radiations are modified, when exposed to the influence of magnetic forces. He experimented with a flame containing sodium vapour, which was placed between the poles of a strong electro-magnet. The effect produced was that when the current was passed through the coils of the magnet, the D lines of sodium, a double yellow line in a spectroscope of sufficiently high dispersive power, were seen to be widened. Subsequent experiments showed that nearly every radiation observed in a powerful spectroscope is split into three components when the original radiations are observed in a direction at right angles to the lines of force. When, however, the lines are observed along the lines of force, the central line of the triplet disappears, and the two terminal lines remain which are found to be circularly polarised in opposite directions. With suitable polarising apparatus one or other of the two components of the double can be quenched, leaving the other unchanged. Now, in sun-spots the spectrum when observed by means of a spectroscope of moderate dispersive power, as compared to modern instruments, appears to be full of broadened and thickened lines. More than a quarter of a century ago at Stonyhurst some lines appeared to be doubled in the spectrum of a sun-spot, suggesting at the time rather the appearance of a bright reversal in the widened dark line, than of two separate lines. But Prof. Hale, armed with the powerful instruments of Mount Wilson, argued that if a sun-spot is an electric vortex, and the observer looks along the axis of the whirling vapour, which coincides with the direction of the lines of force, he should find the spectrum lines double, and by means of a polarising attachment of a Fresnel rhomb and Nicol prism to a spectroscope, he should be able to quench either component at will. This test he applied triumphantly on Mount Wilson to two spots in June 1908, and at once found all the characteristic features of the Zeeman effect. Most of the lines of the sun-spot spectrum are widened in the magnetic field, but others are split into two components.

When a current of electricity flows through a coil of wire in the form of a helix, lines of magnetic force are produced threading the helix. Putting the first finger of the right

hand on the coil in the direction of the flowing current, the extended thumb points to the north pole of the magnetic lines of force. It would seem, then, that a sun-spot, and the vapours constituting it, is essentially a vortex, or tornado, or whirl of electrons or electric particles, which give rise to the magnetic field of force. In the case of a current flowing in a helical wire a reversal in the direction of the current would cause a reversal in polarity. In a laboratory, when a polariscope is so adjusted as to transmit one component of a line doubled by a magnetic field, when observed along the direction of the lines of force, this disappears and is replaced by the other component when the current is reversed. This test also Prof. Hale applied to sun-spots, and found that in typical sun-spot groups of leader and follower spots, the two spots had opposite polarity. The intensity of the magnetic field in sun-spots is sometimes as high as 4,500 units, or about 9,000 times the intensity of the earth's magnetic field. This magnetic field in sun-spots diminishes very rapidly in passing upwards from the sun's surface, and it is probable that the vortex producing the field is at a comparatively low level.

Prof. Hale's discovery was suggested by the whirls observed on the Hα line in spectro-heliographic photographs of the flocculi in the environs of sun-spots. The different behaviour of the spectrum lines in a spot when observed in a polariscopic spectroscope, when near the limb, and when approaching the sun's central meridian, is also a crucial test of the truth of the theory. In the one case the line of sight is directed at right angles, and in the other, along the lines of force. It demands, however, a gyratory motion in the vapours in the environments of sun-spots.

Yet it is doubtful whether the umbra and penumbra of the spots themselves partake in this gyratory motion. For Mr. Evershed finds that if a sun-spot be observed with the spectroscope slit placed across it diagonally from the sun's centre, and when the spot is between 30 degrees and 50 degrees from the central meridian, the lines of the spot spectrum are displaced in the penumbra in opposite directions. If the spot is east of the central meridian, the displacements indicate a motion of approach on the side of the spot nearer the sun's centre, and a motion of recession on the side of the spot nearer the sun's limb, the reverse being the case when the spot is west of the

central meridian. Moreover, the displacements observed when the spectroscope slit bisects a spot in a direction at right angles to a line joining the spot and the centre of the sun's disc are small. If the displacements were due to rotation in the spot, this latter position of the slit should show them to the greatest advantage. The only explanation to account for these displacements in the spectrum lines over the spot is that they are caused by radial movements in the vapours constituting the spot from the centre outwards towards the penumbra. This would accord with the radial structure of the filaments in the penumbra of spots as observed visually.

These interesting observations of Mr. Evershed have been extended and amplified by Dr. St. John at the Mount Wilson Solar Observatory. He observed the radial velocities in sun-spots in the cases of 506 lines of the spot-spectrum, representing 27 chemical elements, and lying at levels extending from the lowest that can be observed to the highest regions in the chromosphere. The reversing layer is the name given to a shallow stratum of vapours and gases some 500 miles in depth, which overlies the photosphere or brilliant light-giving surface of the sun. It is in this region that the absorption which gives rise to the dark lines in the solar spectrum takes place. When the moon covers the sun in a total eclipse, the reversing layer at the edge of the sun is isolated, and immediately, for a few seconds only, all the dark lines of the solar spectrum become bright, the so-called " flash spectrum " being produced. Above the reversing layer lies the chromosphere, or atmosphere of hydrogen, some 3,600 miles in depth, according to the observations made at Stonyhurst during twelve successive years. From the atmosphere rise the flames of hydrogen and calcium and helium, sometimes to enormous heights, called the prominences. From eclipse observations, the mean height to which hydrogen rises is about 4,500 miles, helium 3,500 miles, and calcium about 6,000 miles. The levels of hydrogen and calcium flocculi are presumably still higher, possibly between 15,000 and 16,000 miles. These different vapours and gases are not arranged in successive and isolated layers, like the coats of an onion, but they interpenetrate one another, and rise from the photosphere to different heights. Faint thin lines in the solar spectrum would indicate a rise to a much lower level than would thick black lines. Now, in the spectum of a sun-spot it is mainly the

faint lines that are affected by widening, indicating that a sun-spot does not rise very high above the photosphere, if we regard the umbra as an elevation, and the penumbra as a shallow depression.

Again, a vapour of a metal, for instance that of iron, will be represented by lines of various intensities in the solar spectrum, ranging from very faint lines to some of the most prominent. Hence it follows that some molecules giving certain radiations of iron, as a typical vapour, are confined to low levels, while others extend to higher levels. Now, Dr. St. John finds, and his results have been corroborated by still more recent observations of Mr. Evershed, that the radial velocities of the vapours in the penumbræ of sun-spots decrease with an increase in the intensities of the spectral lines. In other words, low-lying vapours move outwards more rapidly than higher-lying vapours. For instance, in the case of iron there is a gradual decrease in velocity from 1·05 miles per second for lines of intensity 1, to 0·35 miles per second for lines of intensity 8. Taking the whole series of lines of iron, from intensity 00, or very faint lines, to very strong lines of intensity 40, and referring the other elements to the same scale, a sort of sounding-rod is constructed to gauge the depths of sun-spots. One interesting result is that we see to a greater depth into the sun in the red end of the spectrum than in the violet.

Again, after a certain level in a sun-spot has been reached, at a depth represented by iron lines of intensity 10, that is from eclipse results about 2,000 to 2,500 miles above the photosphere, the gases and vapours, chiefly sodium, calcium, and hydrogen, flow inwards. This means that there is in the upper levels a sort of indraught. Hence we may conclude, from the evidence, "that the outward velocities in the reversing layer at the outer boundaries of the penumbra of sun-spots may be referred, with great probability, to a low-lying vortex." The quotation is from the Annual Report of the Director of the Mount Wilson Solar Observatory (Carnegie Institution of Washington) for the year 1913. But we must confess that we fail to see any evidence for a whirling or vortex-motion in the observations summarised above, so far as they concern the radial movements in sun-spots. Undoubtedly there is evidence of a whirling or vortex motion in the flocculi above sun-spots, that is in the hydrogen flocculi.

Among Evershed's conclusions from his observations (*Kodaikanal Observatory Bulletin*, No. 51, 1915) he states that detached penumbræ without nuclei or umbræ show no radial motion; that the radial motion may be continued for a considerable distance outside the limits of the penumbra, or it may stop short at these limits; that there is usually an acceleration of velocity from the umbra to the outer limits of the penumbra, and then a sudden fall to zero in most cases; and that there is a tendency to diffusion of the lines in the direction of displacement near the outer limits of the penumbræ in some spots, indicating turbulent motion. A terrestrial analogue to the radiation movement and diffusion in sun-spots would be the welling-up of substances from the crater-cone of a volcano, and their lateral spreading in all directions outwards.

In Mr. Evershed's latest paper on the radial motion in sun-spots (*loc. cit.*) he details some movements at right angles to the radial movement. These movements might be interpreted in various ways, but one possible interpretation is that they indicate a general descent of the gases over a spot. Again, in photographs taken at Stonyhurst of the calcium radiations (H and K) over sun-spots, more frequently the bright calcium lines are single near the centre of the umbra, but become doubled with an ever-widening absorption between them as they recede from the centre. An obvious interpretation of this appearance is that the absorption is due to the greater quantities of cooler vapours, as welling up from the spot centre they fall to lower levels.

Taking into account, therefore, the Wilson effect undoubtedly present in the penumbræ of round regular spots, when near the sun's limbs, and the appearance of the umbra as overhanging the penumbra when very near the limb, as also the spectroscopic evidence, set forth above, we might picture a sun-spot somewhat as follows. The umbra is a mass of cooled, absorbing vapours, which, mountain-like, rises above the level of the photosphere into the reversing layer. Flowing from it and sinking, these absorbing vapours seek a level slightly below that of the photosphere, and pass into it as dark radial streams giving the appearance of the penumbra. The penumbra itself is in round spots in the form of a shallow saucer-like cavity, the lowest portion being due to the falling-in of the photospheric clouds, caused by the uprush of the mass of

vapours constituting the umbra. This falling-in and heaping together of the photospheric clouds to fill the partial void formed by the ejection of the umbral vapours account for the bright ring which is generally seen separating the umbra from the penumbra. Around the sun-spot thus formed, but in an upper level far above the reversing layer, circulate the hydrogen flocculi, giving rise to the appearance of the solar vortices, in which are generated by the friction of the various gyrating vapours and gases electric currents with the accompanying magnetic fields.

NOTES

The World's Misrulers

The Indictment

Humanity will never be the same after this war as it was before it. It will always feel like a man who at the height of his prosperity but in a sudden moment of aberration has committed an appalling crime—which will burden his conscience for ever, though perhaps he cannot be punished for it. Probably never before had humanity risen to such a stage of success and happiness. Then, in a moment, we were plunged into the darkness. Suddenly the world became full of liars, spies, poisoners, assassins of the night. We, who were honest labourers, farmers, traders, doctors, and lawyers, had to go forth in our millions to do evil things: to blow other men like ourselves to pieces with bombs and shells, to scatter their limbs far and wide, to pierce their bodies, to dash out their brains, to bring them shrieking down from heights in the air, to fling them upwards by subterranean explosions, to throw thousands of them struggling, crying, and gurgling out their last breath, into the sea; to suffer these things in our turn. Why? Did a single one of us wish for it? Were there a dozen people in the whole world so foolish as to imagine that a tremendous struggle between two halves of humanity could lead to any profit to either half, even to the victorious half? Yet the war occurred. It occurred in spite of our boasted religion, science, civilisation, and political organisation—like an earthquake, flood, or pestilence which men are powerless to prevent.

The fact has stupefied the world. We do not blame ourselves—openly; we remember our own courage, wisdom, and self-sacrifice. But nevertheless there is deep in our hearts a voice which accuses us. We protest against the accusation: not we, but those who made the war, are to blame; we have only done our duty. Yes but, our Conscience replies, it was your sloth and stupidity which allowed them to make it; your

greed, boastfulness, lies, dogmas, and indifference which tacitly supported them; you are accessories before the deed, and after it; it is especially you old men who rule the world who have sent forth your nobler sons to die and your widowed daughters to mourn: and yet you call yourselves men, reasoners, Christians, above the order of nature, inheritors of immortality—gods! . . . And mankind remains speechless

> Before the stony face of Time,
> And look'd at by the silent stars.

The Great Crime

There is no doubt that the war was planned during the last forty years in every detail of its infamy by the German autocracy. They said to themselves, We have smitten the Danes, the Austrians, and the French, and we will smite the rest of the world by the same tricks and in the same manner. The world shall look upon us with admiration mingled with fear. We shall be a thunder-bolt among the nations. There shall be only three names written in history, Alexander, Napoleon, and another. The God who befriends and commands us will ultimately place us at his right hand. And, they continue, our victory will also confer benefits upon us and upon the world. We shall dominate the world, we shall grow rich and strong, we shall have innumerable palaces, noble cities, and gardens of pleasure for the poor, and grow wealthy and fat and drink beer at ease. Nay, more than this, we shall bring order and discipline into the world. Let us first, however, organise the victory. Success will excuse excess, and victory, crime. Let us not be meticulous, punctilious, and merciful. When we have conquered, the crown and the purple will cover all our little doings. Our friend God will commend us even for sins, since it is good to do evil that good may come of it. But we must be careful at first, until we are ready. We are surrounded by the silly sheep who shall be our victims—let us therefore say that they are threatening us. Let us be innocents whom they seek to murder for profit. Therefore let us boast that though we seek no war, we are ready to defend ourselves. At the same time we will cunningly prepare treaties in order to lull the quarry as to our intentions. We will call councils to regulate the rules of war. We will guarantee the independence of small nations whose territories we

intend to seize, in order that other nations should not seize them first. We will place paid spies in the dominions of our friendly neighbours. We will secretly prepare platforms for our great guns in peaceful rural spots, and bases for our submarines in peaceful seas. Nay, further, we will regulate the minds of rulers abroad; for all men, including kings, have their price; and we will take this price from our great war chest when the moment comes. At the same time every man among our people shall be trained to war, first in order to insure victory, and secondly in order to make him our accomplice. Then, when everything is matured and ready, we will strike. They will cry out, they will shriek, they will writhe, but they will be powerless. We will prepare beforehand the lies which shall prove that we were the wronged and that they were the aggressors.

In the history of humanity there has been no crime like this. A Macbeth murders a Duncan, and the half-human criminal plots assassination for his personal profit; but they are criminals. Barbarous kings have led their hosts against prosperous countries—but they were barbarians. Worthy nations have made wars against savages or non-civilised and disorderly neighbours—and without blame. But this war has been organised in a time of high civilisation, for offence against prosperous equals. The preparations were made, not for any general good of humanity, but for personal profit, or for what the fools who designed the war considered to be glory. But there is no glory in it; for the preparations, even if they prove successful, will always be, not the preparations of a noble soldier defending his country, but those of a brigand arranging an ambuscade. There will not be three names in history, but only two—and one of them will be Judas. This William the Evil has betrayed mankind as Judas betrayed Christ; and his stupid and barbarous liar-nation will have to pay the forfeit for centuries to come.[1]

[1] The atrocities perpetrated in this war were probably due directly to the Emperor, as it is hard to conceive that any council of officers (even German officers) would have sanctioned them. His capacity for deceit, treachery, and intrigue has recently been proved by his message of August 1914 to President Wilson and by his correspondence of 1904–5 with the Tsar; and to-day the whole world, including Germany, recognises from his character, actions, and speeches that it was really he who conceived, prepared, and precipitated this disaster for humanity.

The Great Folly

But was it only the Germans who were to blame?

The writer was in Germany just before the war. Surprise was expressed to him by several persons of distinction, favourable to England, at the passive attitude of our politicians towards the doings of the Irish and of the suffragettes. There was evidently a veiled feeling of contempt and an idea that Britain was becoming degenerate; and similar feelings certainly existed widely in our colonies and in America. Our trades and manufactures were passing in bulk to other nations. Our cities were collections of slums full of unhealthy people all clamouring for their rights and neglecting their duties as much as possible. Strikes were incessant. Games, processions, shows, and elections were the only things that interested the populace. Education was bad, science and art neglected, the drama despicable. The army was reduced in numbers to the lowest possible limit. There was no sense of discipline anywhere; and the hustings-orators who ruled us by pandering to the mob scarcely dared even to maintain the common law.

Brigandage can exist only under two conditions, the efficiency of the brigand and the inefficiency of his neighbours. From their previous wars the Germans had learnt the old lesson that war may be made a paying game, and had become a brigand-nation. Quietly they perfected their organisation, and watched with amusement the disorganisation of the nations which surrounded them.

The danger was apparent to everyone: how did our politicians meet it? We need only recapitulate the well-known facts.

In 1890 they gave Heligoland to Germany, thus enabling the Germans to form with the aid of the Kiel Canal a strategic base of extreme strength for their navy, situated just opposite our shores, difficult of attack by us but invaluable for attacks upon us. The Germans now commenced to increase their navy rapidly, obviously to challenge our supremacy. After the Boer War, the danger began to be urged in innumerable articles, pamphlets, professional books, and even novels and plays; the National Service League, numbering many thousands of members, was formed; and the old Lord Roberts undertook his prophetic propaganda to induce the country

to arm itself adequately. Not a single leading politician, however, took his side—either from complete ignorance of military matters or from unwillingness to risk popularity. The theory, contrary to every military axiom, was held that the nation could defend itself with one arm only, the navy. The only change made by the politicians was to convert the volunteer force into the territorial force—perhaps equivalent to placing another bucket full of water by the side of a house which might be burnt down; and every soldier saw that that creation of the politicians, the Expeditionary Force, would in the case of a great European war be about equivalent to a pin against a sword. Our rulers, though they rejected universal service, might at least have arranged the organisation for such service in case of need—for recruiting, munitions, training, and all the other requirements of a national army; and this would at least have assured the Germans that if they were to attack us we should mean a serious defence. In the opinion of almost every soldier, if we had possessed a million trained men, as Lord Roberts advised, the Germans would not have precipitated the war, or, at least, would not have made their treacherous attack on Belgium—the war would have been either averted, or Belgium and Northern France would not have been forced to endure their present great suffering. It is also almost certain that if the politicians had unequivocally declared themselves on the side of France and Russia in July 1914, as the French Government begged them to declare themselves, the war would again have been averted.

After the outbreak of war, Lord Roberts' prophecies were immediately and necessarily fulfilled by the German conquest of Belgium, which, of course, was converted into a second base for air-raids and for submarine attacks upon our commerce. For more than a year and a half after the outbreak, the wretched oligarchy which ruled us did not possess enough resolution even to enforce universal service—thus compelling the noble young men who had volunteered at the commencement of the war to return to the trenches over and over again, even after frequent wounds, until they have been nearly exterminated, while the shirkers lived at ease at home. They allowed the country to remain full of spies. They permitted a rebellion in Ireland. They failed to enforce a thorough blockade against Germany. They enabled the king of Greece

to fill the Mediterranean with German submarines. They undertook expeditions—Antwerp, Gallipoli, Salonika, Mesopotamia, Palestine—which have been either disasters or only partially successful. Even now in the fourth year of the war the food question remains doubtful, the submarine problem is unsolved, and we have not provided enough aeroplanes to repell raids on the capital of the empire in the full light of day. But these subjects are too familiar for anything more than mere mention ; and about Turkey, Bulgaria, Serbia, and Rumania, Britons remain silent.

It is not denied that we have much to the credit side of the account. Our action was fully justified. Our navy has kept the surface of the ocean. Lord Kitchener created the new armies. Mr. Lloyd George created the new munition factories. Our men of science have found how to destroy Zeppelins and to control disease and cure wounds in the field ; and the War Office has organised gigantic supply and transport services. Either from the justice of our cause or from considerate handling of neutrals, almost all of these have now ranged themselves on our side. Above all, millions of our young men have shown a magnificent spirit in giving themselves voluntarily for the service of our country, and wonderful courage, endurance, and cheerfulness in the most trying of all wars; and our young women have helped in the hour of need and have not trembled in the hour of sorrow. Even our politicians have lately reformed themselves.

The body of the nation was healthy—but it possessed no brain. The fundamental error was the neglect by our politicians to arm the nation fully in the hour of danger—an error still more colossal than that which cost us our American colonies. It has had three results, first to give the Germans their opportunity, secondly to retard victory, and thirdly to squander the youth and the treasure of the country unnecessarily. As in the other case mentioned, the fault was that of our rulers—utterly ignorant of war, foreseeing nothing, preparing nothing, deciding nothing until driven by public opinion. Campaigns commenced without full consideration; military councils often packed with politicians ; political adventurers put in supreme power ; truckling to cranks—as in the cases of retaliation and the so-called conscientious objectors ; appeals instead of commands to the populace, even in the hour of extreme danger.

And much of the same sort may be said (but cannot be said now) of those who rule other nations.

CONCLUSION

The war is the war, not of the nations, but of their misrulers. The nations have been driven like sheep to the slaughter. They have done their duty. But what of those who plunged them into this tragedy?

The world is full of indignation. Who are these people who govern us? Hereditary princes, political adventurers, hustings-orators. What sanction of superior wisdom and virtue can they show to justify their position? After the war, the blood of our million sons and the sorrow of our million daughters will demand the reply.

Mankind, to which its own higher elements have given dominion over earth, air, and sea, still remains the slave of its own lower elements. The problem of the future is to discover how freedom from this slavery—the real slavery—can be won. Let us now proceed to examine one cause, and perhaps the immediate cause, of the evil.

The Reform of Democracy

DISTINCTIONS

Every one agrees that the world's first duty after the war will be to attempt to render future wars impossible. This implies the reform of all existing types of government.

The politicians pretend that the war was due only to Prussian militarism. So it was—in a positive sense. But in a negative sense it was also greatly due to their own neglect of the lessons of history and preference of personal and party interests to those of their country. Moreover, we must not forget that many wars and rebellions have occurred between and in democracies, and that our party politicians nearly produced a civil war in Ireland just before the present greater conflict began. Even if international wars are put a stop to, there will still be nothing to prevent incessant internal revolutions, separatist wars, and labour-wars in future democracies existing under the names of Right and Liberty.[1]

[1] On July 20 Mr. Lloyd George said that "Democracy is itself a guarantee of peace." Hardly; but no real democracy would be capable of the long-premeditated aggression of the German oligarchy.

The politicians also pretend that this is a war between democracy and autocracy. But what exactly do they mean by these words? In a sense, German autocracy is really the democracy of Germany—accepted willingly for years by the German people because they believed that it was a more efficient form of government than the babel-rule of England, for example. But if we take democracy to mean, not only a type of government accepted by the people, but one in which the people really approve and sanction every separate enactment—then neither the German nor the English form of government is a democracy at all. Each country is, in fact, ruled by a small *clique*, numbering much less than say one per cent. of the total population, and consisting of the same type of person, and that not the best type, in both countries. The principal difference was that the German *clique* were foolish enough to think that the war would be of benefit to them, while the English *clique* were wise enough to see the folly of it, but not wise enough to guard against it. And this difference was due to the facts that the German *clique* were controlled chiefly by soldiers who live by the sword and the English *clique* by lawyers who live by the tongue.

Our Politicians' Democracy

Electoral reform is now before the British people. Let us therefore examine the real nature of the so-called British democracy.

At present the House of Commons contains 670 members, elected by 8,357,648 electors, or one member to about 69,302 people on the average, and to 12,471 electors. How are they selected? By the "free voice of the people"? Well, actually the first selection of candidates is made by the Conservative, Liberal, Nationalist, and Labour organisations. These have branches in every constituency, consisting mostly of a few country gentlemen, local lawyers, manufacturers, builders, surveyors, tradesmen, working men, etc., who nominate the candidates who please them. There are no rules governing the selection, except that the candidate must conform in general to the programme of the party, and, if he wishes to succeed, should establish local "claims." Service to his country, experience of administration or affairs, virtue, trained

reason, science, learning, travel . . . obtain but few additional marks, if any, in comparison with party influence, local position, *savoir faire*, and, of course, a capacity for ready speaking. Lastly, as a general qualification required for all but a few, candidates or their friends must possess enough money to pay election expenses, say £500 for each election on a rough average.

All that the eight million British electors can do is to select in each constituency one of the two or three persons allowed to compete by the caucuses. It is only very rarely that " outsiders " have any chance at all—though there may be in each constituency hundreds of men willing to stand for Parliament and more fit for this important position than the nominees of the political organisations. Thus it is not true that the electors of Britain are free to be represented in Parliament by those whom they consider best. Evidently this is not democracy.

It will be said that the men do not matter, that one candidate is as fit as another to maintain a given policy, and that the electors are really there in order to select policies and not representatives. But as to the policy, who draws up the various political programmes ? Who but these same caucuses in consultation with their nominees, the candidates. Thus the independent elector is again cheated of his free choice, first as to his representative, and secondly as to the details of the policy which he must favour. For he must take the programme *en bloc* or not at all, and in order to support one detail of which he approves he may have to support also another of which he disapproves. If he persists in his objections he is simply passed over, swamped by the hosts of complaisant but probably thoughtless voters. Here again, then, the political organisations not only appoint the fiddlers, but also call the tunes. This again is not democracy.

As to the elections themselves, bribery is not allowed, but every other kind of influence is common—house-to-house canvassing or touting, flattery, cajolery, conveyance to polls in motor cars, subscriptions to local charities and institutions, support of private local enterprises, and probably much surreptitious business also. Again, then, the strict voter may be swamped by the good nature or the easier morality of the mass ; and the actual merits of a candidate or of his programme probably obtain less than half the total marks which go in

his favour. And behind it all is the long-prepared power of the local political organisations.

As to the press, each paper does little more than voice the official programme of the party to which it belongs, and a candidate with exceptional views is almost excluded from presenting them by this channel. As most people read only those papers which belong to the party which they favour, the influence of the press for forming sound opinion in Britain is very small; and indeed elections are usually won simply by the side which can bring the largest number of bigots to the poll—that is, by the less intelligent side. In other words the wrong party will be generally in power.

There are, of course, times when the voters kick over the traces of the political organisations, but this is probably seldom. Observation of the long series of general elections suggests that parties go in or out of power simply in consequence of "the swing of the pendulum"—that is, a change is made when the mass of indifferent but humorous voters say, "The reds have been in long enough, let the blues have a chance." How little intelligence lies behind British elections may be judged from the fact that neighbouring constituencies possessing precisely similar populations and interests frequently take opposite political sides for generations, being well known as "safe seats."

Who then are the persons who form the "political organisations"? Probably not one elector in a hundred knows the name of one of them; and the world knows them still less. Are they men of achievement, or of experience in administration, or of specially trained judgment? Well, they dwell in darkness, and appear to be chiefly local adventurers who find a profit in what they do, or gentlemen who buy titles by subscribing to party funds. What authority and sanction by law do they possess? None at all; they are simply private persons who have the impudence to band themselves together for the purpose of imposing upon the "free and independent electors" of Great Britain the men who shall represent them in Parliament and govern the empire. They are middlemen who have secretly taken democracy by the throat. They are parasites of the Constitution. We are really not governed by democracy at all, but by a kind of Tammany Hall.

What chance has the man of exceptional intellect to enter

Parliament? He is sure to be a busy man, and probably too preoccupied to be wealthy, and too honest to accept one item of a programme for the sake of another item. But he must sacrifice his time in button-holing caucuses, his money in paying for his election, and his conscience by joining a party! *Per contra*, what about the persons who do these things?

Party

Now let us consider what exactly we mean by *party*. If an assembly has a single difficult question before it, it may quite honestly be divided into two groups, a positive and a negative group. If the assembly has two independent questions before it, it may quite honestly be divided into four groups, one with two positive opinions, one with two negative opinions, and two groups each with a positive opinion on one question and a negative one on the other. If there are three or four independent questions before an assembly, it may quite honestly be divided into eight or sixteen groups, and so on. Yet in our political life, though we have many independent questions before us, we have only three or four groups! How is this? Because the word *honest* is omitted from our political combinations.

Thus if we suppose that the six questions, say, Compulsory Service, Free Trade, Home Rule, Welsh Disestablishment, Imperial Federation, and Woman Suffrage, are independent, then the politicians ought to be divided into sixty-four groups holding positive or negative opinions on all the items. But there were only four groups before the war. How was this? Some of the politicians must have sunk their opinions on certain items in order to get their way as regards other items. This is suppression of truth for an object. And if a man suppresses or misstates his opinion on any one point, how do we know that he does not, or will not, do so on any other? Of what value then is the voting in Parliament on any party question? For the expressed opinions of many or most of the voters may not be sincere, and the verdict therefore remains in the clouds. Thus it is that the verdict of Parliament on many of the questions mentioned above has remained in the clouds for generations.

Party is quite distinct from the natural division of honest opinion. Party is a false grouping of persons who suppress

their opinions on certain points (or refuse to consider such points), in order that they may carry decisions on other points which interest them more, or in order that they may stand well with their fellow intriguers, and, perhaps, pick up the plums of office.

It is argued that party is necessary for a strong government. That is, again, suppression of truth for an object. But the argument is merely advanced in the interests of party. There have been many strong governments which were not based upon party. An assembly of straight thinkers and voters is much more likely to organise a strong government than one consisting largely of professional prevaricators.

These facts are dimly apparent to all. It is admitted even by party politicians that certain questions requiring unbiassed judgment should be kept outside party politics altogether. Then why not all questions ?—for truth is never attained without unbiassed reasoning.

The party system was not always with us. At its birth it lost us our American colonies to begin with. Let us hope that before its forthcoming death it will not also have lost us the rest of our empire.

Party government means the preference of personal to national interests. No party politician is morally or intellectually fit to be a ruler of any nation. No nation which adopts the party system is morally or intellectually fit to hold the hegemony of the world.

Party Cabinets

The Ministers of State—the men who finally rule the country—are selected by the same subterranean methods. A *clique* within the party *clique* chooses the leader of the party, who then chooses his cabinet when he comes into power. Neither people nor Parliament possesses any vote in the selection nor any power to eject incompetent Ministers. Yet these men not only hold the corporate direction of national affairs, but individually dominate the executives of all the great specialised departments of State.

What qualifications have they for such powers ? That the political head of a State department needs have no knowledge at all of the subject dealt with by that department is proved by the fact that the same men are constantly being

moved from the charge of one department to that of another; and it is notoriously the opinion of the officials of the departments that their political heads seldom possess any capacity for organisation, knowledge of business, or acquaintance with the details of official work.

Our Ministers are, in fact, generally selected by the political *cliques* because of birth, influence, capacity for public speaking, and, chiefly, because by dint of skilful advertisement or hustings-oratory their names have become widely known to the public. What evidently tells most in the aspirant for political position is the advocacy of some political novelty, some universal political panacea, urged with burning eloquence —the sort of thing which establishes the success of a new patent medicine. Their popular reputation once secured, nothing can oust them from present or future employment; and for no degree of demerit can they ever suffer impeachment. They become political histrions, rivals of the popular heroes of the stage; and the premier becomes an actor-manager anxious to have as many stars as possible in his galaxy.

Neither people nor Parliament seems to possess any real control over their executive doings. Their answers to interpolations and deputations are often intentionally misleading, because they tend to look upon such matters as vulgar impertinence—like the booing of a displeased spectator in the gallery. What is really done under them in the public departments is never known, or, if it is known through a royal commission, brings no real censure upon them. Their very speeches are generally mere rescripts of previous newspaper articles, uttered before appreciative audiences of the same party, and containing as little truth as a commercial advertisement.

On the other hand, they are generally men of ability, and are often sincere, energetic, and hard-working. But that is not the question. The world's rulers should be not only able men, but the most able men procurable. The question is ultimately that of genuine intellectual predominance.

Summary

At present, then, this country is really ruled by an oligarchy consisting of say a hundred thousand professional politicians, political wire-pullers, and journalists who sell their newspapers chiefly by means of party propagandas. The system can be

considered a democracy only in the narrow sense mentioned at the beginning of this article, that it is a type of government generally allowed by the people.[1] Except as regards military power, it is really only a little more democratic than the German system—in which similar defects exist, the German Emperor, for instance, being himself the principal advertising demagogue in that country. Neither system is democratic in the true sense—that the people themselves shall sanction every separate enactment, legislative or executive, made in their name.

The war was due, either in a positive or in a negative sense, to the facts that the difficult task of government is in the hands of inferior oligarchies consisting of men (not of the highest intellectual type) who either profit by war or lose little by it, and that it is not in the hands of the people themselves, who are always the sufferers.

What Shall we Do, then?

Only two courses appear to be open, namely—

(1) To maintain the oligarchic system, but to make such reforms in it as will ensure genuine representation of the people, will illegalise the party system, forbid the political wire-pullers, expel the professional politician or autocrat, and place the government of the country and the management of State departments in the hands of the very best qualified persons available; or

(2) To remove the representative system altogether as being no longer required in these days of railways, posts, and telegraphs, and to subject every important legislative and executive enactment, and the appointment of heads of the great departments, to the entire people themselves by referendum—thus making the people themselves really responsible for their own welfare.

The writer is by no means convinced of the feasibility of the former alternative or of the advisability of the latter one; but hopes to discuss the matter impartially in future articles.

[1] For the scientific reader, especially a mathematical one, it is scarcely necessary to argue at length that the majority-decision of the people upon any particular proposed measure cannot be ascertained at all by the methods of voting and election now in use. A member of Parliament represents his constituency only, not the decision of his constituency upon individual measures in detail.

Both alternatives will spell revolution. Our Parliament has already rejected the former one by throwing out the clauses for proportional representation in the Electoral Reform Bill—action not to be wondered at when we remember that any genuine electoral reform will cast forth into the wilderness the whole host of false prophets who failed to guard against the war and who have been misgoverning the country in many ways for years past. The second alternative will prove equally hateful to them, for they and their caucus-masters will no longer be able to misrepresent the people. An era of incessant strife is before the world. Only thought and science can guide us through it.

From inquiries, the writer gathers that the opinions expressed in this and the preceding article are now held by at least ninety-nine per cent. of the British people, that is by all except those engaged in the profession of party politics, and a few doctrinaires. At the same time, the whole nation is determined to have it out with the Germans, not in order to substitute one kind of oligarchy for another, but in order to punish them for the worst crime in history. And, after all, that is the way to prevent a repetition of the same crime in the future.

Aircraft and Motor-Car Engine Design (J. Wemyss Anderson, M.Inst.C.E.)

The paper read by Mr. Louis Coatalen before the Aeronautical Society of Great Britain at the Royal Society of Arts on May 16, 1917, lifts the veil, if only a little, on the very great progress that has been made since war was declared in the design and construction of aircraft and motor-car engines. The full title of the paper has, added to the title given above, "contrasted from the standpoint of a designer and manufacturer of both types." Mr. Coatalen is, of course, connected with the Sunbeam engine, and consequently we know him to be a successful designer and a successful manufacturer. The paper has been passed by the Censor, and therefore we can be content that the enemy will not be able to glean from its pages any information of a practical or technical nature. The record of progress, however, is so great that we are assured that the enemy will not be able to gather any crumbs of comfort from its reading.

Mr. Coatalen opens with a comparison between the characteristics of a motor-car engine and one for aircraft service, and he demonstrates very clearly "that the aircraft engine is quite a distinct branch of the development of the internal combustion engine." He acknowledges the value of experiences gained from racing cars at the outset of the war in aircraft-engine design, but "on the coming of peace, doubtless it will be found that the position has been wholly reversed," *i.e.* racing cars will have a great deal to learn from aircraft experience.

Doubtless, also, on the coming of peace Mr. Coatalen will be able to place on record details of how the great progress—only outlined in the present paper—was

really achieved, and thus show in his particular branch of engineering how Great Britain managed to regain her lead in the engineering world.

The paper is an outstanding contribution of scientific thought applied to a technical subject, and indicates more clearly than any paper on "engineering education" could possibly do, how essential it is that an engineer should have a thorough scientific training. Selecting, perhaps, not the best example to illustrate this point, but an example of general interest, Mr. Coatalen, in a long list of "desiderata in aircraft-engine design," draws attention to the fact "that some of the most daring Service fliers have not either the temperament or the understanding to spare the engines of which they are put in charge."

In "three years' progress at a glance" Mr. Coatalen cannot, of course, give any information on the relative reliability of the engines of to-day compared to those of three years ago, but we gather that all is well; but he does state that for a six hours' effort the weight of the engine with fuel has been brought down from 11·27 to 5·3 lb. per horse-power hour, while the consumption of fuel has been reduced from 0·6 to 0·52 pint per horse-power hour.

The Conjoint Board of Scientific Societies

The fourth meeting of the Conjoint Board of Scientific Societies was held at the Royal Society on June 13, Sir Joseph J. Thomson, O.M., Pres. R.S., in the chair, to receive the report of the Executive Committee on the work of the previous six months.

As the Report indicates, a number of important questions of scientific and industrial importance have occupied the attention of the Board.

Various bodies are at present interested in the formation of a census of the mineral resources of the Empire. It was agreed to enter into communication with these bodies and to make suggestions with a view to the publication of information in a form useful to the general community.

Interim reports were received and approved on the necessity for an anthropological survey of the British people, on the best methods for carrying on the International Catalogue of Scientific Literature, and on the inquiry into the desirability or otherwise of the adoption of the metric system throughout the British Isles.

The Sub-Committee on National Instruction in Technical Optics reported that a scheme approved by the Board of Education had now come into operation. Under the ægis of the Imperial College of Science and Technology an Advisory and Administrative Committee had been formed to organise instruction, and Mr. F. J. Cheshire, a member of the Sub-Committee of the Board, has been appointed Director of Studies in Technical Optics. Although the President of the Board of Education did not see his way to adopting the suggestions of the Sub Committee, the Board heard with satisfaction that a promising effort has been made to solve a question of considerable national importance.

A sub-committee, having considered special cases of magnetic disturbances revealed by a magnetic survey of the British Isles and their possible connection with the occurrence of iron ores, recommended a detailed investigation of two test areas, in order to ascertain how far, under the conditions of the British iron ores, the magnetic survey was likely to prove of economic value. Arrangements for carrying out the investigation are in progress.

An Agricultural Sub-Committee, with the Earl of Portsmouth as Chairman, reported that it is at present devoting itself mainly to engineering questions. It

is engaged in collecting information with regard to the transport of raw materials to farms and agricultural products from them, to the power required for this purpose, and for seasonal operations on the land, with a view to comparing the relative advantages and costs of steam or internal combustion engines and electrically operated machines. It is dealing also with the possibility of co-operation in repairs and skilled labour, and is considering the various types of tractor most suitable to large and chiefly arable farms and to moderate-sized mixed farms, having regard to the different local circumstances and requirements.

A sub-committee was nominated to report on what is at present being done to ascertain the amount and distribution of water-power in the British Empire.

A complete report of the first year's work of the Board will be published in due course.

The British Science Guild and Experiments on Animals

We print a letter from the Science Guild to the Home Office on the subject of experiments on animals and the reply of the latter. It is doubtful whether the reply covers quite satisfactorily all the points raised in the letter; but we are glad to see that the mere abstraction of small quantities of blood from animals for examination is not regarded as an experiment within the meaning of the Act—a point which not everyone knows. Many workers have complained of the delay which occurs in the granting of licences and allowing certificates; and the reply of the Home Offiee seems scarcely sufficient, as such delay used to occur long before the war. In our opinion no impediment, however small, should be placed in the way of men who are endeavouring to find the secrets of those diseases which destroy millions of human beings and animals; and we regret only that the whole of the Cruelty to Animals Act (whose very name is an insult to men of science) cannot be entirely reformed by our legislature.

BRITISH SCIENCE GUILD, 199, PICCADILLY, *28th Feb.* 1917.

SIR,—I am directed by the British Science Guild to inform you that the Guild has recently addressed a circular letter to a large number of teachers and investigators on Zoology, Physiology, and Pathology, for the purpose of ascertaining whether the Cruelty to Animals Act imposes any difficulties upon investigators in those branches of science. The replies received show in general that the difficulties which exist are principally minor ones, and the following suggestions for amendments have been put before the Guild:

(*a*) Removal of necessity for obtaining various signatures when applying for additional certificates for use in conjunction with existing licences.

(*b*) Power to employ an unqualified assistant in carrying out minor operations, under reasonable restrictions.

(*c*) Removal of frequent delay in granting licences and certificates.

(*d*) Comprehensive certificates, comprising all certificates now granted, to be given to workers of experience and status.

(*e*) The system of returning all licences and certificates, which has been abandoned during the war, might be abandoned altogether.

(*f*) The simple abstraction of blood from an animal for examination should not be considered an experiment within the meaning of the Act.

(*g*) Special powers to perform trivial operations such as injections, inoculations, and venesections, without anæsthetics, might be granted.

(*h*) The heads of laboratories might be allowed permanent certificates on condition that they make returns as usual.

(*i*) A qualification in medicine should carry with it the qualification to perform biological experiments.

(*j*) Pathological experiments on animals should be allowed in all military hospitals without special application.

I am now directed by the Guild to inquire whether you, Sir, are of opinion that any of the suggestions may be met by simple administrative amendments in the working of the Act. The Guild is of opinion that at the present moment, when very large numbers of soldiers are being treated in military hospitals, everything possible should be done to enable the physicians and pathologists in connection with such hospitals to ascertain by experiments on animals (if necessary) the nature of the complaints from which the patients are suffering, and the best methods of treatment to be adopted. The Guild therefore trusts that the working of the Act will be made as smooth and easy as possible within its provision.

I have, etc., ALEX. PEDLER, Hon. Secretary.

THE RT. HON. THE SECRETARY OF STATE, Home Office.

HOME OFFICE, *17th April*, 1917.

SIR,—I am directed by the Secretary of State to inform you that he has carefully considered your letter of the 28th February last making certain suggestions for amendments in the procedure under the Act 39 & 40 VIC. cap. 77. The Home Office administration is carried out in accordance with the provisions of the Act, and the more important of the changes in procedure now suggested could not be put into practice without legislation. The Secretary of State regrets that he does not see his way to introduce legislation for this purpose.

With regard to paragraph (*c*) of your letter, I am to say that any delay which may have taken place at the Home Office in granting licences and allowing certificates has been due generally to pressure of work and shortness of staff owing to the war; but in cases where there is any special urgency, every effort is made to expedite matters. Delay is often caused by the applications and certificates not complying with the requirements of the Act, or not giving adequate information as to the proposed experiments.

In all that concerns Military Hospitals and the requirements of the Army Medical Service, everything possible has always been done to meet demands expeditiously, to remove obstacles, and to facilitate work.

I am to observe with reference to paragraph (*f*) that the simple abstraction of a small quantity of blood for examination—otherwise than in the course of an experiment—is not regarded by the Secretary of State as an experiment within the meaning of the Act; and with reference to paragraph (*g*) that the special powers referred to are frequently allowed under certificate A, there being no other way of giving them consistently with the requirements of the Act.

I am, etc., EDWARD TROUP.

THE HON. SECRETARY, British Science Guild.

Geological Notes of Queensland (North Queensland Register)

From the Gulf of Carpentaria to the Darling Downs, north to south, the fossil remains of extinct mammalia have been found in indurated muds, the beds of old watercourses. The fossils are *Diprotodon australis*, *Macropus titan*, Thylacoles, Phascolomys, Nototherium, crocodile teeth, etc. The Diprotodon inhabited the Queensland valleys freely, and the *Crocodilus australis* had a great range inland. The Diprotodon remains are found chiefly in the most permanent waterholes. No human bones, flint flakes, or any kind of native weapons have yet been discovered with the extinct mammalia of Queensland.

Desert sandstone is the most recent widely spread stratified deposit developed in Queensland. Since it became dry land the denudation of this formation has been excessive, but there is still a large tract *in situ*. Probably this desert sandstone covered the whole of Australia at one time. (It is possible that desert sandstone in Queensland has value for free gold.) On the vast plains west of the dividing range cretaceous strata are found; hot alkaline springs occur in these plains, and the discovery of these suggested the possibility of the existence of

artesian water long before the bores were sunk from which flow "Queensland's rivers of gold."

The whole of Queensland is a vast cemetery of fossilised species—on the surface, buried in drifts, or hidden in clays. The plains of the Flinders river disclose great deposits of marine fossil shells, belemnites and ammonites, and remains of extinct animals. In the Gulf of Carpentaria, 40 or 50 feet below the alluvial deposits forming the banks of rivers, firmly embedded in the hard cement—water-worn stones in an ironstone clay—are the bones of innumerable extinct gigantic animals that, far back in some prehistoric age, roamed over the Gulf country: Diprotodon, Nototherium, and Zygomaturus—grass-eaters and flesh-eaters. The utter extinction of these creatures can only be explained by a great change of climate and great and lengthy droughts. The fossils are from animals of immense size; the teeth found are twice the size of an ordinary bullock's. Gigantic alligators and turtles and marsupials abounded in those days, suggesting a luxuriant and abundant herbage.

From an economic point of view one may say that three-fourths of the area of Queensland forms good pastoral land. Of this, 60,000 square miles contain valuable mines of gold, with outcrops of copper and lead ores, as well as rich deposits of tin; 24,000 square miles are capable of producing illimitable supplies of iron and coal. It may be safely asserted that in Queensland is a wealth of material resource comparing favourably with any other part of Australia.

Pamphlets and Periodicals

The New East, edited by J. W. Robertson Scott, is a new periodical, written in the English language but published at 12, Ichibeicho Nichome, Azabu, Tokyo, Japan, their London agents being the Far Eastern Advertising Agency, 36, Southampton Street, Strand. It is new in every sense—in its aim, in the quantity of ground it covers, and in that it is the first number of a new periodical. It contains, for a very moderate sum, quite an unusual amount of reading matter on a great variety of subjects and appealing to every type of mind, and, unlike most periodicals, it is not a collection of odd articles, but has a constructive purpose which justifies its birth in these days of over-literary production. The Editor says: "*The New East* has two objects: (1) To interpret to the West the best in the thought and achievement of Japan, and to Japan the best in the thought and achievement of the West. (2) To develop, by better acquaintance between the British and Japanese peoples, the good relations which so happily exist between Japan and Great Britain. The Review has secured the help not only of the best writers of Japan and the Far East, but of those most competent to survey and interpret the thought and progress of Great Britain, India and the Oversea Dominions, the European Continent, the United States and South America." Prof. M. Anesaki explains why a large proportion of the Japanese people is pro-German in spite of the fact that nearly all the leading papers of Japan are anti-German. "There have been two periods in the growth of German influence in Japan," he says; "the first was consequent upon the defeat of France by Germany in 1870. This induced the Japanese military authorities to adopt the German model in place of the French, which had been followed previously. The second dated from the middle of the eighties, when Prince Ito found the German constitution and jurisprudence the most congenial to Japanese needs." He gives as another reason that "students staying in Germany or officials making tours of investigation could easily get information and insight, more or less, in Germany," while "an English or American school, for instance, could be under-

stood only by living in it for a long time; and even if one institution could be understood in that manner, it would never stand for others, for each has characteristics of its own." He himself characterises pro-Germanism as "a disease in the political and social life of Japan" which will be destroyed if the Allies are finally "successful not only in military and naval engagements, but in social, moral, educational reconstruction to be achieved brilliantly after the war." It is surely a lesson to Europe that Prof. Anesaki had the perspicacity to write an essay in the first year of the century warning his country against the deleterious influence of Germany, and severely criticising the morality of that country. Both Viscount Motono (Foreign Minister) and Marquis Inouye (lately Ambassador in London), in two independent articles, strongly advocate a lasting and firm alliance between Great Britain and Japan. Another article deplores the barrier that exists between East and West—that of "distrust of each other's morality," which "only time, great liberality of mind, and continuous effort can break down." The section headed Literature includes the translation of a Japanese classic and a good article on "Nicheron, the Buddhist Prophet." Some articles are addressed to the paper's Japanese readers, in which the authors, although pointing out Japanese faults fearlessly, steadfastly cling to their faith in a stable future for Japan.

The Hindustan Review contains two articles (May-June) on the subject of self-government for India, and although one is written by a European and the other presumably by a native of India, they both concur in the opinion that the country is not yet ripe for this step. It is pointed out that those who clamour for home rule are eager for the prerogative of power but are not prepared to shoulder the responsibility of self-defence. It is also stated that there is usually much competition by employees for work under Government in preference to work in firms under purely Indian management, owing to the fact that these firms pay very low wages and give no prospect of a definite tenure of office. On the other hand, in a paragraph of the section "Some Topics of the Day" the editor declares that India has a right to some independence, but brings forward little proof of her capacity in this direction.

The great movement against the neglect of science that has been carried on by scientists for the last few years is having a wider effect than many at first realised. Not only is the immediate object of the extension of scientific education in the schools being accomplished, but the idea of the necessity of the scientific thought in all walks of life is being recognised. In the **Australian Manufacturer** (April 21) Mr. Thomas Poole gives a long article on "The Scientific Method as a Solution of the Labour Problem," in which he says: "To the individual who prefers to cherish ideas of national or communal ownership, we point out that the scientific method, by its disciplinary routine and its interest-compelling and intellect-awakening attributes, will prepare the populace for the Higher Democracy." He then quotes Dr. Frederick W. Taylor, who originated what is known as the Taylor method: "Scientific management has for its very foundation the firm conviction that the true interests of the two (employer and employed) are one and the same . . . and that it is possible to give the worker what he wants—high wages—and the employer what he wants—a low labour cost—for his manufactures." Dr. Taylor's four "basic principles" are also noteworthy: "First, The development of a true science. Second, The scientific selection of the worker. Third, His scientific education and development. Fourth, Intimate friendly co-operation between the management and the men." The conclusion arrived at is that "Scientific management will mean for the employers and the workmen who adopt

it . . . the elimination of almost all causes for dispute and disagreement between them. What constitutes a fair day's work will be a question for scientific investigation, instead of a subject to be bargained and haggled over."

The author of "The Six Hours' Day," published in the same periodical as in the foregoing paragraph (April 14), brings forward Lord Leverhulme's proposition "that on an average six hours ought to be given each day to private business and two hours to the State," and he suggests that two shifts of men working six hours each would meet the employers' needs. He also maintains "that each reduction of the daily hours of labour has invariably resulted in increase of production, improvement in quality of product, and higher wages." The same conclusion is reached by Dr. Vernon in his Report on the Health of Munition Workers. Lord Leverhulme also treats of the subject of Labour and Capital's Co-partnership in a series of three articles in the **Sunday Times** (July 22, 29, August 5). The first revises the position of Labour from the Middle Ages to the present and attributes the wearisomeness and monotony of the work of the modern operative to the vast scale on which industry is now conducted, which renders contact with employer and employed impossible, while the increased use of machinery robs of all variety the daily task of the workman. The remedy suggested is that of allowing time off for the men to go through the factories and see the different processes and the completion of the article of which they undertake only a small part—a plan which has, he says, been adopted in some iron and steel works and resulted in increased interest in the work. The second article draws attention to the disintegrating factor of distrust between capital and labour, resulting in the workman refusing to give intelligent thought to his work, and emphasises the fact that at all costs the workman's interest in his work must be aroused. Lord Leverhulme doubts whether this can be done with the middle-aged man, inured as he now is to his mill round and an absence of the right kind of ambition, but he hopes much for and from the younger generation. The general trend of present-day thought on this subject seems to be that, if the conditions could be so adjusted that the work itself would be of as much importance to a man as the wages derived from it, the root of the difficulty between capital and labour would have some chance of being destroyed. The third article, besides restating the necessity of copartnership between employer and employed together with a six hours' day for men engaged in monotonous work, advocates that two hours daily should be devoted by such men to education between the ages of fourteen and thirty. These two hours should include military training in early manhood and, in later years, work in their own gardens. "So," says Lord Leverhulme, "we shall be opening up fresh resources for the good of the nation. The precious plant of inventiveness is not grown in a close atmosphere of prolonged labour; it is the product of leisure hours, when a man's mind is free from task-work."

The Report of the **Committee on the Neglect of Science** for 1916–17 states that, since its meeting in May 1916, the Government has acceded to its request and has appointed a Committee on the subject of scientific education in Secondary Schools, and that representatives of the Committee on the Neglect of Science should attend on October 14, 1916, to state its views. In November 1916 another Government Committee was appointed to consider the revision of the existing scheme of examinations for the Home Civil Service.

The first number of a quarterly called the **Journal of the Society of Glass Technology** appeared in May. The secretary of the Society informs us that "The issue is of some interest as the Journal is the only one of its type, just as the Society of Glass Technology is the first Society to be formed to study glass

comprehensively. Germany possesses three or four periodicals of the weekly or fortnightly magazine type dealing with glass, but nothing in any way comparable with the new Journal." It will be remembered that the British Science Guild did much after the outbreak of war, not only to point out the urgent need of investigation to enable Britain to manufacture glass for scientific purposes, which in pre-war days had been a monopoly of Germany and Austria, but to encourage its manufacture in this country, and also to standardise microscopes, and so on.

This and That (The Editor).

The whole world, like a sinner that repenteth, is talking of **reconstruction** after the war, but we hope that it will not imitate the personage who when he was sick determined to be a saint. I am glad that British efforts in this direction have been put in the charge of the Right Hon. Dr. Addison, M.P., a man of science as well as a politician; but to be effective the reform must be one of the spirit; and I suspect that it will be the penmen and not the politicians who will lead the revolution—as they did 150 years ago. What reforms of the spirit, then, should we advocate in our own country? First of all, we must disabuse ourselves of the notion—ingrained in us by the efforts of politicians, lawyers, trade-unions, and newspapers—that our rights are more important than our duties. For many years past we have heard in this country almost nothing but one universal clamour for *rights*—*rights* of labour, of women, of aborigines, of small nationalities, of animals, of criminals. Democracy has become a babel of mean people, all shrieking together for their *rights* and beating, biting, and scratching each other to get them. The war was caused by the Germans in order to assert their own *right* to a place in the sun and that of Demigod William to govern the world. Every newspaper, especially every radical newspaper, is plastered all over with the word *rights*—the right to refuse to fight for your country, the right to make friends with your country's enemies, the right to be idle, the right to be fed and to have your children educated by others—all finally merging into the universal right of everyone for anything he pleases. But our duties?—oh yes, of course, we have our duties—which we will perform when we have obtained all our rights! . . . The end of this cult is hell. That hell is paved with good intentions (including reconstruction), but the damned who shriek there are roasted for ever in the burning pit of their own imaginary rights, while the heaven which they see through the smoke and cannot attain to is built upon the rock of duty. The war is a part of that hell. . . . Surely a false cult, this one of rights. Not that of the Great Teaching which was given to us nearly two thousand years ago in times of much wealth and prosperity (and rights and strife, like our own) by the pale Figure Whom we see and ignore in our churches. Rights!—there are no such things. Phantoms of self diseased, when we attempt to define them they melt into air. We have no rights at all. We may claim only such privileges as nature and our fellow-men allow us. But our duties are fixed for ever.

The second great reform of the spirit must be the abandonment of the right to be fools. This too has recently become a cult. The most popular character on our stage is the silly comic-man; and the hero and heroine themselves are generally fools also—to judge by the ease with which the villain gulls them. Folly has become one of the perfect gentleman's properties; and the man who knows anything is either the pantaloon uncle or the villain of the piece. Wisdom and knowledge are derided even in the schools instituted to impart them, and our lecturers on education are always belittling them in favour of what they call character—usually another name for a carefully cultivated hypocrisy. But the

fool is really a waste product—of no use as a citizen, and one who ends by living upon others and clamouring for his rights; and the nation which contains too many such people naturally goes down in the world. We are also too fond of claiming a right to hold our own opinion. There is no such right. A capacity for sound judgment can be earned only with great labour by the study of almost all branches of knowledge combined with an express intention to practise the art of weighing evidences and arguments against each other before forming conclusions. But the opinions of most men seem to be born with them, like their nose. When a man claims a right to hold an opinion he really claims a right to be considered a capable judge. A nation which admires fools (though I suspect poor Mr. Bull is not always such a fool as he would like to be considered) will therefore seldom have sound opinions, and it is notorious that England is the home and generator of every kind of crank, from the party politician to the anti-vivisectionist. These imposters often thrive well, become members of parliament, journalists, schoolmasters, and priests, retard education, science, colonial development,[1] and, finally, disgrace the country to which they belong, first because they exist and still more because they are listened to. Personally, I can respect an opinion only if I can respect the man who holds it; and I cannot respect any man who is culpably ignorant or who shows no desire to train his judgment.

Together with these reforms, we should like to see in this country a much higher regard for work, discipline, and good manners, and, what Goethe praised so much, reverence for achievement. They all go together. The fact is that we have carried the doctrine of individualism (originally a sound one) to excess. To be a man, a man must be more than himself. The rights-monger, the fool, the character-monger, the idler, the liberty-monger, the cub—all consist only of their self. The free and independent person, and the man who is as good as you, are not wanted much longer. We are not separate stars, but only atoms in contact with each other.

The war has already burnt off much of this old stubble. And the young Briton in khaki—is another being. He knows ten times what he did. He can command and be commanded. Healthy, burnt, active, experienced in realities, he is yet modest, courteous to all he meets, respectful to seniors, and even acknowledges that he has superiors in the world; for he has given his little self, his life, and his rights unreservedly into the large hand of his country, and has received in return the only right there is—that of self-respect. A nation in arms and a nation of gentlemen. Reconstruction after the war!—this is reconstruction *now*, and the best we can have.

We owe it to the Germans. Let us be just to them. They have always taught that duty (to their own Government) is above rights, and we have called them slaves for their obedience. Unfortunately they used the great power which this principle gave them for the basest of purposes. The supreme wrong which they have inflicted on humanity has been that they taught virtue the service of evil; and that will be their epitaph in history.

Here are some more possible details of reconstruction. A real effort should be made to abolish *slums*, the great defect of British and American civilisation, and probably of democracy—apparently due principally to the fact that town councils are dominated by jobbers. There is also the *drink problem*: of which the solution appears to me to lie in the abolition of our secret drinking dens called

[1] For example, the important sanitary measure of segregation for white men in the tropics has long been opposed by cranks, because it would be wrong for white men to separate themselves from their coloured fellow-subjects!

Public Houses and the substitution of cafés on the continental system—open-air ones as much as possible. The argument that our climate is too rainy will not hold, because open-air cafés are numerous in Holland and Belgium, where the rainfall is much the same. The open-air café allows Bacchus to remain a god, but the secret drinking den makes him a sot! Those who can have a glass of beer or a cup of coffee in the open obtain rest and fresh air at the same time; but in England the poor literally have nowhere to sit down in the streets except in the stinking bars of public-houses, where tired labourers are obliged to resort in order to obtain a little rest. In my experience, in no country are the poor so seedy and weedy as in Britain—partly owing to slums and partly to the drink evil. Another suggestion is that many more of our beautiful *woods* and *fields* should be laid open to the public than at present. Boards denoting that "trespassers will be prosecuted" are seen everywhere, even in places of which the owners can never make any real use except for shooting. I would suggest a special tax on lands kept private far from dwelling houses. Still another suggestion. At present, owing to the fact that all the schools break up for *holidays* at about the same date, there is a wild rush at that moment for the seaside, with the result that hotels and lodging-houses are overcrowded and that the owners can charge extravagant rates for them. But if arrangements could be made whereby schools should commence their summer holidays on different dates, say from June 1 to August 1, this crush would be avoided, and people could also send their children to the schools which break up on dates which they prefer. This would materially affect hotel charges, which are now excessive—with the result that country hotels remain almost vacant all the year round except for a few weeks in summer and a few days near bank holidays, when the owners have to charge double and treble rates in order to enable them to keep their hotels going at other times. A greater measure of fresh air for the whole people of Britain is such an important matter that these subjects ought to receive careful attention from Government. Another point which I would like to see attended to is the *purification* of the stage, the press and novels. It is merely a matter of discipline, and when the war has taught us some of this forgotten virtue, we may possibly be able to enforce it in civil life.

Three small but important **books** on *Education* have recently been issued. They were received too late for review, but I would advise the reader to buy them. Mr. V. Seymour Bryant (Wellington College) deals with the *Public School System in Relation to the Coming Conflict for National Supremacy* (Longmans, Green & Co.). Lord Raleigh contributes a Preface, and the contents consist of Education and the Preparatory School, the Public School Curriculum, the Rôle of Science in the Educational Scheme, and the Advantages and Defects of the Public School System, and concludes with a chapter on Constructive Policy. It is the work of a practised teacher on scientific subjects and not merely a set of generalisations. Prof. John Burnet's book on *Higher Education and the War* (Macmillan) is ably, but in my opinion casuistically, written. It argues the case for the so-called humanistic ideal of education, and reminds me of a certain mathematical book of great length which seems chiefly to be concerned with proving that two and two make four—with this difference, that I think Prof. Burnet aims at proving that two and two make five. In my opinion the best book on education which I have seen since the one of Herbert Spencer is that of Dr. Charles A. Mercier, called *The Principles of Rational Education* (Mental Culture Enterprise, 329, High Holborn). This work deals with the subject in a scientific spirit and deserves a full review.

ESSAYS

LATIN, GREEK, AND ENGLISH (Charles Mercier, M.D., F.R.C.P.).

It is an accepted truism that a knowledge of Latin and Greek confers a mastery over the English tongue. It is known that only by acquiring a thorough knowledge of Latin and Greek does it become possible to speak and write English correctly and forcibly. We know that this is so, because it is well drummed into our heads at school, and because it is the chief reason, or one of the chief reasons, that is always alleged in support of the otherwise unaccountable practice of making a knowledge of Latin and Greek the chief objects of education, and practically the sole objects of education, on the classical side of our public schools. It is true that in practice we do not find that classical scholars exhibit any eminent mastery of the English tongue. It is true that the English of a classical scholar may usually be recognised by its nebulosity, by its Latin idioms, by its misplaced relatives, and by its general inferiority to the English of those who have not had the enormous advantage of a classical education. It is true that the English of the public school and classical University man is usually as vile as his handwriting is illegible and unformed; but classical education is an affair, not of facts, but of words. The public school and University man is not expected to know the difference between a fact and an assertion, and he does not know it. Consequently the facts that a classically educated man rarely writes good English, and, other things equal, usually writes worse English than the man who has not had a classical education, go for nothing at all. The assertion that a classical education confers ability to speak and write good English is made by every schoolmaster, is repeated by every one who has had the misfortune to undergo a classical education, and as the assertion is made, it is taken for a fact, and no doubt these classically educated gentlemen will be astounded to hear that there is a difference between an assertion and a fact.

A very fine specimen of the English of the classical scholar has recently come into my hands, and I feel that it is worthy of wider publication than its author modestly gave to it. It illustrates so well the vast superiority in the mastery of the English tongue that is conferred by a classical education that it would be a shame to conceal it and prevent it from attaining the admiration that is its due. It is the more important since it is the utterance of one of those fine flowers of classical scholarship, the headmaster of a great public school. If these things are done in the green tree, what will be done in the dry? If this is the English of the headmaster, what is the English of the assistant masters likely to be? and above all, what will be the English of the scholars? The hero of this exploit is M.A. of Oxford, and Scholar of a celebrated Oxford College, at which Latin and Greek are cultivated "intensively," to use the latest slang, and this is a specimen of the mastery of English that this intensive study of Latin and Greek has conferred upon him:

X College.

Easter, 1917.

"The holidays end on Wednesday, May 2nd, 1917.

"Boarders must report themselves to their House Masters as soon as they arrive in X; [How the Boarders are to know when their House Masters arrive

in X we are not told. Presumably, the Boarders are to wait at the station until the House Masters arrive; but how if the House Masters arrive in X by motor car?] and none may return, *except by special leave obtained from their House Masters, by any train that is due at X later than* 8 *o'clock*."

If this had been written by a common vulgar Board School person who knew no Latin or Greek, we should have sworn that it contained a false concord, and that the plural demonstrative pronoun "their" referred to the singular numeral "none"; but this was written by a classical scholar, a Master of Arts of the University of Oxford, Scholar of Y Z College, a man whose knowledge of Latin and Greek has conferred upon him that mastery of the English tongue which a classical education always does confer, and which can be attained in no other way. That such a master of English should ever perpetrate a false concord is not to be thought of; but that he should perpetrate an obvious and elementary false concord in composing official instructions for the boys of the school of which he is headmaster is such a wild improbability that it must be dismissed from conjecture, and we must find some other meaning in this remarkable regulation, which is so important that in the original before me it is underlined. According to the ordinary rules for the composition of English, the pronoun "their" should refer to the next preceding plural noun, and the next preceding plural noun is House Masters, so that we must "conclusively presume," as the lawyers say, that none may return except by special leave obtained from the House Masters of the House Masters. It would appear, therefore, that in this public school the House Masters are arranged in a hierarchy, like the big fleas and the little fleas in Swift's epigram. Perhaps that is why it rises above the common designation of a school, and is called a College.

"Boys are subject to all College rules from the moment of their arrival in X." Quite right, but when do the rules arrive in X? When they issue from the pen of the headmaster, whose classical education has conferred upon him a head-mastery of English? or at what other time? And how are the boys to know at what moment the rules arrive? A little explanation here would have been kind to the boys, but it is probably beneath the dignity of a headmaster to be kind to the boys.

"All boys (whether previously at the College or not) are to be in School at 8.30 a.m. on Thursday, May 3rd." There are two nice distinctions here that no doubt the boys are taught, in their studies of English, to appreciate, but that are puzzling to an outsider. "All boys are to be in the School at 8.30," that is plain enough, but are all boys subject to all College rules from the moment these rules arrive in X? We are not told so. We are told that "boys" are subject to these rules—that is to say, boys as distinguished from girls, men, and women. Presumably, therefore, the College receives girls as well as boys, and it is to the boys alone that the regulation applies. The girls need not conform to the rules the moment the rules arrive in X; but are all boys to conform to them? Boys, as any Master of Arts of the University of Oxford must know, since it is a well-known logical rule, and Oxford is the home of logic—boys may mean all boys or some boys, and which the term means in this context is not explained. However, all boys (whether previously at the College or not) are to be in the School at 8.30 a.m. on Thursday, and this introduces us to a nice distinction between the College and School, a distinction familiar, no doubt, to boys, but whether to all boys or to some boys we are left to conjecture.

"Boys who are not prevented by illness, or some other equally urgent cause, *must* return punctually to the day, and will be held *personally responsible* for their

punctual return." Again we notice the implied privilege given to girls, another injustice to the male sex. Girls, it would appear, may return on any day they please, but boys *must* return punctually to the day—a very proper regulation, no doubt, but which day? Two days have already been mentioned, and a third is mentioned in the next regulation but one.

"Where one of two brothers is for any reason detained at home"—Where? why, obviously, at home. Surely the boys that attend X College are not so stupid as not to know they are at home when they are at home, but no doubt the headmaster knows best "the other must not be kept back, except where one of the two is too young to travel alone, in which case the House Master should be communicated with, and permission asked." This is clear enough. The only place in which the other may be kept back is the place in which one of the two is too young to travel alone. What curious boys the boys that attend X College must be! In some place or places a boy may be too young to travel alone, and in another place, or other places, he may not. No doubt the headmaster is acquainted with the peculiarities of his boys, and it is benevolent of him to take them thus into account and allow for them. "For all boys who return late on the ground of health, a certificate, signed by a Doctor, will be requisite." To a certain extent this clears up our uncertainty. It appears that the ground of health is the place at which one of the two is too young to travel alone, and yet it is hard to say. The ground of health may be the reason why boys return late, so that it seems they may return late either on the ground of health or on the ground of illness. In any case, it does not appear that the boys who return late on the ground of illness require a certificate signed by a Doctor, but only those who return late on the ground of health. What unblushing effrontery the young rascals must exhibit!

"The Principal hopes that no application for extension of holiday may be made to him or to any House Master which is not covered by this notice." We have heard much of the hauteur and arrogance with which the headmasters of public schools treat their assistant masters, but we have never before known a headmaster to depersonalise his assistants, and speak of them as if they were things, mere chattels, as this Principal does. It seems, too, that he covers his House Masters with notices! Tyrannical despot!

"ALL NEW BOYS without exception are required to be in the College a day before the old boys return. Boarders must be at their Houses before 6 p.m. on Tuesday, May 1st." This would be clear enough if only we knew on what day the old boys return. As at present advised, it seems that it may be Tuesday, Wednesday, or Thursday; and suppose some of the old boys return on one of these days and some on another, how are all new boys to regulate the time of their arrival? It seems that the only safe course would be to arrive on Monday, but this would not really be safe, for the holidays end on Wednesday, and some of the boys need not arrive till Thursday, and the miserable boys will be held "*personally responsible* for their punctual return," so that if they return either too late or too early they will be punished. I hesitate to attribute any such diabolical design to any headmaster, but it does seem as if the Principal were determined to catch the boys one way or the other, and by hook or by crook make them punishable.

"Day boys must present themselves at the College at 9 a.m. on Wednesday, May 2nd, or (see below) on the previous evening." Boarders are to be in their Houses on Tuesday, and day boys must present themselves on Wednesday, and all boys (whether previously at the College or not) must be in school on Thursday.

It seems, therefore, that either there is a third unmentioned class of boys, who are neither boarders nor day boys, or else it is necessary to warn the boys, both boarders and day boys, not to play truant on the very first day that studies begin—viz., Thursday. Surely discipline must be lax if such a contingency is contemplated. The boarders are to be in their Houses, the day boys are to present themselves (but to whom? and where?), and all boys, whether previously at the College or not, are to be in school at 8.50 a.m. on Thursday. Let us hope there are sign-posts to direct the new boys from the College to the School, or they may not be punctual, and then they will be held *personally responsible*. The situation seems to be a curious one. If all the boys are to be in the School, what is the use of the College?

"All new boys, whether previously examined or not, will be examined, for the purpose of determining the Form which they will enter, on Wednesday, May 2nd, from 9 to 12.15 and from 2 to 4.15."

"For those who do Greek or German, papers will be set on Tuesday evening from 7 to 8.30." But what is to become of the unhappy boys who "do" Greek or German, and understand that they need not be in School (or College) till Wednesday or Thursday?

These regulations were sent to me by a despairing parent who wished to conform with them, but could not understand how they applied to his son. I studied them long and carefully, and was quite unable to advise him. If they had been drawn up by any one but an accomplished classical scholar, I should have said that they showed the grossest ignorance of the construction of English, and that the man who drew them up ought to go to school—preferably a Board School—for elementary instruction in his own language; but, as they are concocted by an accomplished classical scholar, M.A. of Oxford and Scholar of his College, and as a knowledge of Latin and Greek gives to the fortunate possessor a perfect command over English, a command that cannot be acquired in any other way, I am compelled to conclude that the regulations have been drawn with the intention of puzzling and confusing the reader, and of securing the certainty that they cannot all be complied with. The only discernible motive for rendering the regulations unintelligible is indicated by the words *personally responsible*. What an orgy of birching must usher in the term! and how the Principal must chuckle and enjoy himself!

FURTHER NOTES ON CAPTIVE SPIDERS (Theodore Savory, Exhibitioner of St. John's College, Cambridge).

SEVERAL points of certain interest in the economy of spiders have arisen since the publication of my paper on *Tegenaria atrica*, Clerck, in SCIENCE PROGRESS for October 1916.

One of the most fascinating occurred with a pair of *Agelena labyrinthica*, Clerck, which—in the face of popular opinion of all spiders—were living together peaceably enough. I admitted a fly to the cage, and the female, much the bolder and more active, caught it by the leg. The male now rushed up, and in the mêlée which followed, the fly escaped. Soon, however, the female caught the fly again, and I, hoping to prevent a competition which might end fatally, put in a second fly to attract the attention of the male. But not he! A bird in the hand being worth more than one in the bush, he approached the female, who had the fly by its thorax, and buried his chelicerae into its abdomen. Thus they remained

imperturbable, the two spiders sharing the one fly, for two hours. Arachnological conjugal bliss!

There is, in fact, more in the common occurrence of a spider's eating a fly than might appear. Fabre, in his description of the Labyrinth Spider, states that when he fed it with Locusts, the spider sucked the "haunches" of its prey, and left untouched the bulk of its body. It has been, therefore, the more interesting to observe the House Spider, *T. atrica*, after throwing away the dry exoskeleton, and after brushing its cheliceræ with its palpi in the way I have described, after this, to observe it pick up severally with its palpi the detached legs that were lying in the tubular part of its web, and to suck them, as if they were, as Fabre suggests, a tit-bit, a dessert after the solid meal.

The rejected carcase is nearly always dropped into the corner of the cage farthest from where the spider's habitat, the tube, is spun. Mature females, especially, seem to be particular in this respect, and I have seen a little heap of corpses grow in one corner, while the rest of the cage was quite clean. It is the same with the fæces; 80 per cent. of these are dropped right into the far corners and another 15 per cent. or so quite close to the sides, the middle spaces of the floor of the cage and the region near the tube being comparatively clear.

Not so with the mature male spiders, which, naturally perhaps in their more peripatetic character, do not take so kindly to captivity. Before the final moult, their behaviour is fair: thereafter, no silk is added to their webs, which rapidly fall into ruin, and their cages become generally untidy. A mature male put into a new cage makes either a very poor attempt at a web or none at all.

One is tempted to include in a third category, the character of the very young spiders. These little creatures require practically a gas-tight cage to meet successfully their migratory instincts and their ability to squeeze themselves through the smallest of cracks. Those that one is able to retain, however, are worth any trouble spent on their cages. The delicate little webs that they spin are almost invisible, yet they will entrap small *Culicidæ* and such insects, which the spiderling, "as brave as a stoat," attacks vigorously. The gnat may be three times as big as the spider, but the latter clings to it so firmly with its chelicerae as to allow itself for a moment to be carried bodily round the cage. It is often a problem to find food for these small spiders, a problem which is only partially solved by the use of the young from the succeeding cocoon; but I have known them to survive the winter on no more than a few drops of water. They are also extremely partial to a drop of beer!

One may note in passing that the form of the cage employed has considerable influence on the behaviour of the spider. A *Tegenaria* in a plain rectagonal box is not one tithe so comfortable as one in a box with a little shed or shelter in one corner. This corner, slightly the darkest, it employs for the tube of its web. For other genera, it seems to be almost essential to make the general lines of the interior at least slightly resembling the surroundings in which the spider naturally lives. And it is only right, if the habits of the captive spider are to be recorded and assumed similar to those of the free specimens, that this should be done.

A note on the peculiarities of a captive *Pisaura mirabilis*, Clerck, appeared in *The Field* of July last year. This was the first of the *Citigradæ* that I had succeeded in persuading to live in captivity, and it certainly repaid my trouble. Its most interesting feature was its obvious power of vision, compared with that of spiders of sedentary nature, and again and again I have seen it make a sudden dart on to a fly that walked within some inches in front of it. In fact, it seldom missed anything that once it caught sight of, and although it laid down a number

of lines of silk which sometimes helped it to catch its food, its skill as a retiarius was not great. I have seen it take perfectly in its "jaws" a fly flying at some speed towards it, as prettily as a cricketer making a slip catch. Another point of interest was its great voracity. It would suck absolutely dry three or four bluebottles (*Call. erythrocephala*) a day and every day, while a large *Tegenaria parietina*, Fourc., would not eat more than a quarter of that amount.

This *Pisaura*, with several others, practically confirmed my suspicion that a spider tends to bite a captive insect in the most vital part, the thorax. As often as not, the spider at its first rush secures the leg of its victim, and it invariably follows this up by biting the thorax. I have also seen a spider which had pounced upon a *C. erythrocephala* definitely turn the struggling insect over and bite it in the thorax from beneath. And a House Spider that I saw tackle a large *Pronuba*, or Yellow Underwing, approached the moth from behind and contented itself with putting its claws on to its wings, until it was in a position to seize it by the thorax. I must mention, however, that I was entirely at fault in comparing this action to that embodied in Dr. A. H. Cooke's investigation on the mussel. Dr. Cooke's results showed precisely the opposite, viz., that *Murex* tends to strike the mussel practically anywhere.

With regard to the action of spiders when thrown on the surface of water, a more extended series of experiments shows the following results:

i. Vagabond spiders can run on water as easily as on land.
ii. Sedentary spiders unaccustomed to run, struggle hopelessly and make no progress.
iii. Sedentary spiders accustomed to run on their webs are either light enough to run off, or else can "swim" off like *Tegenaria atrica*.

A curious action of male spiders, and I believe of male spiders only, is a sharp twitching of the abdomen, or jerking it down on to the web. No silk can be seen to be secreted, and a male spider on the web of a female does it whenever it stops moving, but never more than once at a time. This habit was recorded by F. M. Campbell (*Jour. Linn. Soc.* xvii. 1883), who describes it as a sign of impatience. I cannot, however, help thinking that it must possess some physiological rather than psychological significance. I open admittedly a controversial subject, and one in which each is entitled to his own opinion, for what it is worth, when I express my belief that no spider ever felt impatient. It is hardly probable that, even supposing such a mental state possible, an animal whose life was so largely spent in waiting for the chance blundering of an insect into its web could retain such a characteristic, nor do I believe that a spider could become impatient any more than it could feel delighted or love-sick. We cannot, however, by any means get inside the spider's mind, and the final court of appea must remain, as with Aristotle of old, the judgment of the educated man.

SOME HISTORICAL REFLECTIONS ON CANCER (Harold P. Cooke, M.A., University, Durham).

IT will probably, I think, be allowed that ancient history and ancient literature may throw some light upon modern disease.[1] While I cannot, unfortunately, pretend to more intimate knowledge of the ills of the flesh than the generality of men are the heirs to, I desire in these papers to put forward some reflections on the subject of cancer.

[1] I may, perhaps, refer more especially to my friend Mr. W. H. S. Jones's little volume on *Malaria: A Neglected Factor in the History of Greece and Rome*.

I propose, first of all, to remark on two kindred and coherent points, calling ancient Greek writers to witness. It has, I believe, been suggested that cancer may be caused by the eating of meat, and, secondly, that the disease is most prevalent in the most wooded districts of England, and may, therefore, be due in some manner to the presence and prevalence of timber. My friend Sir Clifford Allbutt writes that cancer was familiar to the Greek physicians in the period with which I am dealing.

Now, Plato in *Republic*, Book II., refers to the question of diet. He speaks of a primitive diet and then of a richer or luxurious. The latter may be called vegetarian (with one most important exception) and exhibits a wonderful contrast with that of the Homeric poems. But what is the exception referred to? Swineherds, according to Plato, will be found in the luxurious State (*πόλις τρυφῶσα* or *φλεγμαίνουσα*), "and we shall need the other kinds of cattle even—very many of them—if any one will eat them."[1] In the simpler Platonic State or the so-called "City of pigs" oxen and cattle are mentioned only for ploughing, for purposes of draught, for weaving, and also for cobbling. Now Plato, as I think, in this passage must be taken as having in mind the diet that at least in its salient items was prevalent in Athens and Attica—if not, indeed, Hellas in general. The very casual and haphazard manner, in which he enumerates foods and interposes other luxuries also, is nothing against this conclusion, and is clearly explained by the demands of dramatic and colloquial reality.

Another passage will merit attention. In the famous Atlantis myth (*Critias*, 211) Plato describes the Attica of his day as still unrivalled in respect of the suitableness of its pastures for all kinds of living things (*τοῖς ζῴοις πᾶσιν εὔβοτον*), but goes on to betray the fact that "some of the mountains now only afford food for bees," while 9,000 years before "there were many other high trees cultivated by man and providing abundance of food for cattle" (*νομὴν βοσκήμασιν ἀμήχανον*). As dramatic reality demands that the first of these statements be understood literally, we are naturally led on to conjecture that, if the diet of Athens and Attica was in the main vegetarian in character, so also *a fortiori* was that of the other Greek States. Less pasture or less suitable pasture would probably imply fewer flocks. Nor do I know of any *positive* evidence that, generally speaking, the different Greek peoples had divergent tastes in this matter of meat.

Now, bearing these reflections in mind, let us turn to Aristotle for a moment. We find in Book I. of the *Politics* a very significant remark in his passage on the sources of food. The laziest people, he says, are nomadic. The tame animals supply them with food without effort or labour on their part. But seeing their cattle are compelled to wander from place to place for their pasture, they must needs go along with them too, cultivating a sort of live farm.[2] This indicates

[1] *Republic*, II., 373c: *δεήσει δὲ καὶ τῶν ἄλλων βοσκημάτων παμπόλλων, εἴ τις αὐτὰ ἔδεται.* Notice *ἔδεται*—the future. It appears to express a suspicion that none will be found to eat them, however luxurious or varied the diet.

[2] Book I. c. 8. Jowett (Davis's edition) has mistranslated the text; "the laziest," he says, "are shepherds, who lead an idle life, and get their subsistence without trouble from tame animals; their flocks having to wander from place to place in search of pasture, they are compelled to follow them, cultivating a sort of living farm." Welldon more correctly translates: "Thus the most indolent lead a nomad life because such food as the domestic animals supply is obtainable without trouble or effort, and as their cattle are obliged to change their quarters for pasturage, they themselves must needs go with them, so that they carry on a sort of husbandry of live stock." The Greek is *τῶν ἡμέρων ζῴων*—note here the definite article. Aristotle makes clear later on that he wishes to set out in this passage a pretty full list of the classes, who obtain their food for themselves and

clearly, I fancy, the shortage of pasture in Greece, and, therefore, of meat or potential meat. Somewhat later Aristotle comments on those species of fishes or fowls, from which human needs are supplied; and the first human need is of food. I am aware that Aristotle also tells us in this Chapter II. of Book I. that a statesman or householder should have an acquaintance with the most profitable kinds of property in horses, cattle, sheep, and so on. But this passage does not bear exclusively, if at all, on the subject of food, as is evident from the reference to ἵππων and ὁμοίως δὲ καὶ τῶν λοιπῶν ζῴων. Nor do all these various animals thrive in the same place, he says. Moreover, whatever their uses, they are described in the variant readings as being either κτήνη or κτήματα, and are from their context distinguished from what Aristotle himself terms γεωργία. In truth, he goes on to remark, as we saw, that *in the next place* the statesman or householder must be practically acquainted with farming, both agriculture and tree cultivation, and also with the tending of bees and with all other winged or swimming creatures, so far as they supply human needs.[1]

Let us further observe that Aristotle cites with approval Hesiod's *Works and Days*, l. 405—

οἶκον μὲν πρώτιστα γυναῖκά τε βοῦν τ' ἀροτῆρα,

"First house and wife and an ox for the ploughing withal." And the reason he gives is significant—ὁ γὰρ βοῦς ἀντ' οἰκέτου τοῖς πένησιν ἐστίν—the ox is the poor man's slave, that is, his instrument, not his food.[2]

We are, I think, led to conclude (1) that fish and birds were consumed, albeit Plato's silence about them would suggest they were not staple items; (2) that meat in the sense of beef and mutton was not a common article of diet.[3] These conclusions are, I think, in accordance with what we might infer *prima facie* or may read in another connection. *Prima facie* we are led to conjecture from Athens' geographical position that there at least fish would be eaten; and we know that in more primitive times "Fish was caught plentifully on the coast, and formed the staple article of diet. Meat was rarely eaten."[4] Whether fish in the days of Aristotle was to any great extent still consumed is immaterial to my immediate purpose, and we need not stay here to inquire. There are, moreover, collateral reasons—they will shortly appear in some measure—for assuming that the soil of Greece was but poorly adapted to flocks and herds. And its unsuitability to pasture became each year ever the greater.

It may in this connection be objected that Plato in *Republic*, Book III., speaks of roast, as opposed to boiled, meat as being the most convenient diet for soldiers.[5] But a reference to this passage will show that Plato is citing the custom in Homer,

not by exchange nor by retail trade (οἱ μὲν οὖν βίοι τοσοῦτοι σχεδόν εἰσιν, κ.τ.λ.). But shepherds, neatherds, and swineherds are not specifically mentioned and must all be included, if all are included, in the laziest class.

[1] Jowett again mistranslates: "the keeping of bees and of fish or fowl or of any animals which may be useful to man." This rendering is wrong in two ways. It is not "the *keeping* of fish or fowl"—the words understood with each genitive are clearly δεῖ ἔμπειρον εἶναι. Nor is it "or of any animals"—the Greek words are very precise, τῶν ἄλλων ζῴων τῶν πλωτῶν ἢ πτηνῶν, ἀφ' ὅσων ἔστι τυγχάνειν βοηθείας. Βοηθεία is wider than τροφή; see the phrase in Bk. I. c. 8, τῆς τροφῆς καὶ ἄλλης βοηθείας ἕνεκεν.

[2] *Politics*, Bk. I. c. 2.

[3] Plato refers to the breeding of birds, but always of cocks or of quails for fighting (*Rep.* V. 459 *a*; *Laws*, VII. 789 *b*; *Lysis*, 212 *d*; *I Alcib.* 120 *a*).

[4] Grant, *Greece in the Age of Pericles*, p. 70.

[5] 404 *d*, μάλιστ' εὔπορα.

whose soldiers, he says, have no fish, though they live by the shores of the Hellespont, and are not allowed any boiled meats but only roast, which are most convenient, as not involving the carrying about with the army of pots and pans. If the passage be not wholly ironical, as the quoting of Homer suggests, Plato would appear to approve of this diet as the best or, at least, good and simple for soldiers under military training; but the passage proves nothing whatever with regard to the Greece of his day, and the omission of these meats in the passage, in which he builds up and "fodders" his State, becomes the more curious and striking, for his citizens are to be soldiers. Again, in primitive times, we are told in a passage of Book VI. of the *Laws*, animals were not even tasted, as, for instance, the flesh of the cow, but only cakes and fruits dipped in honey.[1] I do not, however, conclude that in Classical Platonic times men ate daily of the flesh of the cow. Nor can anything, I think, be inferred from such passages as that in the *Republic* about Polydamas, the pancratiast, and his eating τὰ βόεια κρέα or beef.[2] Indeed, we may fairly conclude that the diet of Platonic times contained little beef and mutton; of later times even less. The supply of such foods could, in truth, be recruited by importation; but we have, as I presume, neither evidence nor *prima facie* reason to suppose that such importation took place.

I come now to a kindred point. I understand my friend Mr. Maurice Thompson, who is probably the best living authority, to hold (1) that, while Greece in the Homeric poems would appear to be well-wooded and forested, by the beginning of the fourth century B.C. the problem of deforestation had become a very serious question; (2) that Attica, for instance, in Platonic times was not very far removed from what it is at the present moment in the matter of timber and trees.[3] I think there may have been more trees in Attica than perhaps Mr. Thompson suggests. Plato, for instance, mentions yews and myrtles in a passage already referred to; there was a plane-tree by the Ilissus.[4] But the process of deforestation was never to our knowledge arrested. And deforestation, Mr. Thompson points out, would involve and imply denudation.

So much, then, by way of evidence. What conclusion is to be drawn from these facts? That cancer was familiar in a country very largely denuded of trees, among a people that mainly subsisted on what we may call a vegetarian diet. The later the date we assign to the relevant Hippocratic writings, the more pronounced were the deforestation and the consequent shortage of pasture and herds. It would, therefore, appear from this evidence that grave suspicion, to say the least, has been thrown on the supposed universal connection of cancer either (1) with the prevalence of trees or (2) with the eating of mutton and beef.[5]

[1] 782*c*.

[2] Bk. I. 338*c*. Incidentally, it is interesting to note that Polydamas was a Thessalian; and Thessaly was regarded in the other Greek States as much on a level with Macedon—half-Hellenic but semi-barbarian.

[3] For details I refer the reader to his very instructive paper on "Deforestation in Ancient Greece" in the *Proceedings of the University of Durham Philosophical Society*, vol. v. part 2 (1912–13). He assumes what I try to show here on the subject of diet in Plato: "Flocks and herds are common in Homer, and Homeric food supplies are a striking contrast to the meagre vegetarian diet of Plato's

[4] *Rep*. II. 372*b*; *Phædrus*, 229*a*.

[5] I may add that I shall be greatly obliged if readers of SCIENCE PROGRESS can throw further light on the evidence given. It would also be instructive to know to what extent cancer is prevalent in Greece and especially in Attica to-day, and whether those olive-trees, for which Athens and Attica have ever been famous, exhibit an analogous disease.

ESSAY-REVIEWS

THEISM AND MODERN THOUGHT, by JOSHUA C. GREGORY, B.Sc., F.I.C.: on **The Idea of God in the Light of Recent Philosophy,** by A. SETH PRINGLE-PATTISON. The Gifford Lectures delivered in the University of Aberdeen, 1912–13. [Pp. xvi + 423.] (Clarendon Press, 1917. Price 12s. 6d. net.)

MAX MÜLLER traced the source of religion to impressions made on the human mind by natural phenomena and forces. For Durkheim religion has a social source—it originated in collective action, emotion, and thought. Since the "Idea of God" ultimately appears in religion, on any view of the latter's primary nature, these two thinkers represent a transference of emphasis in determining its origin. A similar transference of emphasis is apparent in attempts to justify the belief in Deity. The argument from design is typical of the first centre of emphasis, the argument from value is typical of the second. The argument from contrivance, as Mr. Balfour prefers to call it, did, and where it still finds favour continues to, consult human interests; but it can suppose the external universe to declare a purpose, as an ordinary machine declares a human intention, without inclining its line of emphasis to man as its centre. When the "Idea of God" is connected with the "Principal of Value," the existence, nature, and life of man becomes the determining factor in the discussion. Prof. Pringle-Pattison illustrates this shift of emphasis from nature to man by comparing Hume's *Dialogues Concerning Natural Religion* with Kant's subordination of "the starry heavens above" to "the moral law within." He contrasts the "contemplation of the works of nature" in the former ("and its exiguous result") with the "Kantian argument which rests the whole case on the intrinsic worth of moral personality." In abstract possibility this change of emphasis might simply result from the innate restlessness of the human mind—for thought, like dress, has its fashions. It might aim at a doubly sure assurance by making one argument support another. It might, again, be moved to the second centre of emphasis by dissatisfaction with the first. The last motive is ascribed by Prof. Pringle-Pattison. Dissatisfaction with "the vague residuum of theistic belief which is all that Hume considered deducible from the evidence" produced the passage from Hume to Kant and his successors. "Hume himself," Prof. Pringle-Pattison points out, suggests that religion did not first arise from the study of nature but from the concerns of human life with all their hopes and fears. Hume centred emphasis on nature in discussing the justifiableness of the notion of Deity; he transferred that emphasis to the being and nature of man in accounting for its origin. In trying to secure a justification for the "Idea of God" superior to the "exiguous result" of Hume, Kant and his successors were compelled to make the same transference of emphasis that Hume had made in accounting for its origin.

The same difficulty that induced Theism to transfer its centre of emphasis besets it when the transference is made. This difficulty runs all through the scientific, theological, and philosophical thought of to-day. Even the bare theistic

minimum—simple belief in a personal Deity—fits very badly into the present conceptual scheme. This failure to assort seems very real, whatever be its significance. The conceptual scheme of the totemistic Australian who classes himself with a kangaroo is obviously different from ours. A system of concepts that has changed once may change again—the present incompatibility between Theism and thought may be superseded by a harmony and the supersession may come to-morrow and remain for ever. Religion may root in a conviction that is essentially independent of our stumbling efforts to think things out. The past has taught us to be humble and to avoid a dogmatic obstinacy in opinion. But whatever the future may reveal, the present conflict between Theism and thought is evident. The work under discussion is the exposition of a master who believes in God and in the sound philosophy of his belief. These Gifford Lectures are noble in spirit, profound in conception, and able in execution; but they cannot conceal the failure of modern thought to place the "Idea of God" in organic connection with its conceptions and implications.

Chapter IV. is headed "The Liberating Influence of Biology." "Liberating" because biology has broken the spell cast over thought by "the mechanistic tradition," though that "is still strong, among the 'old guard' of physiologists." It is very commonly supposed, more particularly in popular discussion, that we have to choose between the horns of a sharply defined dilemma—if mechanism be inadequate or false, then religious, or at any rate theistic, belief is the only alternative. Prof. Pringle-Pattison knows that the problem is not so circumscribed, but he hardly seems aware that biology is even more dangerous to Theism than mechanism or naturism. The change of conception in passing from mechanistic interpretation to the notion of the organism corresponds to the transfer of emphasis already noted between Hume and Kant—not rigidly or in detail, but in principle. To these two centres of emphasis correspond two ideas or conceptions, between which the notion of Deity has constantly oscillated. "The Potter and the Clay" is typical of one of these notions—the notion of the Artificer. Hume noted that "Hesiod, and all the ancient mythologists . . . universally explained the origin of nature from animal birth . . .," and remarks, through Philo in the *Dialogues*, that if the universe more resembles an animal or vegetable than a work of human art it should have originated as the former originate and not through reason or design. This is the second notion—the notion of Generation. The argument from design connects with the notion of artificer. If the universe be an executed plan, then Theism is established, since the artificer remains when his work is performed. If there is no evidence of an artificer it is still possible to suppose that a God exists whose purposes are beyond our comprehension. The artificer notion, whether substantiated or not, is not fatal to Theism. But the notion of generation is more likely to be fatal. To transform the concept of artificer into the concept of generation, the artificer must become what he creates—the world, with the living creatures that inhabit it, replaces the being that brings it forth and, if it represent a God at all, represents a God who is dead. This is simply a first approach to the significance of regarding the universe under the category of organism. Theism depends on the principle that the superior existence precedes the inferior. If we gather the principles of organic growth from our experience of their operation we are forced to the opposite conclusion. Man proceeds from the amœba—the lower grows into the higher. If we decide to search out the origins of human institutions, of human conceptions (including religion or Theism itself), of language, of societies, we naturally look for something ruder, simpler, and lower in the scale of organisation than the final results we are explaining. "Idealism

takes its stand on the impossibility of explaining the higher from the lower," writes Prof. Pringle-Pattison. It is certain, explanation or no explanation, that the "Idea of God" will be very attenuated if the fundamental process of the universe be not from higher to lower levels. Idealism obviously "takes its stand" on a principle that flatly contradicts all experience. Prof. Pringle-Pattison might try to turn the flank of this argument by suggesting that God and man—to mention one item of the universe—are related as father to child. The father still exists when his child is born; the infant issues as an inferior from a superior; the development of the child appears as a growth from a lower to a higher stage. Such an argument, so far as experience is concerned, attaches more significance to an incident of the process than to the process as a whole. The primal fathers of the plain and forest have passed, through the children of thousands of generations, into the fathers of to-day. One set of habits, one set of conceptions, one set of practices and institutions, one set of languages have grown into others and merged in them. When experience is narrowed to a point it may discover a passage from higher to lower; whenever experience makes a sweep it finds the rivers rising higher than their source. It is possible that the process from lower to higher may be suddenly and permanently inverted at a point. The widest sweep our experience can make, however, has failed to reveal it. Mind has arisen from life and life, apparently, from the non-living. "Both life and self-consciousness," Prof. Pringle-Pattison admits, "appear to emerge from antecedent conditions in which these distinctive qualities cannot be detected"; and he adds that he cannot believe that abiogenesis has any philosophical significance. It has one very material bearing on the theistic issue. The growth of the living out of the non-living is, at any rate, very near to the utmost limit of the sweep of experience. That widest sweep still discovers the lower passing into the higher. When the idealist elects to stand on the "impossibility of explaining the higher from the lower," he stands on a speculative construction; experience gives no warrant for the choice. The theistic hypothesis must involve descent from high to low. Experience insists on the opposite description. The Theist should be able to describe some process of descent: in every region of experience, if that region have any range at all, the run must be described as up and not down.

"Philosophical criticism is simply the thinking out and setting in a clear light of the conceptions and methods which science actually employs." Science follows closely, though not by any means exclusively, the method of description derived from observation and experience. The survey of evolution discovers "continuity of process and the emergence of real differences"—this process constantly follows an upward path. Theism must suppose that the universe, including man, must be grounded as an inferior in the superiority of God. The contradiction between this supposition and the process as experience constrains us to describe it is a simple matter of fact. It remains true even if "the autonomy of life" supersedes physical and chemical forces. Contrasting organisms as "self-maintaining individuals" with the operations of physics as "only a continuous transmutation of energy" leaves the same fact untouched. Interpreting the facts does not alter them. "Interpreting the more developed by the less developed" may be "logically tantamount to a reduction of the more to the less"; "The world without conscious subject" may be "a world waiting for its meaning"; Theism's speculative reference of the universal process to a source higher than itself contrasts sharply, in spite of these asseverations, with Bosanquet's descriptive résumé that "in apparent cosmic development, whether inorganic, organic, or logical, the rule is for the stream to rise higher than its source."

Prof. Pringle-Pattison prefers to think that "the whole process wears the appearance of a progressive revelation," and that novelties pour from "the inexhaustible nature of the fountain from which we draw." It is possible, abstractly, that life might be an extra dole by the Dispenser, or by the Dispensing Agency, consciousness another dole, and so on in succession. In any case the problem of accounting for the doles remains. "Creative Evolution," the higher springing creatively from the lower, represents creation by instalments. "Progressive revelation," or novelties doled out from a prior store, represents an original creation in the lump. Experience suggests the instalment method—events suggest that they follow it. If novelty or creation appear at one point, it is reasonable to expect it at others. The mystery of creation distributed through development is no less a mystery when collected at the source, and the substitution rejects our perceptions for a speculative interpretation. "How can anything come into being unless it is founded in the nature of things—that is, unless it eternally is?" asks Prof. Pringle-Pattison. To be quite candid we do not know, but, while the human mind certainly appeared at some point in the world's history, there is no indication that it "eternally is." This notion that what comes out must have been in shirks the category of organism. To suppose that the amœba was really a man or contained one is to revert to mechanism—supposing growth to resemble the principle that for all energy taken out an equivalent quantity must be put in.

Prof. Pringle-Pattison tries to evade this issue by a line of argument which he thus sums: "Questions of the apparent historical genesis of the higher or more complex from the lower or simpler have no philosophical importance or relevance, seeing that, philosophically considered, the lower or simpler phases are not independent facts existing as *prius*, but abstract aspects of a single fact, which is fully expressible only in terms of self-conscious experience." Now the earth *seems* to us to have existed before man appeared on it. The above quotation informs us that the earth was no *prius*, for it existed only when men could think about it—we dwell, in short, in a world of illusion. This is a singular conclusion to discover in a writer who insists that man's perception and knowledge "put him in touch with reality," regards primary and secondary and even "tertiary" (such as beauty) qualities as realities and not as phenomenal mediations, and declares of each creature that "what it apprehends, up to the limit of its capacity, is a *true* account of its environment, so far as it goes." If we can trust our powers of apprehension at all we must believe that a past condition existed before the present, and that a future condition, not yet existent, is due. An example of the confusion imported into this simple principle by the idealistically minded philosopher occurs in the further statement that "it would be more consonant with the structure of consciousness . . . to place it in the future," if it became necessary "to choose between placing the *vis directrix* in the past or in the future." He is referring, of course, to the anticipatory character of the mind—to the *ends* that control its operations. There is no future to contain the *vis directrix*; our purposes refer to what will or may be, but they occur and act in the present. An attempt is made to secure some point in the future to which the *vis directrix* can be hitched by "explaining the evolving subject not only by what it has been, but, still more vitally, by what it is not yet, but is on its way to become." This is the "Ideal or the end realised in the process," and thus "we may be said to supplement the causality of the past by the causality of the future." This hints at a phenomenon even more remarkable than the standing puzzle of creation out of nothing, for causation by the future is creation *by* the

non-existent. The whole argument confuses *end* in the sense of an event that ultimately happens with *end* in the sense of a purpose that is always present (or past).

This attempt to mingle the future with the past is part of a general mental habit, evident throughout the book, of crushing together, or unduly identifying, things that are separate, because they have connection as well as independency. This mental habit is closely connected, as effect or cause, with the writer's conception of philosophy as something that "sees life steadily and sees it whole." "It is the process as a whole that has to be considered," he writes, and, again, "the process of the universe . . . must be taken as a whole, *in which the spirit of the whole is everywhere present.*" Regard wholes by all means, but do not forget that the distant mass of foliage conceals a number of distinct trees! "The spirit of the whole is everywhere present" clearly expresses this mystical habit of blending different things together in a sort of pseudo-identity. Surely there is one spirit among men, another spirit among beavers, and yet another in the fires of the sun. Theories of perception of the subjective or representationist type are condemned because they mystify by "extruding man from the world he seeks to know." "The fact of man's rootedness in nature," and "man as organic to the world," are so emphasised that perception becomes virtually a mixing of man with the objects he perceives. Perception and knowledge surely imply both a connection between man and nature and an element of independent existence, which is the element recognised in the misnamed "extrusion." So thorough is the mixing that beauty and sublimity become "qualities of the object just as much as the vaunted primary qualities." Feeling and emotion are so essential a part of beauty and sublimity that pleasure (and pain by implication) are torn by this conception from their natural habitat in consciousness and distributed throughout external things. The same line of thought inspires the statement that "man does not make values any more than he makes reality." This touches a central point of Prof. Pringle-Pattison's philosophy. It is not easy to gather into a whole his own "Idea of God" scattered through the present "construction through criticism"; but he is emphatic "that the idea of value is central and decisive throughout. It is, at bottom, the question of the divineness or undivineness of the universe."

Mr. Balfour's Theism and Humanism is a recent expression of the present dominant tendency, discerned and approved by the Professor, to justify the Idea of God through the Idea of Value. Now all ideas of value, be the values great or small, represent human valuations. A minor value is most convenient to take to illustrate the whole—as Euclid argues from the triangle A B C to all triangles. If we take the greater values there is a danger, as often happens to Prof. Pringle-Pattison and his confrères, of a mental excitement that surrounds the argument with a cloud of rhetoric. A cabbage has an edible or nutritive value because there are men to eat it. It would have no edible value if there were no men, or if men did not eat. This edible value is not something that "eternally is," or "the spirit of the whole" expressing itself in the cabbage. It is simply a value that appears when cabbages grow and man requires to eat. It is quite unnecessary to look beyond the physiological and appetitive nature of man, in combination with the material of the cabbage, as far as this particular value is concerned. The antecedents are quite competent to discover to us why we value cabbages as edible. The edible value of cabbage arrived with us; if we were to disappear it would disappear too. Values, in short, from the value of the humble cabbage to the loftiest æsthetic or moral significance, depend for their existence upon us. They grow out of our life, or from the interaction between us and other existences. It

is quite natural that values, particularly the highest values, should receive special consideration from thought. They are, in a special sense, affairs that concern us—very largely because they are man's own private concern or creation ; they also derive a special gusto of reality from their effective and emotional character which seems to place them outside us. They absorb naturally, almost inevitably, the notion of Divinity. The general impression remaining after Prof. Pringle-Pattison's discussion of modern views of the "Idea of God" is that value is generally regarded as something that is somehow Divine. It is not easy, as before remarked, to gather completely his own "Idea of God." He does say definitely that "the traditional idea of God must be profoundly transformed," and modern thought certainly seems to be acting on this maxim. The total impression remaining from the discussion is that the various thinkers who operate with the Idea of God and the Idea of Value continue to apply the term "God," when they do not prefer the term "Absolute," to very considerably transmuted versions of the usual meaning of the word. One suspects that the Idea of Value is leaving an even more "vague residuum of theistic belief" than the old argument from design. Modern thought is unable to fit Theism satisfactorily into its conceptual scheme either by means of the category of organism or through the Principle of Value.

THE ORIGIN OF THE OLD RED SANDSTONE, by G. W. TYRRELL, A.R.C.Sc., F.G.S.: on

Dominantly Fluviatile Origin under Seasonal Rainfall of the Old Red Sandstone, and **Influence of Silurian-Devonian Climates on the Rise of Air-Breathing Vertebrates**, by JOSEPH BARRELL (Bull. Geol. Soc. Amer., 1916, **27**, 345–86, 387–436).

The Geology of Caithness, by C. B. CRAMPTON and R. G. CARRUTHERS, with contributions by J. HORNE, B. N. PEACH, J. S. FLETT, and E. M. ANDERSON. [Pp. 194, 30 figures, 7 plates.] (Mem. Geol. Surv. Scotland, 1914.)

THE interpretation of the physical geography, conditions of deposition, and climate of past geological periods by means of the study of the sediments that build up the geological record is a fascinating branch of geology which has recently been taken up with enthusiasm on both sides of the Atlantic. In Britain, however, dominated as we are by "our heritage the sea," and deprived by the smallness of our isles of the opportunity of studying continental deposition on the large scale, geologists have been inclined to exaggerate the importance of marine deposition, and to minimise the extent to which deposits of continental origin, in deserts, river flood-plains, and lakes, have entered into the composition of the British geological column. American, Australian, Indian, and South African geologists, on the other hand, with areas of continental dimensions to range over, and a great variety of climates and conditions of deposition to study, have generally been led to a truer view of the relative importance of continental deposition as contrasted with marine. Along with this has come the recognition that fluviatile sedimentation over the great flood-plains is the dominant mode of continental deposition: and several formations (*e.g.* the Tertiary of Western America), which were formerly thought to have been accumulated in great lakes, are now known to have been deposited on river flood-plains. A similar movement of geological thought has led to the recognition of delta deposition in many

formations which formerly were vaguely said to be of "estuarine" or "brackish water" origin.

It has been left to an acute American geologist, Prof. Joseph Barrell of Yale University, to extend these views in great detail to a British formation. The Old Red Sandstone, first thought by Hugh Miller to be of marine origin, and still so regarded by P. Macnair, has long been attributed by the majority of geologists to deposition in great freshwater lakes. This view, originated by Fleming and Godwin-Austen, was adopted and developed by Sir A. Geikie, and owes its great vogue to his authoritative advocacy. Prof. Barrell now interprets the Old Red Sandstone as a typical continental deposit mainly of fluviatile origin, arising in a region of seasonal rainfall in which alternations of semi-arid and rainy conditions were the rule, with probably longer periods of drought than of humidity. While this is the first detailed study of the Old Red Sandstone from this point of view, several British investigators have already reached the same standpoint, especially Dr. C. B. Crampton and Mr. R. G. Carruthers in the new Caithness Memoir of the Scottish Geological Survey (1914), which Prof. Barrell has evidently not seen. These authors clearly recognise the continental nature of the Old Red Sandstone and the fluviatile origin of many of its sediments, but they have obviously been hampered in accepting the full consequences of this view by their manifest reluctance to discard the Lake Orcadie of Sir A. Geikie. In other recent Scottish Geological Survey memoirs Mr. E. B. Bailey and H. B. Maufe have approached the theory of fluviatile origin, and Prof. J. W. Gregory in his *Geology of To-day* (1915, p. 204) has ascribed the Old Red Sandstone to conditions similar to those which have produced the widespread sheets of gravel known in New Zealand as "shingle rivers." The late J. G. Goodchild in 1904 came very close to the fluviatile view, as is indeed acknowledged by Barrell. He regarded the Old Red Sandstone as accumulated under continental conditions, partly in large inland lakes, partly as torrential deposits of various kinds, partly as desert sands, and partly as the results of extensive volcanic action. The fluviatile deposition advocated by Prof. Barrell is intermediate between torrential and lacustrine, yet quite distinct from either, and may include subordinate deposits of torrential, lacustrine, or desert origin in different parts of the basin of accumulation. Dr. R. Campbell, in a recent account of the geology of south-eastern Kincardineshire (*Trans. Roy. Soc. Edinburgh*, 1913, **48**, 954), recognises the torrential origin of the great conglomerates of the Lower Old Red Sandstone, but regards the finer conglomerates, sandstones, and shales as having been accumulated in a large, shallow, freshwater lake, or chain of lakes.

The criteria relied upon by Barrell for the establishment of the theory of fluviatile origin are chiefly the great extent, thickness, and coarseness of the conglomerates, and the great vertical and areal distribution in the finer sediments of marks of exposure to the air; but there are numerous supporting lines of evidence. He recognises the inadequacy of certain characters, such as ripple-marks, cross-bedding, absence of fossils, and presence of red or variegated beds, when taken by themselves, as criteria of origin, since all of these may occur under a variety of conditions. It is the concurrence and quantitative development of certain lithological and structural features, together with stratigraphical and palæontological characters, which are to be regarded as diagnostic of certain modes of origin.

The thick and extensive gravel deposits of the present day are being laid down at the foot of mountain ranges by torrents, and on the upper parts of great river flood-plains. No thick marine gravels are known, and the thickness of coarse-

marine shingle seems to have an upper limit of 100 ft. (H. E. Gregory, "The Formation and Distribution of Fluviatile and Marine Gravels," *Amer. Jour. Sci.*, 1915, **39**, 487–508). These facts are well supported by the evidence of unquestionably marine conglomerates in the geological record, which seldom exceed 100 ft. in thickness. On the other hand, gravels consisting of well-worn boulders and pebbles are now accumulating to depths of over a thousand feet along the Himalayan foothills, as well as along other mountain ranges, and are closely comparable in thickness and character to the Old Red Sandstone conglomerates. Stream-laid gravels may be carried far and wide over a flood-plain, and thus form very extensive deposits; but subaqueous gravels, marine or lacustrine, are of very limited lateral extent, since the currents and waves are unable to carry coarse material far from the shore, and tend to deposit it in layers parallel to the coast. Hence thick and extensive conglomerates are far more likely to be of fluviatile than of marine or lacustrine origin.

On river flood-plains, which may be desiccated in the dry season, marks of exposure to the atmosphere are abundant, especially mud-cracks (sun-cracks, desiccation-cracks). Not only may these marks have a wide areal distribution, but the conditions of their formation may persist throughout the deposition of a great thickness of sediment. Within tidemarks, however, the area upon which they may be developed is extremely limited, and being swept by tides twice a day, the chances against the preservation of the marks are very great. If some happy chance ensures their preservation, exposure-marks will then be confined to thin beds on a continually rising horizon, and will be of restricted areal extent. The seasonal oscillation of the level of a lake may provide better and longer exposed mud-surfaces for the development of exposure-marks, but these marks cannot be continued through any great thickness of strata. It is clear, therefore, that a widespread areal and vertical distribution of marks of subaerial exposure in a series of strata is evidence of their fluviatile origin.

All accounts of the Lower Old Red Sandstone in the typical area of "Lake Caledonia" agree in the description of huge masses of conglomerate consisting mainly of water-worn pebbles of quartzite and vein quartz derived from the adjacent Highland Schists. Macnair says (*Geology and Scenery of the Grampians*, 1908, vol. ii. p. 2) they extend from Stonehaven on the north-east to Arran and Kintyre on the south-west of Scotland, and gives the thicknesses of various beds as 300, 3,300, and 1,000 ft. on the north-western side of the main syncline, and 1,100 ft. on the opposite side. Campbell (*op. cit.*) gives 6,900 ft. as the thickness of the Dunnottar conglomerate at the base of the Lower Old Red Sandstone of Kincardineshire. Conglomerates of this enormous thickness and extent can only have been deposited under fluviatile conditions along the fronts of great mountain ranges, as is indeed recognised by Campbell.

The Middle or Orcadian Old Red Sandstone contains no great conglomerate deposits. It is characterised by a great flagstone series containing numerous fish remains, with arkoses and mudstones of subaerial origin at the base, and a thick sandstone formation (John o' Groat's Sandstone) at the top. The flagstone group, according to Prof. Barrell, was deposited on the lowest portions of river flood-plains, where more or less continuous swamp conditions prevented the aeration and desiccation which gave rise in earlier times to red sediments. These conditions favoured the preservation of organic matter and the maintenance of the iron in a low state of oxidation, giving dominantly grey, green, or blue colours to the sediments. Hence the flagstone series appears to represent the consolidated sands, silts, and muds of river alluvia; and this

interpretation is favoured by the marked sedimentary rhythm which is described in the series by Crampton and Carruthers. The views of these authors may be illustrated by a quotation (Caithness Memoir, p. 103): "Lake Orcadie may thus be pictured as one of several sheets of very shallow water varying in number, position, and communication with one another, with a permanent drainage system depending on the direction of crustal warping and the age of the accumulations in the basin.

"With semi-arid surroundings and a warm climate, its waters supported abundant plant and animal life, and were subject to extensive advances and retreats from variations in the water-level due to silting and periodic seasonal changes, or from bodily migrations induced by warping of the basin of deposit. At one time extensive lakes, at another endless mud-flats and stagnant lagoons. At first, semi-desert shallow valleys and half-buried mountain peaks with fans and flood plains of ephemeral streams; later, extensive lakes and alluvial deposits; and, at last, a wind-swept plain of sand and wandering water-courses."

This view is nearly indistinguishable from that formulated by Prof. Barrell, of flood-plain conditions which, however, do not exclude subordinate marginal semi-desert phases and impersistent lacustrine intercalations; but it almost entirely disposes of the conception of great permanent lakes such as those postulated by Sir A. Geikie. Nevertheless, in the discussion preceding this statement there is much insistence upon sedimentation in Lake Orcadie; but the reader is often left in doubt in the case of a particular formation, whether it is considered due to flood-plain deposition marginal to the lake, or to deposition in the lake itself. In fact, no clear distinction is made between the fluviatile and lacustrine phases of deposition.

The palæontological evidence is no less favourable to the hypothesis of fluviatile origin of the Old Red Sandstone than the lithological and structural evidence. The fossils consist of fish, eurypterids, crustacea (*Estheria*), and plants, a biota decidedly more continental than marine. The presence of *Estheria membranacea* is especially important, as its fossil as well as its present-day associations stamp the landlocked and freshwater habitat of this organism.

The absence of unequivocally marine fossils throughout a great series of strata creates a presumption against, although it does not entirely negative, its marine origin; but when this negative evidence is reinforced by the presence of tremendously thick, widespread conglomerates, and by the prevalence of marks of subaerial exposure throughout a great vertical thickness and areal extent of strata, the presumption of continental origin becomes irresistible. The theory of marine origin of the Old Red Sandstone has been upheld on the ground that fishes of the same genera have been found in the marine Upper Silurian and Devonian as well as in the Old Red Sandstone; but this does no more than emphasise the fact that fishes (also, probably, eurypterids and *Estheria*) may adapt themselves to either freshwater or marine habitats, and may thus be entombed in the respective deposits. It has also been held that the interdigitation of beds carrying marine fossils in the Russian Old Red Sandstone, and the presence of "marine" beds in the immediately preceding Downtonian of the south of Scotland, proves the marine origin of the Scottish Old Red Sandstone. The Russian area, however, is far from Scotland, and is not proved to contain the distinctive Scottish facies of the Old Red Sandstone. It probably represents the site of deltaic accumulations on the margin of the Old Red Northland, in which some interfingering of the seaward and landward deposits took place. Prof. Barrell throws doubt on the marine nature of the Downtonian fauna, which consists of

plants, ostracods, phyllocarids, eurypterids, and fishes. He says: "There is to be noted the absence of cœlenterates, brachiopods, echinoderms, and trilobites, representatives of which are found in the true marine Ludlow rocks. This absence is as striking as the lingering presence of a few marine types. On the other hand, the ostracods, eurypterids, and fishes are groups which are found also as characteric fossils in clearly freshwater deposits as well as in brackish water or marine deposits." The same remarks apply as pertinently to the fauna of the Old Red Sandstone itself. The Downtonian probably represents deltaic deposition on a rising shore-line.

The Old Red Sandstone conglomerates have been ascribed to a littoral origin or to the beaches of lakes. But this view cannot now be held in view of the demonstration by Barrell and others of the insignificance of littoral deposition both in thickness and areal extent, and of the numerous chances against the preservation of littoral sediments (*i.e.* deposited within tidemarks) in the geological record. Even if the term "littoral" be extended to cover the deposits of shallow seas, this mode of deposition is inadmissible in the case of the Old Red Sandstone in view of the great thickness of its conglomerates and its persistent records of subaerial exposure. Even if a great lagoon or marine playa like the Rann of Cutch (8,000 square miles) be postulated, the difficulty of the maintenance of the nice balance of geological forces necessary to ensure conditions of subaerial exposure for long periods of time and during thick sedimentation must negative the idea of marine origin.

For a close analogy to Old Red Sandstone conditions of deposition in a much later geological period, Prof. Barrell cites the Tertiary deposits laid down in the intermontane basins between the growing ranges of the North American Cordillera. At the present time regions which have similar climatic and depositional conditions to those of the Old Red Sandstone are widespread. In particular South America east of the Andes and south of the Amazon basin, Central Africa except the Congo basin, and the southern alluvial basins and deltas of Indo-China, may be cited.

The study of sedimentation in relation to past climates and topographies has led Prof. Barrell to another fruitful line of thought, which concerns the effect of Silurian-Devonian climates upon the evolution of air-breathing vertebrates (see Barrell, *op. cit.*). He observes truly that the study of the rise of any group of animals involves not only biological considerations, but also an analysis and evaluation of the environment to the fluctuation of which they made the response that led to evolution. There is therefore a side of the story of life which belongs to physical geography, the study of past climates, geographies, and conditions of deposition; and it is from this point of view that the influence of the Middle Palæozoic climates upon the development of amphibia has been studied.

Briefly the rather complex argument is as follows: it is probable that fishes arose in land waters where they constituted primarily a river fauna. If they arose in the sea it is difficult to understand the comparative absence of their fossils from truly marine sediments, and their comparative abundance in the deposits of brackish and protected waters, and in sediments of truly continental origin. In the early Palæozoic the record of land deposition is very meagre, and consequently it is only in the opening phases of the Devonian that fish fossils are found in great abundance. At this time the acanthodian sharks were the dominant fishes in the continental waters; but in the later Devonian the sharks had disappeared from the fresh waters and the crossopterygians reigned in their stead. Their ability to use air adapted them better for life in a climate marked

by alternations of wet and dry seasons; and the few remaining dipnoans and crossopterygians still live under these conditions.

The earliest known amphibian skeletons were preserved in the coal-swamps of the later Palæozoic; but their fossil record may be traced as footprints in formations having the characters of semi-arid flood-plain deposits as far back as the Lower Carboniferous, and thus leads back to the habitat of the river fishes. The chief cause that controlled the evolution of the amphibia from fishes is found in the nature of the climates of the Silurian and Devonian, which favoured those fishes able to make continually larger and larger use of air during the recurrent dry seasons. It is believed that the air-bladder of fishes arose as a supplemental breathing organ; and under the compulsion of seasonal dryness its use for that purpose became essential. The relatively severe land conditions in which the fishes found themselves in the Devonian are consequently those which determined the whole course of vertebrate evolution, culminating in the appearance of man.

The argument is far more elaborate than can be indicated in a rough sketch such as this. To those who know his recent work on these subjects it will be superfluous to remark Prof. Barrell's acumen in making these clearings in a jungle of geological thought, to comment on his fair-minded manner, or to hold up for admiration his luminous, exact, and balanced literary style.

REVIEWS

GENERAL

Annals of the Royal Society Club. The Record of a London Dining-Club in the Eighteenth and Nineteenth Centuries. By SIR ARCHIBALD GEIKIE, O.M., K.C.B., D C L., Past-President of the Royal Society. [Pp. xv + 504, with contemporary portraits.] (London: Macmillan & Co., 1917. Price 18*s.* net.)

THIS long history of the Royal Society Club, or the Society of Royal Philosophers as they were called for fifty years, which is now the Dining-Club of the Royal Society, goes to prove that old Aubrey's definition of a Club is correct. Aubrey says: "We now use the word Clubbe for a sodality in a taverne," and the book before us shows that a great part of the doings of this ancient body has consisted of "sodality in a taverne," together, we will hope, with a certain amount of *convivium.*

From statements made by Pepys, Evelyn and others, we know that there is no doubt that certain of the Fellows of the Royal Society used to meet and eat together on the days of the meetings of the Society long before 1743, when the formal foundation stone of the Royal Society Club was laid; and we know that the Invisible College, which was the forerunner of the Royal Society, used to meet at the Bull Head in Cheapside, and it is not likely that they would have met in a tavern without eating and drinking together.

But the first attempt to make a formal incorporation took place on October 27, 1743, when eight gentlemen met together, two of them not at that date Fellows of the Royal Society, and formed what was to be called later the Royal Society Club.

The records of the Club or Society then formed are very extensive, the Minute Books being complete from the beginning to the present day, and the Dinner Registers complete from 1747 to 1855, and from 1879 to the present day. There is an unfortunate gap of twenty-four years in the Dinner Registers, which are apparently lost, as they have hitherto eluded the most careful search. This book was written mainly for the present members of the Club, but the records of a body of this character must have great value; partly on account of the members and guests having often been men of the greatest eminence, and partly on account of the long and interesting period which the history of the Club covers; and this book, done by a very gifted member of the Club and a Past-President of the Royal Society in the charming and intimate style suitable to such a record, will therefore have an historical value far beyond that of its immediate object, which is to detail the past history of the Royal Society Club for the instruction and delectation of its present members.

The book is written in the form of "Annals," that is, the details of each year's doings, as set forth in the annual report of the Treasurer, are recorded; interspersed with delightful short histories of new members of note, and with lists of the most important visitors and notes, sometimes copious, of any interesting facts connected with their lives. As an example of an account of a new member, the sketch of Henry Cavendish is a masterpiece. Any gleanings about the life and habits of this extraordinarily shy and reserved man, one of the greatest on the roll of the

Royal Society, are welcome; for we know really less of Cavendish and of his ways and habits than of any man of similar position. He came to the Club first as the guest of his father, and later became himself a member: he attended occasionally at first, but gradually became more and more regular in his attendance, until in 1784 he attended fifty-three dinners in the year. The meetings were then held every week during the year, irrespective of the vacations of the Royal Society. The Royal Society Club saw much more of him than did any of his contemporaries, and it is a great pity that there is not more in the way of definite record of his personality in their archives This shy and silent genius seemed to have found at the Club what he was seeking for, namely, an accoucheur for his thoughts; there he was sure to find some one or other waiting and seeking to give him assistance, and thus birth was given to a good conversation. As he became at home at the Club he invited guests, among them such men as D. Solander, Benjamin Franklin, Capt. Phipps, W. H. Wollaston, Dr. C. Hutton, Mr. Wedgwood, Matthew Boulton, Capt. Riou, Geissler and others, and the varied interests of such guests shows us that Cavendish must have had far wider and more human sympathies than those of the laboratory alone, with which he is generally credited. He dined at the Club up to about a fortnight before his death.

Another vivid sketch, this time of a strange guest, is that of Rudolf Eric Raspe. He was a man of remarkable originality, who gave Scott the idea of the character of Dousterswivel in the *Antiquary*, and who, amongst other things, was the author of *Baron Munchausen's Travels*, which thrilled us in our school-days; he was also a linguist, a versatile scholar, and a man of science. He was made a foreign Fellow of the Royal Society in 1769 for work which still holds good, but he was ejected from the Society in 1775 in consequence of its becoming known to the Council that he had stolen a number of valuable objects from the Cassel Museum, and that he had escaped from custody in Germany. Curiously, his name has never been removed from the list of Fellows, and it is to be found in the last revised edition of 1912. He wrote, and wrote well, on gems, volcanoes, oil-painting, and mineralogy, and died in Ireland in 1794.

Other slighter sketches such as those of Josiah Colebrooke, the Club's first Treasurer, of Benjamin Franklin and Thomas Young for instance, will arrest the reader's attention; and there are naturally copious notes on the geologists who attended the Club, either as members or guests. At the dinner of April 3, 1760, "Mr. Sterne, author of *Tristram Shandy*," was a guest: we should like to have Yorick's account of his dinner with the Royal Philosophers!

One curious fact, which is revolting to our sense of the sacredness of the word hospitality, must be mentioned: the visitors, apparently from the beginning, were allowed to pay for their own dinners! In 1831 a rule was made that "foreigners" should not be required to pay for their dinners, but in 1841 this rule was rescinded, and there is no indication in the records as to when the right rule, that guests should be paid for by the members who invited them, was introduced. This is all the more curious as in later years, as the present writer knows from personal experience, the hospitality of the Club has become a characteristic and is perfect: but so it ought to be, in order to atone for the sins of the past, which make the only blot on the history of this interesting body.

Another point of interest is that contemporary historical events are scarcely ever alluded to in the Club records. During the greater part of the earlier history of the Club, England was almost constantly at war in one or other part of the globe, and during its whole history some of the members and guests must

have been actors in most of these undertakings. But there is no mention of the great events which were happening outside, nor even of those which were taking place just outside their own doors, such as the riots in 1768, the Gordon riots in 1780, or the Chartist rising in 1848. The Club met and dined serenely as usual through all the wars outside and all the dangers at their own gates, and the Royal Philosophers remained true to their title: this, however, does not mean that they were wanting in patriotic concern then any more than now, but rather that the meeting together was a great relaxation to them, and that such companionship was most helpful to them in trying and strenuous times.

A few words must be given to the dinners themselves. Sir Archibald Geikie has given the menus of many of the earlier dinners, which will be of interest to the deipnosophists of the present day. The Bill of Fare consisted generally of "two kinds of fish at the top followed by joints of beef, lamb, veal or pork, with calves' head, brawn, bacon and greens, fried trype, wild ducks, lobsters, one or more 'plumb-puddings,' several apple-tarts, and ending off with butter and cheese." What strikes one most is the extraordinary solidity of the fare, which makes one marvel at the digestive powers of our ancestors; but perhaps the dinners were designed rather with a view to their appearance, than to the consequences which might follow from eating them. Evidently for the Royal Philosophers strong meat was indispensable! Some of the Bills of Fare given by the author make one agree that "Les restaurants anglais sont des hôpitaux pour guérir de la gourmandise."

So there are many subjects of interest in this book, all of them treated with a sympathetic humour which makes it delightful reading, quite apart from the more solid historical part; and any one who is interested in the scientific and social life of the period covered by the Club's history cannot fail to derive much pleasure and also knowledge from its perusal.

God the Invisible King. By H. G. Wells. (Cassell & Co., Ltd., 1917.)

Mr. Wells herewith presents to the world a succinct account of his religious opinions. He believes that the views which he advocates represent the tendency of thought exhibited by this age: and he has all the guileless confidence of an average apostle in the forthcoming universal adoption of his creed.

Our author appears to believe in two separate divinities. The one which he calls the "Veiled Being" is responsible for the creation of the universe, and stands behind the ultimate mysteries of nature. It appears to correspond with Spencer's "Unknowable," except indeed that its name suggests a living organism. The man of science will, however, regard it as no more than a name to cover our ignorance, and will fail to perceive the utility of bestowing titles which mean nothing while suggesting false connotations.

Mr. Wells, however, is more interested in his other invention—the practical God of humanity. It is not to be confused with the "Veiled Being," from which proceeds "an impulse thrusting through matter" in the best style of Bergsonian metaphysics. Nor is it the "Life Force," the "Will to Live"—for Mr. Wells makes free use of these and other meaningless catchwords of popular ignorance. On the contrary this second God is defined by four identifications. He is "courage"; he is "a person"; he is "youth"; and he is "love." The thinker who translates words into ideas will be totally at a loss to conceive an entity which is all these four things at the same time: a moral attribute, an organism, a stage of growth, and an emotion. Mr. Wells thinks he has "transferred the statements of science into religious terminology." The religious terminology is obvious, but

minute search has failed to reveal any clue to "the statements of science." We can do no more than express regret that Mr. Wells should have deployed his remarkable powers of language in defence of this new variety of mysticism and obscurantism. We cannot accept this *tour-de-force* of oratorical verbiage as a substitute for genuine ideas and plain statements. Mr. Wells has carried his genius for writing fiction into a sphere where it is out of place.

HUGH ELLIOT.

The Order of Nature. By LAWRENCE J. HENDERSON. (Cambridge, U.S.A.: Harvard University Press; London: Oxford University Press. 1917.)

THIS is an essay by the Assistant Professor of Biological Chemistry in Harvard University, on the subject of Teleology. This word is used by the author in a somewhat wider sense than usual, since he does not regard it as connoting necessarily either purpose or design. He employs it apparently merely to indicate the harmony and independence which are found throughout nature, and especially organic nature. We may remark at once that this book, although favouring some vague conception of a teleological ordering of the Universe, is on a much higher level both of thought and scientific knowledge than we usually find in works of this character. Especially valuable is the history of teleological ideas in the past. Having completed this survey, the author sets himself to inquire how an apparently purposive regulation of the Universe may be rendered compatible with a mechanistic theory. He bases this inquiry to a great extent on the work of Willard Gibbs. Although his analysis is worthy of careful study, we do not think its result can be considered satisfactory. The author's strength is in the extreme and intentional vagueness which he attaches to the word *teleology*. If by that he means no more than adaptation and harmony among living things, then there seems to be no special point in writing the book. If, on the other hand, he wishes to suggest that mechanical laws are diluted by "purpose," then the book fails completely in establishing that conclusion. By a conscious resort to vagueness, the problem is not solved but merely shelved.

HUGH ELLIOT.

The Borderlands of Science. By A. T. SCHOFIELD, M.D. (Cassell & Co., 1917. Price 6*s*. net.)

THE author of this work laments that a former volume from his pen, *The Unconscious Mind*, was received "with scorn and ridicule" when he "presented it to scientists at the close of the nineteenth century." We fear that no better fate can be anticipated for this new book, in which he makes a fresh incursion into the land of spooks. The author divides his work into two parts, "Light" and "Twilight," though the contents would undoubtedly be better described as "Darkness" and "Inky Blackness." A considerable part of the book is devoted to such subjects as telepathy, aura and colour vision, clairvoyance, and other so-called "psychical phenomena." Where he deals with the special sciences, he does not often express definite opinions, but indicates a decided leaning for every kind of heresy that he can find. He believes, of course, in vitalism; he believes that impressions on the maternal organism are specifically reproduced in the offspring, having known such cases himself; he is favourably disposed to the belief in inheritance of acquired characters, etc. More remarkable still, in his own subject of medicine, he believes that certain cases of mania, which he has seen, can only be accounted for on the assumption that the body of the patient is possessed by a spirit. Throughout the book Dr. Schofield exhibits a most

astonishing *flair* for methods of inquiry long since discredited, and known to be barren or deceptive.

HUGH ELLIOT.

MATHEMATICS

Differential Calculus. By H. B. PHILLIPS, Ph.D., Assistant Professor of Mathematics in the Massachusetts Institute of Technology. [Pp. vi + 162.] (New York: John Wiley & Sons; London: Chapman & Hall, 1916. Price 5*s*. 6*d*. net.)

THE author says in his Preface that in this book "a few central methods are expounded and applied to a large variety of examples to the end that the student may learn principles and gain power. In this way the differential calculus makes only a brief text suitable for a term's work, and leaves for the integral calculus, which in many respects is far more important, a greater proportion of time than is ordinarily devoted to it." After an Introduction, in which there are explanations of function, limit, and indeterminate forms, which are not quite satisfactory from a logical point of view, the concepts of derivative and differential are described. The explanation of the idea of a differential is good (pp. 15–16), though the way in which higher differentials are treated in certain cases (p. 30) is not satisfactory. The usual topics treated in courses on the differential calculus are gone through. Rates of change of position and velocity on a straight line and a curved path are not introduced, I think, as early as they might be with advantage. The enunciation and exemplification—it is not more than this—of Rolle's theorem (p. 94) has a bad mistake; the examples are not examples of discontinuity of the derivative, as the author states, but of its *non-existence*. There are certainly some advantages in the way in which the subject of the differential calculus is exposed in this book: for example, I may notice the—to a student—illuminating definition given (p. 10) of a "continuous" function. But I hardly think that such advantages compensate fully for the decided disadvantages of carelessness and lack of logic. Even elementary students in mathematics are far more puzzled by a teacher's logical obscurities than teachers seem inclined to admit, and, at any rate for the training of future mathematicians, even what may seem to some to be excessive care in logic may not be out of place. It is quite true that even more important in teaching than strict logic is suggestiveness of ideas and stimulation of the student's imagination; but this book seems to fall short in this requirement also.

PHILIP E. B. JOURDAIN.

Elliptic Integrals. By HARRIS HANCOCK, Professor of Mathematics in the University of Cincinnati. [Pp. 104.] (New York: John Wiley & Sons, Inc.; London: Chapman and Hall, Ltd., 1917. Price 6*s*. net.)

THIS volume is the eighteenth of the excellent series of "Mathematical Monographs" edited by Mansfield Merriman and Robert S. Woodward, and is a treatise almost entirely on the well-known elliptic integrals of Legendre. It may thus be regarded as a somewhat more advanced part of a course on the integral calculus, in which are considered integrals of rational expressions involving square roots of cubics and quartics in one variable. The integrals of the three kinds and the Legendre transformations are treated in the first chapter, but on the whole, in order to keep within a limit of about one hundred pages, the author has had to confine the discussion almost entirely to the elliptic integrals of the first and second kinds. In the second chapter the functions which arise from the inversion of elliptic integrals are shortly described, and their doubly periodic properties are emphasised. The third chapter is concerned with the reduction of

elliptic integrals of the first kind to Legendre's normal form, and the fourth chapter is on numerical computation of the elliptic integrals of the first and second kinds, and on Landen's transformations. The fifth chapter is concerned with various examples and problems--the rectification of the lemniscate and the ellipse, the motion of a pendulum, and so on. The sixth chapter gives five-place tables, from Levy's *Théorie*, of integrals of the first and second kind. There is a short account of the history of the subject in the introduction (pp. 6-8), but, of course, in such a small space the account is not of much interest to a student. This book is of value to those students who have a taste for the rather complicated technical work of calculation involved in this "higher trigonometry": it seems to me much the best plan to introduce students to the study of elliptic functions in the way followed in this book.

PHILIP E. B. JOURDAIN.

Leçons sur les Méthodes de Sturm dans la théorie des équations différentielles linéaires, et leurs développements modernes, professées à la Sorbonne en 1913–1914. By MAXIME BÔCHER, Professor at Harvard University and (temporarily) at the University of Paris. Collected and edited by GASTON JULIA. [Pp. vi + 118, with 8 figures.] (Paris: Gauthier-Villars et Cie., 1917. Price 5 fr.)

THIS book is a very well written introduction to the interesting methods first developed by Sturm in a memoir published in 1836 for the discussion of what are now known as boundary value problems with ordinary differential equations. Of course the question of boundary values first comes before a student, as it did in history, in discussing the partial differential equations of mathematical physics, but it is well known that such discussions can often be reduced to the study of certain ordinary differential equations of the second order. The third chapter of the present treatise is devoted to a study of the results given by Sturm in his first memoir; his first great theorem (pp. 45–6) is that the zeros of the real independent solutions of a linear and homogeneous differential equation of the second order with real coefficients are mutually separated from one another. The first chapter is a treatment of the theorems on the existence of solutions of ordinary real and linear differential equations of the second order; the second chapter is on the analogies of linear differential systems with linear algebraic systems—Sturm, like many before him, having regarded differential equations as limits of difference equations; the fourth chapter is on the characteristic functions and their zeros in two problems which go beyond the problems of Sturm treated in the third chapter, and here there is some account of the important work of Klein (1881); and the fifth chapter is on those properties of Green's functions which can be extended to ordinary linear differential equations and their applications. It should be added that this volume is one of M. E. Borel's famous series of monographs on the theory of functions.

PHILIP E. B. JOURDAIN.

Leçons sur les Fonctions Elliptiques en vue de leurs applications. By R. DE MONTESSUS DE BALLORE. Cours libre professé à la Faculté des Sciences de Paris. [Pp. x + 267.] (Paris: Gauthier-Villars et Cie., 1917. Price 12 fr.)

THERE have been lately many indications of a growing feeling that, for purposes of application to mathematical physics, the elliptic functions of Jacobi are far more convenient to deal with than those of Weierstrass. In fact, it is possible to begin the study of elliptic functions as soon as the principles of the integral calculus have been mastered, and this study is necessary, for at each step in applied mathematics we meet elliptic functions, and it seems a mistake to consign the theory of such functions to the "higher" branches of analysis.

The object of this book is to set forth in an elementary way the fundamental properties of the elliptic functions. The whole work is divided into four parts, consisting in all of twenty chapters: in the first part the Jacobian functions are treated; in the second the Weierstrassian functions; in the third the general principles of the subject from the point of view of the theory of functions; and in the fourth the Theta functions. In the first part, the first chapter deals with the definitions of the functions *sn*, *cn*, and *dn*, their elementary properties, their real periods, their derivatives, their developments in series, their variations, and their addition-formulæ; the second chapter with imaginary variables, the Jacobian functions for their imaginary periods, and their zeros and poles; the third chapter with elliptic integrals; the fourth chapter with numerical calculations of elliptic integrals and functions; and the fifth chapter with the direct calculation of elliptic integrals of the first and second kinds. The only criticism which is at all obvious in this very complete and thorough work seems to me to be in the, to a student, not very clearly proved statement (p. 150) that if an elliptic function were finite in an elementary parallelogram, it would be finite at all points at a finite *or infinite* distance, because of the double periodicity. No further foundation of this statement is given, though it is evidently not necessarily the case that a function which is finite at all finite points is finite everywhere.

PHILIP E. B. JOURDAIN.

A Treatise on the Analytical Dynamics of Particles and Rigid Bodies; with an Introduction to the Problem of Three Bodies. By E. T. WHITTAKER, Hon. Sc.D. (Dubl.), F.R.S., Professor of Mathematics in the University of Edinburgh. [Second edition. Pp. xii + 432.] (Cambridge: University Press, 1917. Price 15*s*. net.)

THE first edition of this excellent treatise was published in 1904, and there are not very many important alterations in this second edition. The author says in the preface to this edition that he has "endeavoured to give references to, and in some cases accounts of, the numerous original researches in Dynamics which have been published by various investigators since the first edition appeared." The chief value of the book lies in the thorough, systematic, and modern treatment of the integration and transformation of the dynamical equations. The problems of dynamics are approached in a general and analytical way that might, I think, be introduced with advantage at a somewhat earlier stage in our teaching than is usually done. Much more light is thrown on the subject when we inquire in general into the nature of those problems which makes them solvable than if we merely collect individual examples of solvability, as if nature were a huge examination paper; and there is no need to spend quite such a long time on what is called "elementary" work. The most noteworthy additions to this edition are the new explanation of the contact-transformation theory of dynamics and Hamilton's characteristic function (pp. 288-92), and the brief account of Sundman's regularisation of the problem of three bodies (pp. 411-12). Also there are some changes in the treatment of the motion of a body about a fixed point under no forces (pp. 144-52) which arise from Prof. Whittaker's opinion that the Jacobian elliptic functions are preferable to the Weierstrassian ones in numerical calculations. The only marked criticism that seems to me might be made on the new edition is that there is rather a noticeable—in view of Prof. Whittaker's remarks in the preface—neglect of work published from 1904 to 1908 on the questions that arise in formulating Hamilton's principle and the principle of least action for non-holonomic conditions and generalised co-ordinates.

PHILIP E. B. JOURDAIN.

An Introduction to the Use of Generalised Co-ordinates in Mechanics and Physics. By WILLIAM ELWOOD BYERLY, Perkins Professor of Mathematics Emeritus in Harvard University. [Pp. viii + 118.] (Boston, New York, Chicago, London: Ginn & Co., 1916. Price 5*s*. 6*d*.)

THIS is an exceedingly valuable working text-book, and it is marked by exceptionally good examples and summaries of the contents of chapters, which will be most useful to students engaged in the work of revision. The introductory chapter is on co-ordinates and various aspects of the dynamics of a particle and rigid dynamics, and Lagrange's equations are introduced almost at once. The second chapter is devoted to the Hamiltonian equations, Routh's modified Lagrangian expression, and the ignoration of co-ordinates; in it the "canonical form" of the Hamiltonian equations is not given, but is postponed to the fourth chapter, and the equations considered are of the form usually known as "Poisson's." A very good feature is the use of such equations in a few problems already solved by the Lagrangian process, so that the student is familiarised with the actual working of the Hamiltonian forms. The third chapter is on impulsive forces; the only criticism that we have to offer here is that the author so closely follows Routh that an impression may be given to the student that Gauss's principle of least constraint is merely a principle for use in problems on impulsive forces (p. 71). The fourth chapter is on conservative forces, and it is for these forces that Hamilton's principle and the principle of least action are derived. However, in a note on p. 90 it is shown that Hamilton's principle is of wider signification. The fifth chapter is on applications to physics, and here we have a short treatment of the important question of concealed bodies. Two Appendices contain respectively a brief syllabus of rigid dynamics and a useful summary of part of the calculus of variations.

PHILIP E. B. JOURDAIN.

ASTRONOMY

A Manual of Field Astronomy. By ANDREW H. HOLT. [Pp. x + 128.] (New York: John Wiley & Sons, Inc; London: Chapman and Hall, Ltd., 1917. Price 6*s*. net.)

THERE are so many books on Field Astronomy that the appearance of another almost demands an apology from the author as a matter of course. The present volume has been written with a definite aim—to provide a more complete treatment of the methods of determining longitudes, latitudes, azimuths, and time than is usually given in text-books on surveying, but at the same time something more elementary and less extensive than is given in books on field astronomy. It is intended for use by civil engineers and to give sufficient information to enable the observations and computations required in general engineering and surveying to be made. The need was a real one, and is well met by this volume.

The arrangement of the subject is admirable; a brief but sufficiently adequate treatment of the fundamentals—the celestial sphere and systems of co-ordinates—is followed by a more detailed description of the measurement of time, the use of a *Nautical Almanac* and various problems in conversion of time. The more common methods for the determination of latitude, azimuth, time, and longitude are then given, and the various corrections necessary are explained. At the end of the book is collected a number of actual observations by the various methods to illustrate how systematically to record and compute the observations in the field. A "summary of observations" is collected together in one chapter, with sufficient facts to determine what data are necessary for any observation, and which is the most convenient method to use under any given circumstance.

A collection of tables for convenience in computations is added, together with appendices in which are given the derivation of the formulæ in spherical trigonometry which are used in the book, and a description of the solar attachment of a transit.

The book is of convenient size to slip into the pocket, and has a limp cover. The price is unduly high for the size of the book. H. S. J.

PHYSICS

The Teaching of Physics. By C. RIBORG MANN, Associate Professor of Physics at the University of Chicago. [Pp. xiv + 304. Teachers' Professional Library.] (New York: The Macmillan Co., 1915. Price 5*s*. net.)

IT is to be hoped that in the near future the teaching of science will come to be regarded as one of the essential features of a secondary school course; nevertheless, as H. G. Wells has, in effect, lately pointed out, the impetus given to the cause of science by the war will largely die away unless its advocates come forward with a clear-cut account of their needs and ideas. Among these the scheme of school teaching is of primary importance. If Physics is to be taught to the masses, to young people who have no thought of specialising in it or in any branch of science whatever, then it becomes a serious question whether the methods which are now in general use are best adapted for their needs. Are they best designed to supply them with knowledge which will be useful to them in their everyday lives and, more important still, do they develop a "conscious ideal of scientific method" which is transferable to their work outside the laboratory?

To elucidate this problem Prof. Mann, in the first section of his book, traces the development of Physics teaching in the United States from the early days of last century and shows how, under the influence of the University, modern teaching has drifted away from the original inductive scheme of natural philosophy and become deductive and mathematical. Without criticising this scheme so far as it applies to the University, he is doubtful of its desirability for youthful learners, and in the next part of his book proceeds with his purpose of "finding out how far the science of Physics may be made to contribute most efficiently to the development of democracy." The method and discipline of Physics are considered at length, and finally, as a result of his conclusions, the author puts forward a scheme for experimentation designed to appeal to the emotions of the student, coming into touch right at the start with his everyday experiences and gradually leading him into more abstract regions as his interest is aroused. This is shown to involve a reversal of the "present logical method which proceeds in the order: principle, demonstration, exemplification in the laboratory, application," substituting for it as a "psychological method the order: application, problem, solution in laboratory, principle." As an illustration, Prof. Mann suggests that Mechanics should be commenced with the idea of Work (using the term poundweight just at first without definition), and Electricity with an experiment on the measurement of energy in watt-seconds with an ammeter and voltmeter. There is at least no doubt that such a course would prove more interesting than those in vogue in England to-day, where it sometimes happens that a student comes to his intermediate science examination without ever having used either of these instruments!

However much one may disagree with him, it must be admitted that these later chapters in Prof. Mann's book which deal with the organisation of the course make very suggestive reading, and the scheme he proposes, amplified by a little modern theory, might make no bad basis for the rigid development of the science which *must* be given to the specialist at college. There is just one point which cannot be allowed to pass without notice. In the chapter entitled "The Pedigree

of Physics" there is suddenly sprung on the reader the remarkable proposition that *Germanic* Industry is the father of modern Physics! However, as this statement is backed only by quotations from the opinions of Houston Chamberlain, its absurdity is manifest and the adjective becomes ridiculous rather than offensive.

D. O. W.

Advanced Text-Book of Magnetism and Electricity. By R. W. HUTCHINSON, M.Sc., A.M.I.E.E. [Vol. i., pp. vii + 372; vol. ii., pp. vi + 468; not sold separately.] (London: The University Tutorial Press, 1917. Price 8*s*. 6*d*. published.)

THIS book is written to cover the ground of the Final Degree Examinations of the Universities. Both the experimental and theoretical sides of the work are fully treated, and a comprehensive account of the main principles of the subject is given which embodies the distinctive results of modern research. For its professed purpose the book seems admirably fitted. The text is carefully written; diagrams are clear and abundant, and there are a considerable number of fully-worked examples illustrating the application of principles, with a large selection of problems to be attempted by the student. In such a work naturally the greater part of the space is devoted to what one may call the classical parts of the subject, but the author contrives to give brief accounts of a goodly number of recent investigations, and four chapters in the second volume are devoted to electric oscillations, passage of electricity through gases, radioactivity, and the "New Physics" which bases all electrical and optical phenomena on the electron. So determined is the author to be "up-to-date," that there are short sections devoted to such subjects as Thomson's recent work on Positive Rays, the investigations of Laue and the Braggs on X-Ray Diffraction and Crystal Structure, and Planck's Theory of Quanta. These are in the nature of things brief, but they are sufficient to give the intelligent student some notion of the direction in which physical research is going at present. To all students reading for their final degree the book can be heartily recommended.

J. RICE.

CHEMISTRY

Chemical Discovery and Invention in the Twentieth Century. By SIR WILLIAM A. TILDEN, F.R.S., D.Sc., LL.D., Sc.D., Professor Emeritus in the Imperial College of Science and Technology. [Pp. xvi + 487, with 161 illustrations, plans, and portraits.] (London: G. Routledge & Sons; New York: E. P. Dutton & Co. Price 7*s*. 6*d*. net.)

PROF. SIR WILLIAM TILDEN has placed us once more in his debt by the timely issue of his book on *Chemical Discovery and Invention in the Twentieth Century*.

With the experience of many a lecture, both of the popular and technical variety, Sir William has recognised from the outset the necessity of providing such a book as the present with a diversity of photographs and diagrams if the attention of the dilettante and the casual reader are to be fixed.

The result has been to produce a book which is deeply interesting and fascinating, not only to the lay mind, but also to the scientific elect, who sometimes are inclined to forget that the materials they deal with in the laboratory by grams and ounces are being handled in the outside world by the ton and the thousand tons, and may, as steel or nitric acid for instance, be helping to shape the destiny of mankind for ages to come.

The book is divided into four parts, namely:

Part I. Chemical Laboratories and the work done in them.

Part II. Modern discoveries and theory (covering radium, electrons, solutions, catalysis, colloids, etc.).

„ III. Modern applications of chemistry (including such diverse matters as water, metals, coal-tar dyes and drugs, explosives, and the fixation of atmospheric nitrogen).

„ IV. Modern progress in organic chemistry (including sugar, albumens, enzymes, etc.).

It will be seen, therefore, that the work is most comprehensive; but, despite the large amount of material dealt with, it is always concise and eminently readable. The illustrations are admirable and the book well got up; in fact, one can imagine no better present to a boy (of any age from eight to eighty) than a copy of this work.

One may venture to congratulate both Sir William Tilden and the publishers on the production of so excellent a volume. FREDERICK A. MASON.

Ozone, Its Manufacture, Properties and Uses. By A. VOSMAER, Ph.D. [Pp. xii + 197.] (London: Constable & Co., 1916. Price 10*s*. 6*d*. net.)

OZONE, like many other electrically formed substances, has always preserved for the general public an atmosphere of mystery, and its production has been regarded by the scientific "man in the street" as one of those subtle phenomena that are not susceptible of accurate experimental verification. In his preface Dr. Vosmaer states that there are many publications on ozone that cannot be classed otherwise than as commercial information and advertising matter. He states, however, that the main object of this book is to "give a full outline of our personal experience," but he adds: "That same experience has made us very critical about outside information." The account given of the properties and chemical structure of ozone is incomplete and unsatisfying, and this applies to a good many other sections of the book.

The chapter, however, dealing with the brush discharge and the conditions which underlie its successful production is excellent; it contains a good summary of our knowledge of the subject and will be useful for reference, but the book lapses again when the author begins the description of the various apparatus that have been constructed for the production of ozone, or "ozonators." The Schneller system is dismissed as follows: "When once somebody has proven a generally accepted statement, in this case the necessity of a solid dielectric, to be wrong, it is easier for some one else to continue research in that line of deviation (!). *That* credit should be given to Schneller." Dr. Vosmaer, however, is equally frank about his own system in which no dielectric was used. It "has had to undergo the same fate as all the other attempts to go round the dielectric. Neither this nor the Otto nor the Schneller nor the Tesla system has succeeded."

The descriptions of successful ozonators are not very clear or detailed, and no information is given which could be of use to those desiring to construct such apparatus for experimental purposes. The author, however, gives an alluring account of the uses of ozone. One is apt to feel that the scientific attitude of complete detachment from financial results is prejudiced when one comes across such statements as these:

"There is any amount of evidence to be had that, in Europe, the success is irrefutable, and yet here in America people are not daring enough to even try.

"The explanation is not far to seek: the application of ozone is of a very simple nature for those that know how to handle it, but it is no easy matter for those that have looked at it, and believe themselves clever enough to copy it."

There is sound truth in the statement that follows: "Ozone does its work. That is a fact beyond question, ozone can be made in any quantity. That is another fact, but it does not include (!) that every layman who 'invents' an ozonator will be able to manufacture ozone on a large scale and apply it on a commercial basis."

The use of ozone for the purification of drinking water and for sterilisation, its advantages for the purification of air, for bleaching and other industrial purposes, and for therapeutics, of which detailed descriptions are given, are sufficient to emphasise the importance of the study of its production, and its practical utility under modern conditions. For all those interested in ozone from that standpoint this book should provide a useful work of reference.

Theoretical Chemistry from the Standpoint of Avogadro's Rule and Thermodynamics. By PROF. WALTER NERNST, Ph.D. Revised in accordance with the Seventh German Edition by H. T. TIZARD, M.A. [Pp. xix + 853.] (London: Macmillan & Co. Price 15*s*. net.)

PROF. NERNST'S *Theoretical Chemistry* has become so much a standard work that the appearance of a new edition calls for little fresh criticism. As the translator points out, the gradual evolution of the book has caused it to assume a rather specialised form owing to the impossibility of discussing the whole realm of theoretical chemistry within two covers.

Thus the present edition contains practically no account of recent work on radioactivity or the atomic theory, whilst Prof. Nernst's own researches are treated fully; again, the important subject of tautomerism, which plays so large a part in the mechanism of organic chemical reactions, is compressed within a page and a half, and the valuable work of Dimroth, K. H. Meyer, Acree, and others on the mechanism of the changes and the influence of solvents, is summarily dismissed with a single reference to Dimroth's work; catalysis again is surely a subject deserving of somewhat lengthier treatment than the paragraphs allotted on pp. 616–17.

In spite of the mass of valuable material contained in the book, one cannot but feel that it has reached a point in its development when it must either expand into two volumes, so as to cover the whole field of chemistry, or else the work must definitely specialise on certain points on which Prof. Nernst is a recognised authority.

As it stands, one is bound to notice a certain patchiness in the work, due to the elaboration of certain aspects of chemistry at the expense of others.

In general, however, the book is fully up to the standard of previous editions, and the interesting account of Nernst's own researches almost makes up, perhaps, for the scanty treatment of other branches of chemistry.

FREDERICK A. MASON.

The Problems of Physiological and Pathological Chemistry of Metabolism for Students, Physicians, Biologists, and Chemists. By DR. OTTO VON FÜRTH, Professor Extraordinary of Applied Medical Chemistry in the University of Vienna. Authorised translation by ALLEN J. SMITH, Professor of Pathology and of Comparative Pathology in the University of Pennsylvania. [Pp. xvi + 667.] (Philadelphia and London: J. B. Lippincott Co. Price 25*s*. net.)

THE present work is described as a translation of the second volume of v. Fürth's *Probleme der Physiologischen und Pathologischen Chemie.* It deals with the

secretion and actions of the digestive juices, with the changes undergone by the products of digestion and of metabolism in the animal economy, and with the processes and mechanisms of respiration and of thermotaxis. The subject is presented, attractively, in the form of twenty-five lectures, in style somewhat reminiscent of Bunge. The presentation is original. It bears throughout the thumb-mark of its author, whose work adds the authority of the laboratory to that of the rostrum.

The translation is fluent and readable. It contains a few oddities, *e.g.* "Lab-process," "Rest-nitrogen," "Electromotor power," which, for the most part, are half-translated Germanisms, free from ambiguity, and do not materially diminish the interest or the value of the volume. It is more seriously marred by omission of dates from the prefaces. Apart from the words "Copyright 1916" on the back of the title page there is nothing in the volume to imply when it was translated; nor anything, save an implication (in the translator's preface) that the German edition appeared in or about 1913, to fix the date of publication of the original lectures.

But the volume is of real value. It is described, very happily, by the translator as being "rather a guide to thought than to the technicalities of the laboratory," and may be commended heartily to advanced students, to experimenters, and to those engaged in human and in veterinary medical practice as an interesting and suggestive contribution to the subject with which it deals.

W. L. S.

GEOLOGY

William Smith: His Maps and Memoirs. By T. SHEPPARD, M.Sc., F.G.S. (*Proc. Yorks. Geol. Soc.* 1917, **19**, pt. iii. pp. 75–253, 17 plates.)

THIS memoir is by far the most complete account extant of the epoch-making work of William Smith, the father of English stratigraphical geology, as it is set forth in his maps and memoirs. Especial attention is given to his stay in Yorkshire during the latter part of his life, and to his scientific activities during that period. At this time Smith made a geological map of the district of Hackness, the extraordinary accuracy of which is illustrated by the severe test of placing it side by side with the 6-inch geological map of the same district as surveyed by the late C. Fox-Strangways in 1878. A new discovery of MSS. by Smith has been made through relatives of R. Turnbull, who was a pupil of Smith's during his sojourn at Hackness. These documents, which have been presented to the museum of the Scarborough Philosophical and Archæological Society, include several short scientific papers, some of which are printed *in extenso* in appendices. The memoir also contains a valuable account of earlier attempts than Smith's to indicate soil-boundaries and other geological information on maps. Another feature of interest is the inclusion of a series of notices of Smith's geological work, testifying to the growth of the universal regard in which he is now held. The work is profusely illustrated with fac-similes of Smith's maps and the title-pages of some of his works.

G. W. T.

BOTANY

Carbon Assimilation: A Review of Recent Work on the Pigments of the Green Leaf and the Processes connected with them. By INGVAR JÖRGENSEN and WALTER STILES. [Pp. iv + 180, with 18 figs.] (London: William Wesley & Son, 1917. Price 4*s.* net.)

IT is well recognised that without the sun life on this planet would be impossible, but it is not always so clearly understood that without the green plant and its

sun-driven activities neither man nor other animals and plants could exist. Green plants are thus the physiologically basal organisms in the evolutionary edifice. Such plants owe their superiority to their capacity to use again not only their own waste products but those of other organisms. In common with other living organisms the green plant produces carbon-dioxide, but in contrast with them it possesses the power of building up again—in the process of carbon assimilation under the influence of sunlight—fresh food material from this substance. A clear conception of the present state of our knowledge of this fundamental process is thus of great importance not only to plant physiologists but to the agriculturist and science workers generally. Messrs. Jörgensen & Stiles have placed such workers under a heavy debt of gratitude by the publication in book form of their review which has recently been appearing in the *New Phytologist*. Very considerable advances have been made of late years in this field, and it is with the work of the last twenty years that this review deals. It should do much, as the authors hope, to convince those who still doubt the position of plant physiology as an independent science. The great value of the work lies in the fact that it is no mere résumé, but is throughout severely critical. We are thus left with a very clear picture of the present position of the subject stripped from the unfounded conclusions and the mists of hypothesis which have obscured some aspects of the process. The authors start with an admirable account of Willstätter's work on the chemistry of the green pigments of the leaf, and they had the happy idea of appending a number of simple laboratory experiments which can be carried out by students. They then deal with the paths of gaseous exchange in the green leaf and with the factors influencing the intake of carbon dioxide; to the last section the Cambridge school of plant physiology, under F. F. Blackman, have made the largest contributions. Further chapters deal with the products of carbon assimilation, the energy relations in carbon assimilation, and theories of carbon assimilation. As an example of very sound critical treatment we may instance the chapters on the factors influencing the rate of assimilation and on the products of assimilation. The emptiness of the several theories of carbon assimilation is exposed with a ruthless hand, but it will certainly save in future much misguided labour. The review indicates clearly the many directions in which the subject can be profitably pursued. When it is realised that carbon assimilation is the basis of all crop-production, it is clear that on economic as well as on purely scientific grounds a fuller knowledge of this fundamental process must be of the greatest value. With the renewed interest in agriculture in this country it is to be hoped that a serious attack on this problem may not be long delayed.

V. H. B.

ZOOLOGY

A Critique of the Theory of Evolution. By PROF. T. H. MORGAN. [Pp. x + 197, with 95 figures.] (Princeton: University Press; London: Oxford University Press, 1916. Price 6*s*. 6*d*. net.)

THIS is a series of lectures delivered under the Louis Clark Vanuxem Foundation by one of America's best workers and a well-known upholder of Mendelism. The lectures were delivered to a mixed audience, and little if any biological knowledge is assumed. The result is an admirable introduction to the theory of organic evolution. This in itself is very useful, and should be read by all students of biology, as it gives a review of the past work and present position in a concise manner, but nevertheless in a critical spirit. We cannot quite accept all the criticisms, but this is only a matter of detail that detracts but little from the work.

Particularly interesting are the parts on the inheritance of a number of characters in the fruit-fly *Drosophila*, whose name has become a veritable household word to all biologists owing to the classical investigations of Prof. Morgan and his co-workers. This information, accessible to the specialist in a long series of papers from Morgan's laboratory, is here put within the reach of all and in such a form that it can be comprehended by the beginner. Its clearness is helped in large measure by a fine series of figures, many reproduced from actual photographs.

It may be that a study of biological problems tends to produce a conservative attitude which looks with suspicion upon new theories, or perhaps it is that a knowledge of the failure of the many great theories of the past to provide a complete explanation of inheritance results in a very healthy scepticism of a panacea. It may be both. Whatever it is, however, when we read that "with the discovery of this mechanism (Mendelism) I venture the opinion that the problem of heredity has been solved," and further, "So I repeat, the mechanism of the chromosomes offers a satisfactory solution of the traditional problem of heredity," we admire the courage but doubt the wisdom of the prophecy. It is the privilege of a pioneer to work with unbounded enthusiasm in the future of his theories, and when this is backed by such substantial results as in Morgan's case no other course is open save to treat it with profound respect. When, however, we consider that other good workers have been led to an entirely opposite opinion, we are forced to the conclusion that a verdict of "non-proven" more precisely states the case.

The book is very useful to the student, be he beginner or more advanced, and one that can be read with profit and considerable pleasure by all interested in biology. It is perhaps significant that at the time of writing the supply in this country is exhausted.

C. H. O'D.

On Growth and Form. By D'ARCY WENTWORTH THOMPSON. [Pp. xvi + 793, with 408 illustrations.] (Cambridge: at the University Press, 1917. Price 21*s*. net.)

IT is of course obvious upon reflection that if animals utilise inorganic materials, as they do to an enormous extent in their skeletal structures, such substances necessarily conform to the ordinary physical laws. Further, that animals being in the main solids or semi-fluid must conform to the ordinary laws governing the relation of surface to volume, of surface tension and so on. Few, however, have troubled to trace to what an extent these laws of physical science apply in the animal kingdom, and with a somewhat characteristic distrust of mathematics and physics the biologist has been content to leave such questions alone. In so doing a wide field of research has been left almost untrodden, and as this volume will soon show, a field that has already yielded a number of interesting results and promises a rich harvest to future investigators. The criticism applies particularly in this country, and Prof. Thompson deserves thanks for bringing to notice a large number of the phenomena to which these mathematico-physical laws apply. Although in the main dealing with animals, their application to the plant kingdom is also dealt with, as for example in the height of trees, the arrangement of cells, the formation of intercellular partitions, and Phyllotaxis.

It is of course in the skeletal structures that we find the most striking examples of the working of these laws, and such parts as horns, tusks, shells, and spicules receive full treatment. It is a pity that the book was published too early to include a reference to the recent interesting work of Dendy and Nicholson on the

last of these. The soft parts are also treated, and we have accounts of the forms of cells, the internal structure of cells, etc. Again, in dealing with rate of growth the date of publication did not allow of a reference to Donaldson's book on the White Rat.

One small point for criticism occurs, and that is the references to the literature. In our opinion it would have been better to have given a bibliography at the end of each chapter, as this facilitates the refinding of a reference only vaguely remembered in reading the book. By no means the least interesting chapter of the book is the last, which deals with "The Theory of Transformations, or the Comparison of Related Forms," in which the outlines of related organisms or of skulls of related organisms are inscribed in systems of Cartesian co-ordinates with most striking results.

The modest claim in the Epilogue that "my task is finished if I have been able to show that a certain mathematical aspect of morphology, to which as yet the morphologist gives little heed, is interwoven with his problems, complementary to his descriptive task, and helpful, nay essential, to his proper study and comprehension of Form" is fully substantiated. The author is to be congratulated on a very interesting book that has an appeal for biologist and mathematician, and in which both will find many suggestive hints for future work.

C. H. O'D.

Genetics and Eugenics. By Prof. W. E. CASTLE. [Pp. vi + 353, with 132 figures and 37 tables.] (Harvard University Press; London: Oxford University Press, 1916. Price 8*s*. 6*d*. net.)

THIS book is composed of three parts—the first on Genetics, the second on Eugenics, and the third, in the form of an appendix, is a translation of Johann Mendel's paper on plant hybridisation. The need for such a book was felt by the author, who annually gave a course on these subjects to his pupils, and its aim is clearly set out in the sub-title, "A Text-book for Students of Biology and a Reference Book for Animal and Plant Breeders." It quite realises its object and is a source for a great deal of useful information on the matters with which it deals, giving in its extensive bibliography references to the most important works on the subject. The addition of a number of photographs of the actual animals referred to in the text brings home the salient features in a striking manner and adds much to the appeal of the book.

The part dealing with Genetics is a very plain, straightforward account of our present-day knowledge in the subject prefaced by a short historical introduction. The chapters on Eugenics are much shorter, mainly for the obvious reason that our knowledge is much less extensive. On the whole the author is inclined to lay too much stress on the Mendelian interpretation of inheritance, and tends to try to fit the theory to the facts, even when its application is by no means obvious. He does not go to the extremes sometimes encountered, and criticises very justly the unreliability of the American data obtained by the Eugenics Record Office, owing to the questionnaires being drawn up on the assumption that all characters are inherited in Mendelian ratios.

His conclusions with regard to the improvement of the human race will probably meet with fairly general acceptance. Although we do not know enough yet to manage human marriages as we do the mating on a stud farm, nor might it ever be desirable from social and sentimental reasons to do so, yet we do know enough to limit the propagation of the unfit. This whole question of the future of the race is of paramount importance, and is particularly brought home at the

present time by the wastage of fine lives in the war. It is therefore desirable that every student of biology, and every layman too for that matter, should be informed in such matters. This information is made available in a readable manner in the present volume, and although the author puts forward his own views, this is done without prejudice to other schools of thought. The whole forms a very fair and reasonable review of our knowledge in these two fascinating fields of biological inquiry. C. H. O'D.

The Fundus Oculi of Birds especially as Viewed by the Ophthalmoscope. By CASEY ALBERT WOOD, M.D. [Pp. 180, with 145 drawings in the text and 61 coloured paintings by A. W. HEAD, F.Z.S.] (Chicago: The Lakeside Press, 1917.)

THE ophthalmoscopic examination of the eye of birds is a subject that has received practically no attention from naturalist or ophthalmologist, and the present volume lays a very satisfactory foundation for the bridging of the gap in our knowledge. Wood, a well-known ophthalmologist, has been fortunate enough to secure the co-operation of A. W. Head, whose skill in depicting the *Fundus oculi* is well known, and the result is a beautifully illustrated volume. The general "get-up" of the book is in keeping with the illustrations and is very satisfactory. In probably no vertebrate group is so much variation met with in this part of the eye as in birds, and the thorough examination of a large number of different species suggests the possibility of utilising the conformation of the fundus for systematic purposes.

To one used to the fairly constant relation of the *macula lutea* with its fovea in the mammals it comes as a surprise to find that oval, ovoid, circular, or quite long ribbon-like areas of acute vision are found in birds. These also may be supplied with two maculæ fairly widely separated from one another, as, for example, in the Flamingo (*Phœnicopterus roseus*). The great variation in the size, shape, and complication of the pecten is also fully dealt with; but although several theories regarding its function have been advanced, it cannot yet be said that any one of them is completely satisfactory, although it appears to be correlated with the relation between monocular and binocular vision.

The fundi of certain other vertebrates have also been examined, and the author says: "If one may draw any conclusion from such sparse material and from such an incidental examination of the subject, it is that whatever of common origin the avian and reptilian classes may have originally had the ornithological branch left the parent stem with a subdivision of the Lacertilia and not with the Ophidia."

A useful Bibliography and account of the technique of ophthalmological investigation is given, and it is interesting to note that the ordinary mydriatics for dilatation of the pupil have little effect in birds, where, owing to the anatomical relations of the lachrymal apparatus, drops instilled into the eye run down into the œsophagus. The most successful agent in widening the pupils is a 1 per cent. solution of nicotin, and the author suggests that the action is a general systemic one and not specific for eye muscles alone. C. H. O'D.

Three Lectures on Experimental Embryology. By J. W. JENKINSON, M.A., D.Sc. [Pp. xvi + 130, with 20 figures.] (Oxford: at the Clarendon Press, 1917. Price 7*s*. 6*d*. net.)

SOME of us have a vivid recollection of the delivery of these lectures a few years ago at University College, London, where Jenkinson practically laid the foundation

of his biological knowledge under Prof. Weldon, and we are grateful for this permanent record of them in what is a sort of memorial form. Although over military age he enlisted at the beginning of the war, and later received a commission in the Worcestershire Regiment. As a captain he led his men in the advance on Gallipoli on June 4, 1915, and fell in action on that day. A short biographical note by Dr. R. R. Marett and a good portrait add considerably to the value of the present book.

Jenkinson was practically the only man in this country working on experimental embryology, in which subject his own researches had shown him to be a master. The lectures were not merely a record of his own work, too modestly referred to, but also a critical account of the recent advances in other countries. They provide therefore a very useful sketch of the position of our knowledge on three topics: Growth and the Structure of the Germ-Cells, Cleavage, and Differentiation. Each is treated in the thorough manner characteristic of the author, and his powers of criticism enabled him to deal with a large amount of information in a way that is most useful to those not specialists in this branch of zoology.

A useful literature list and index are added.

The whole forms a terse but very instructive and suggestive review of the fascinating study of experimental embryology in regard to the very early stages.

C. H. O'D.

Studies in Insect Life and Other Essays. By ARTHUR EVERETT SHIPLEY, Sc.D., F.R.S. [Pp. ix + 338, with 11 illustrations.] (London: T. Fisher Unwin, Ltd., 1917. Cash price in Great Britain, 10*s*. 6*d*. net.)

UNDER the above title Dr. Shipley has collected and reprinted in book form a number of delightful and instructive articles which were previously distributed among various periodicals and scientific journals. These essays relate chiefly to zoological subjects, but are penned as much for the benefit of the general reader as for the student. The subjects are ably treated, and the lucid and fascinating style of the author will render the perusal of these essays a real enjoyment to the reader, who will obtain from them much valuable information with little effort upon his own part.

The studies in insect life are devoted to certain forms which, just now, are of particular interest owing to their increased importance in time of war; and to those wonderful creatures, the honey- and humble-bees, whose habits and social economy can never fail to evoke man's admiration. Three essays relating to maritime subjects follow—1. The romance of the ocean depths, with some account of those famous expeditions which elucidated so many of their mysteries; 2. Our sea-fisheries and the important results obtained from the investigation of them during recent years; and 3. The oceanographical reminiscences of Sir John Murray. A chapter is next devoted to the diseases (Coccidiosis and Strongylosis) and numerous parasites of that unhappy bird the Grouse. This is followed by two historical articles on science—Zoology in the time of Shakespeare, an essay dealing more particularly with the acquaintance of the subject displayed by the poet in his writings, and the Revival of Science in the Seventeenth Century. Hate, that emotion so much in evidence in these terrible days, forms the subject of the concluding essay, and is most interestingly written throughout. Of especial interest is that part which treats of the physiological conditions accompanying and perhaps producing the ultimate manifestation of this passion-rage.

H. F. C.

BOOKS RECEIVED

(Publishers are requested to notify prices)

Differential Equations. Being Part II. of Vol. II. By Édouard Goursat, Professor of Mathematics in the University of Paris. Translated by Earle Raymond Hedrick, Professor of Mathematics in the University of Missouri, and Otto Dunkel, Assistant Professor of Mathematics in Washington University. London: Ginn & Co., and Boston, New York, Chicago, Atlanta, Dallas, Columbus, and San Francisco. (Pp. viii + 300.) Price 11*s.* 6*d.* net.

The Continuum and other Types of Serial Order. With an Introduction to Cantor's Transfinite Numbers. By Edward V. Huntington, Associate Professor of Mathematics in Harvard University. Second Edition. Cambridge, Mass., U.S.A.: Harvard University Press, 1917. (Pp. vii + 82.)

Census of the Commonwealth of Australia. Appendix A. Vol. I. The Mathematical Theory of Population, of its Character and Fluctuations, and of the Factors which influence them, being an Examination of the General Scheme of Statistical Representation, with Deductions of Necessary Formulæ; the whole being applied to data of the Australian Census of 1911, and to the elucidation of Australian Population Statistics generally. By G. H. Knibbs, C.M.G., F.S.S., F.R.A.S., etc., Member of the International Institute of Statistics, Honorary Member of the Société de Statistiques of Paris, and of the American Statistical Association, Commonwealth Statistician. Published under Instructions from the Minister of State for Home and Territories, Melbourne. By Authority; McCarron, Bird & Co., Printers, 479, Collins Street, Melbourne. C.S.—No. 312. (Pp. xvi + 466.)

A Sixth List of Writings on Determinants. By Sir Thomas Muir. Extracted from the *Quarterly Journal of Pure and Applied Mathematics*, Vol. XVII., No. 188, 1917. (Pp. 344–84.)

On the Natural Occurrence in Certain Fish-Liver Oils of High Percentages of Hydrocarbons. By A. Chaston Chapman, F.I.C. Reprinted from the *Analyst*, the Journal of the Society of Public Analysts and other Analytical Chemists, May 1917. (Pp. 6.)

Introduction to the Rarer Elements. By Philip E. Browning, Ph.D., Assistant Professor of Chemistry, Kent Chemical Laboratory, Yale University. Fourth Edition, thoroughly revised. New York: John Wiley & Sons, Inc.; London: Chapman & Hall, 1917. (Pp. x + 250.) Price 7*s.* net.

A Class Book of Organic Chemistry. By J. B. Cohen, Ph.D., B.Sc., F.R.S., Professor of Organic Chemistry in the University of Leeds. London: Macmillan & Co., St. Martin's Street, 1917. (Pp. viii + 343.) Price 4*s.* 6*d.* net.

The Theory and Use of Indicators. An Account of the Chemical Equilibria of Acids, Alkalies and Indicators in Aqueous Solution, with Applications. By E. B. R. Prideaux, M.A., D.Sc., Lecturer in Chemistry in University College, Nottingham. London: Constable & Co., 1917. (Pp. vii + 375.) Price 12*s.* 6*d.* net.

Technical Handbook of Oils, Fats, and Waxes. By Percival J. Fryer, F.C.S., Soap and Glycerine Works Manager and Chief Chemist Lecturer in Oils, Fats, and Waxes at the Polytechnic, Regent Street, W., Honours Silver Medallist, City and Guilds of London, and Frank E. Weston, B.Sc., F.C.S., Head of the Chemistry Department in the Polytechnic, Regent Street, W. Vol. I., Chemical and General. Cambridge: at the University Press, 1917. (Pp. x + 279, with 33 illustrations and 36 plates.) Price 9*s.* net.

Continuity, or From Electrons to Infinity. By P. S. G. Dubash, D.Sc., Phil B., Ph.M., F.S.P., F.B.E.A., F.P.C., Vice-President of the International College of Chromatics, the Esperantic Consul for Karachi, Le Membre du Conseil du Cercle d'Etudes Ethnographiques, Le Représentant de la Société Internationale de Science, Philologie et Beaux-Arts en Karachi. Blackburn: George Toulmin & Sons, Northgate; London: Hickie, Borman & Woods, 36, Lime Street, E.C., 1917. (Pp. 60.) Price 1*s*. 6*d*. net.

Sull' Applicazione dei Metodi Psico-Fisici All' Esame dei Candidati All' Aviazione Militare. Relazione di recerche sperimentali compiute per incarico del Ministero della Guerra presentata al Congresso della Società per il progresso delle Scienze, Aprile 1917. Con 4 illustrazioni, 13 diagramma, 4 tabelle. Milano: Edizione di *Vita e Pensiero*, 1917. By Agostino Gemelli, O.F.M., Capitano medico. Premiata tipografia Agostino Colombo & Figli, Cusano-Milanino. (Pp. 38.)

Diarsenides as Silver Precipitants. By Chase Palmer. Reprinted from *Economic Geology*, Vol. XII. No. 3, April-May 1917. (Pp. 207-18.)

Significant Mineralogical Relations in Silver Ores of Cobalt, Ontario. By Edson S. Bastin. Reprinted from *Economic Geology*, Vol. XII. No. 3, April-May, 1917. (Pp. 219-36.)

Soil Conditions and Plant Growth. By Edward J. Russell, D.Sc., F.R.S., Director of the Rothamsted Experimental Station, Harpenden. Third Edition. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1917. (Pp. viii + 243, with diagrams.) Price 6*s* 6*d*. net.

A Bibliography of British Ornithography from the Earliest Times to the end of 1912. Including Biographical Accounts of the Principal Writers and Bibliographies of their Published Works. By W. H. Mullens, M.A., LL.M., F.L.S., M.B.O.U., and H. Kirk Swann. To be issued in 6 bi-monthly Parts. Part VI. Price 6*s*. each part.

The Mosquitoes of North and Central America and the West Indies. By Leland O. Howard, Harrison G. Dyar, and Frederick Knab. Vol. IV. Systematic Description (in 2 Parts). Part II. Washington, D.C.: Published by the Carnegie Institution of Washington, 1917. (Pp. 525-1064.) Baltimore: The Lord Baltimore Press, MD., U.S.A.

The Biology of Waterworks. By R. Kirkpatrick, Assistant in the Department of Zoology, British Museum, (Natural History). Economic Series, No. 7. London: Printed by order of the Trustees of the British Museum, 1917. (Pp. iv + 58, with 18 text figures.) Price 1*s*. net.

Animal Micrology. Practical Exercises in Zoological Micro-technique. By Michael F. Guyer, Ph.D., Professor of Zoology in the University of Wisconsin, President (1916) the American Microscopical Society. With a chapter on Drawing by Elizabeth A. Smith, Ph.D., Instructor in Zoology in the University of Wisconsin. Revised Edition. Chicago: The University of Chicago Press. (Pp. xi + 289.) Price 9*s*. net.

The Last Glaciation and the Submerged Forests. Reprinted from *Journal of the Torquay Natural History Society*, 1917. By Major R. A. Marriott, D.S.O. (Pp. 14.)

The Biology of Dragonflies, Odonata or Paraneuroptera. By R. J. Tillyard, M.A., B.Sc., F.L.S., F.E.S., Macleay Fellow in Zoology to the Linnean Society of New South Wales. Cambridge: at the University Press, 1917. (Pp. xii + 396, with 4 plates.) Price 15*s*. net.

Modern Man and his Forerunners. A Short Study on the Human Species, Living and Extinct. By H. G. F. Spurrell, M.A., M.B., B.Ch., F.Z.S. London: G. Bell & Sons, 1917. (Pp. xii + 192, with 5 illustrations.) Price 7*s*. 6*d*. net.

The Causes of Tuberculosis, together with some Account of the Prevalence and Distribution of the Disease. By Louis Cobbett, M.D., F.R.C.S., University Lecturer in Pathology. Cambridge: at the University Press, 1917. (Pp. xvi + 707.) Price 21*s*. net.

The Growth of Medicine from the Earliest Times to about 1800. By Albert H. Buck, B.A., M.D., formerly Clinical Professor of Diseases of the Ear, Columbia University, New York, Consulting Aural Surgeon, New York Eye and Ear Infirmary. New Haven: Yale University Press; London: Humphrey Milford, Oxford University Press, 1917. (Pp. viii + 582.) Price 21*s.* net.

Shell Shock and its Lessons. By G. Elliot Smith, M.A., M.D., F.R.C.P., F.R.S., Dean of the Faculty of Medicine and Professor of Anatomy, and T. H. Pear, B.Sc., Lecturer in Experimental Psychology. Manchester: at the University Press, 12, Lime Grove, Oxford Road; London: Longmans, Green & Co., and New York, Bombay, Calcutta, and Madras, 1917. (Pp. xi + 135.) Price 2*s.* 6*d.* net.

Organism and Environment as illustrated by the Physiology of Breathing. By John Scott Haldane, M.B., LL.D., F.R.S., Fellow of New College, Oxford. New Haven: Yale University Press; London: Humphrey Milford, Oxford University Press, 1917. (Pp. xi + 138.) Price $1.25 net.

Kala-Azar: Its Treatment. By Upendranath Brahmachari, M.A., M.D., Ph.D., Rai Bahadur, Physician in charge of the First Medical Wards, Campbell Hospital, and Teacher of Medicine, Campbell Medical School, Calcutta. With a Foreword by the Honourable Surgeon-General W. R. Edwards, C.B., C.M.G., M.D., I.M.S., Surgeon-General with the Government of Bengal. Calcutta: Butterworth & Co., 6, Hastings Street; Winnipeg, Sydney, and London: Bell Yard, Temple Bar, 1917. (Pp. vi + 123, with 6 plates and a frontispiece.) Price 8*s.* 6*d.* net.

The Thyroid Gland in Health and Disease. By Robert McCarrison, M.D., D.Sc., F.R.C.P., Lauréat de l'Académie de Médecine, Paris, Major Indian Medical Service. London: Baillière, Tindall & Cox, 8, Henrietta Street, Covent Garden, 1917. (Pp. xvii + 286, with 82 figures in the text.)

The Distribution of Attention. By E. Neil McQueen, M.A., D.Sc. *The Journal of Psychology*, Monograph Supplements V. Cambridge: at the University Press, 1917. (Pp. vii + 142.) Price 5*s.* net.

Le Mensonge du 3 Août 1914. Paris: Librairie Payot & Co., 106, Boulevard Saint Germain, 1917. (Pp. viii + 397.) Price 5 francs.

Les Déportations Belges à la lumière des Documents Allemands, avec de nombreux fac-similés et la reproduction de tous les documents belges. Paris: Berger-Levrault, Libraires-Editeurs, 1917. (Pp. xx + 435.) Price 7 francs 50.

The War and the Nation: A Study in Constructive Politics. By William Cecil Dampier Whetham, F.R.S., Fellow and Senior Tutor of Trinity College, Cambridge. London: John Murray, Albemarle Street, W., 1917. (Pp. viii + 312.) Price 6*s.* net.

Science and Industry. A Series of Papers bearing on Industrial Research. No. I. Industrial Research in the United States of America. By A. P. M. Fleming, M.I.E.E. London: Published for the Department of Scientific and Industrial Research by His Majesty's Stationery Office, 1917. (Pp. viii + 60, with 85 plates.) Price 1*s.* net.

Report on the Resources and Productions of Iron Ores and other Principal Metalliferous Ores used in the Iron and Steel Industry of the United Kingdom. Advisory Council. Department of Scientific and Industrial Research. Printed by His Majesty's Stationery Office, 1917. (Pp. 145.) Price 2*s.* 6*d.* net.

The *Athenæum* Subject-Index to Periodicals, 1916. Issued at the Request of the Council of the Library Association. Theology and Philosophy. *The Athenæum*, Bream's Buildings, Chancery Lane, E.C. New York: B. F. Stevens & Brown, 16, Beaver Street; London: 4, Trafalgar Square. June 1917. (Pp. 48.) Price 2*s.* 6*d.* net.

Science and Industry. The Place of Cambridge in any Scheme for their Combination. The Rede Lecture, 1917. By Sir Richard T. Glazebrook, C.B., M.A., F.R.S., Fellow of Trinity College, Director of the National Physical Laboratory. Cambridge: at the University Press, 1917. (Pp. 51.) Price 1*s*. 6*d*. net.

Higher Education and the War. By John Burnet, Officier de l'Instruction publique, Dean of the Faculty of Arts in the University of St. Andrews. London: Macmillan & Co, St. Martin's Street, 1917. (Pp. 238.) Price 4*s*. 6*d*. net.

The Organisation of Thought, Educational and Scientific. By A. N. Whitehead, Sc.D., F.R.S., Fellow of Trinity College, Cambridge, and Professor of Applied Mathematics at the Imperial College of Science and Technology. London: Williams & Norgate, 14, Henrietta Street, Covent Garden, W.C., 1917. (Pp. vii + 228.) Price 6*s*. net.

Spiritualism and Sir Oliver Lodge. By Charles A. Mercier, M.D., F.R.C.P., F.R.C.S. London: The Mental Culture Enterprise, 329, High Holborn, W.C., 1917. (Pp. x + 132.)

The Art of Living Long. A New and Improved English Version of the Treatise by the celebrated Venetian Centenarian Luigi Cornaro. With Essays by Joseph Addison, Lord Bacon, and Sir William Temple. Milwaukee: William F. Butler, 1917. (Pp. 214, with 4 plates.)

First and Last Things. A Confession of Faith and Rule of a Life. By H. G. Wells. Revised and Enlarged Edition. London: Cassell & Co., Ltd., New York, Toronto, and Melbourne, 1917. (Pp. xviii + 233.) Price 6*s*. net.

The Public School System in relation to the coming conflict for National Supremacy. By V. Seymour Bryant, M.A., with a Preface by Lord Raleigh. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1917. (Pp. xviii + 78.) Price 1*s*. 6*d*. net.

British Boys: Their Training and Prospects. By Colonel M. J. King Harman. London: G. Bell & Sons, 1917. (Pp. viii + 254.) Price 2*s*. net.

Saber and Song. A Book of Poems. By William Thornton Whitsett. Whitsett, North Carolina: Whitsett Institute, 1917. (Pp. vi + 156.) Price $1.25.

Folklore di Guerra, per uno studio sistematico della psicologia del soldato. By Agostino Gemelli, O.F.M., Capitano medico. Estratto da *Vita e Pensiero*, 1 Gennaio 1917. Premiata tipografia Agostino Colombo & Figli, Cusano-Milanino, 1917. (Pp. 11.)

The Principles of Rational Education. By Charles A. Mercier, M.D., F.R.C.P., F.R.C.S., late Examiner in Psychology in the University of London. London: The Mental Culture Enterprise, 329, High Holborn, W.C., 1917. (Pp. xi + 87.) Price 2*s*. 9*d*. net.

The Ideal Nurse; an Address to Nurses, delivered to the Nursing Staff of the Retreat at York, at the Opening of the Winter Session, 1909. By Charles A. Mercier, M.D., F.R.C.P., F.R.C.S., at that time Chairman of the Education Committee of the Medical Psychological Association. London: The Mental Culture Enterprise, 329, High Holborn, W.C. (Pp. 49.) Price 1*s*. 3*d*. net.

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By PHILIP E. B. JOURDAIN, M.A., Cambridge.

Various.—The Presidential Address to the Mathematical Association, delivered in January 1917, was on the essential nature of technical education and its relation to a liberal education. It was by A. N. Whitehead (*Math. Gaz.* 1917, **9**, 20–33, and on pp. 29–57 of the book on *The Organisation of Thought*, reviewed elsewhere in the present number), and the following sentences are very characteristic of British logical work : " The thought which science evokes is logical thought. Now logic is of two kinds, the logic of discovery and the logic of the discovered. . . . The logic of the discovered is the deduction of the special events which under certain circumstances would happen in obedience to the assured laws of nature. . . . Without deductive logic science would be entirely useless.".

The number of the *Gazette* (March 1917) which contains this Address contains also some important papers on the teaching of mathematics by P. Abbott, C. J. L. Wagstaff, W. J. Dobbs and J. L. S. Hatton, which were read at the same Annual Meeting of the Mathematical Association.

The same *Gazette* has lately added two interesting features to its appearance, and both are due to the wonderful literary research of its editor, W. J. Greenstreet. One is a series of articles on some incidental writings of De Morgan ; and the other is sets of " Gleanings from Far and Near," the object of which is much too modestly stated to be to fill up space at the ends of articles so that each article may begin at the top of a fresh page.

G. Loria (*Boll. della Mathesis*, 1916, 32–9) discusses the tasks of mathematical symbols.

History.—L. C. Karpinski (*Amer. Math. Monthly*, 1917, **24**, 257–65) tries to show that much of the material of our elementary algebra was long ago anticipated, to some extent, by the Egyptians and Babylonians. Without doubt there seems, to the

reviewer, to be a tendency with some people to disparage the work of earlier civilisations as compared with that of ancient Greece; but there is no doubt that strictly deductive science was first founded by the Greeks.

There is an interesting article by A. Favaro on apocryphal writings of Galileo in the *Boll. di Bibl. e St. delle Sci. Mat.* (1917, **19**, 33–43).

W. H. Bussey (*Amer. Math. Monthly*, 1917, **24**, 199–207) gives a very useful account of that part of the work of Maurolycus on arithmetic (1575) which deals with the method of complete induction. This paper serves to correct a minor error into which Moritz Cantor fell when attributing, in an article of 1902, the use of the method to Maurolycus instead of to Pascal, as he had in 1900 in the latest edition of the second volume of his *Geschichte*.

H. S. Carslaw (*Journ. of Proc. Roy. Soc. N.S. Wales*, 1916, **50**, 130–42) suggests that there are three distinct stages in the development of Napier's idea of a logarithm. In the first he was concerned with a one-one correspondence between the terms of a geometrical progression and those of an arithmetical progression, and there are traces of this in the *Constructio* and in the derivation of the word "logarithm." In the second he has passed from this correspondence, and his logarithms are given by the well-known kinematical definition which forms the foundation of the theory of the *Constructio*. In the third, referred to in the Appendix to the *Constructio*, he reached the idea of a logarithm as defined by the property that the logarithms of proportional numbers have equal differences, with the additional condition that the logarithms of two numbers are given. Further, it is shown that logarithms to the base 10—as we know them—are Napier's logarithms just as much as the logarithms of his *Canon*. Cf. SCIENCE PROGRESS, 1916, **10**, 434.

G. Milhaud (*Rev. gén. des Sciences*, 1917, **28**, 332–7) discusses the attitude of Descartes in 1637 and 1638 to the *De maximis et minimis* which Fermat sent him at the end of 1637. Although Descartes's criticisms were by no means profound, his attitude may possibly be explained, not merely by the probability that he was offended by Fermat's interference with the subject (of tangents to curves) of which he was particularly proud, but also because he felt that Fermat did not proceed with quite the generality that seemed to him necessary. This

more or less conscious intellectual need is creditable to Descartes, though he was mistaken in thinking that Fermat's method was not general. Milhaud also (*ibid.* 464–9) gives several examples from the correspondence of Descartes to show how naturally and easily he worked with infinitesimal considerations, to show that, in opposition to the opinions of some, Descartes was by instinct accessible to all new methods and ideas in mathematics.

L. E. Dickson (*Annals of Math.* 1917, **18**, 161–87) gives a most detailed and important paper on Fermat's theorem that $x^n + y^n = z^n$ has no integral solutions different from zero when n is greater than 2, and the origin and nature of the theory of algebraic numbers. The first part of the paper contains the early history of the theorem mentioned and a summary of the more important results concerning it which have been proved without the theory of algebraic numbers; the second part enables the reader to obtain a clear insight into the origin, nature and use of ideals; and the third part is on those papers which treat Fermat's theorem by means of algebraic numbers.

As a sequel to his study of the enigma of imaginary numbers (cf. SCIENCE PROGRESS, 1917, **12**, 6), Gino Loria (*Scientia,* 1917, **22**, 1–13) shows that geometry in the course of its evolution was continually meeting a quite analogous conception which passed through several stages of development strikingly like those which Loria brought out in the case of the evolution of the treatment of imaginary numbers. In this article most attention is paid to the work of Carnot, Monge, Poncelet, Chasles, Steiner, and von Staudt.

The most interesting of the instalment of letters between Schläfli and Casorati published in the first number of the *Boll. di Bibl. e St. delle Sci. Mat.* for 1917 (**19**, 9–14) is an animated letter in which Schläfli gives an extension of a method of his to prove the existence of Abel's integrals. Some correspondence between Schläfli and Cremona from 1867 to 1887 is also given (*ibid.* 43–9).

Sir Thomas Muir (*Quart. Journ. of Math.* 1917, **47**, 344–84) gives a sixth list of writings on determinants in continuation of his former lists published in the same *Journal.* The present list is concerned mainly with the five years following 1910, but gives also additional titles for all the periods specially covered

by its predecessors. In the present state of mathematical recording it is only by repeatedly looking back in this way that any close approximation to completeness can be secured. The valuable Catalogue issued by the Mathematical Association is mentioned with appreciation, and a suggestion is made that it should be brought up to date at intervals of a few years. This paper is a further step towards the very valuable and rigorously full treatment of that part of mathematical history to which Muir has devoted himself.

Logic and Principles of Mathematics.—P. J. Daniell (*Bull. Amer. Math. Soc.* 1917, **23**, 446–50) introduces a new concept into the calculus of classes (logical classes or sets of points) which is analogous to the modular difference of two numbers. The "modular difference of two classes" is defined by a direct logical operation on the classes; some properties are proved, limiting classes of sequences are defined, and some properties of these limits are proved which are of great interest in connection with Borel's work in his *Théorie des Fonctions des Variables réelles*, and, though this is not mentioned, with the much earlier work of Cantor on derivatives of infinite order. However, where the *existence* of limits has to be proved, Daniell's paper does not seem complete.

E. V. Huntington and J. R. Kline (*Trans. Amer. Math. Soc.* 1917, **18**, 301–25) give various sets of independent postulates for "betweenness" which are selected from the twelve postulates due essentially to Pasch in 1882, and show the characteristic properties of the type of system represented by points on a line by which this type is distinguished from that of any other system of elements related by a triadic relation.

A. Padoa (*Rev. de Métaphys. et de Morale*, 1917, **24**, 315–25) writes on the consequences of a change of primitive ideas in any deductive theory.

G. Peano (*Boll. della Mathesis*, 1915, 1–15) describes the principal definitions by abstraction which are met with in mathematical books: ratios, cardinal numbers, real numbers and vectors. The objection that the sign of equality is defined again by the above definition is not valid, according to the author, because the above sign is not defined, "but the whole equality composed of the two numbers together with this sign." The method used by Russell of replacing such definitions by nominal definitions is called the "theory of classes," and the

author says that we cannot urge logical objections against this theory of classes, " but only practical ones, because the language in use is modified. Many mathematicians will find it a novelty to write ' 1 = proper fraction ' to say that unity is the upper limit of proper fractions " (p. 7 ; cf. p. 13).

A manuscript of the late Louis Couturat on symbolic logic and the calculus of probabilities is printed in the *Rev. de Métaphys. et de Morale* for May, 1917 (**24**, 291–313). This manuscript dates from before 1902 and will not be reprinted in the forthcoming *Manuel* of his (cf. SCIENCE PROGRESS, 1917, **12**, 6).

A. N. Whitehead (*Proc. Aristot. Soc.* 1916, **16**, 104–29, and pp. 191–228 of *The Organisation of Thought* reviewed elsewhere in the present number) discusses the theories of space, time, and relativity, and brings into relation with one another the standpoints of mathematical physics, experimental psychology, metaphysics, and mathematics. It will be seen that this paper continues on the same lines Whitehead's important work. Sir Joseph Larmor (*Proc. Aristot. Soc.* 1916, **16**, 130–32) has a short article entitled, " Relativity : A New Year Tale."

B. Petronievics (*Rend. della R. Accad. dei Lincei*, 1917, **26**, 309–16) tries to show that Fontenelle's " attempt at a rational theory of infinite numbers " had an " historical value " in that it may have affected the ideas of Cantor and Veronese. There do not, however, seem to the reviewer to be any grounds whatever for this supposition, and it is and has always been recognised that Fontenelle's theory is fundamentally unsound and quite different in its nature from, at least, Cantor's theory.

Arithmetic, Theory of Numbers, and Algebra.—G. Peano (*Rend. della R. Accad. dei Lincei*, Rome, 1916, **25**, 8–14) gives the rules for numerical approximations under an elementary form without presupposing the differential calculus ; and then gives from these rules the form of the rules of derivation.

G. H. Hardy and S. Ramanujan (*Proc. Lond. Math. Soc.* 1916, **16**, 112–32) prove a theorem on the distribution of integers of various types by an interesting method. The theorem includes the " highly composite " numbers studied by Ramanujan (*ibid.* 1915, **14**, 347–409 ; see SCIENCE PROGRESS, 1916, **10**, 434).

C. Burali-Forti (*Il Pitagora*, 1917, **1**, 1–6) gives a new definition of the complex numbers of algebra.

Sir Thomas Muir read to the Royal Society of South Africa on April 18, 1917 (*Nature*, 1917, **99**, 360), a note on the expan-

sion of the product of two oblong arrays, in which the form of the expansion given is an aggregate of single determinants. The relation with the form given by Binet and Cauchy in 1812 as a sum of products of pairs of determinants is explained, and an historical remark added.

W. A. Manning (*Amer. Journ. Math.* 1917, **39**, 281–310) discusses primitive groups of class 15.

E. Kircher (*ibid.* 272–80) studies some of the properties of a finite algebra whose elements combine by addition and multiplication, subject to the commutative, associative and distributive laws. The paper is closely related to investigations by H. S. Vandiver (1912), L. E. Dickson (1905) and A. Fraenkel (1915).

Analysis.—P. J. Daniell (*Amer. Math. Monthly*, 1917, **24**, 109–13) gives three rules of quadrature developed from Euler's summation formula, of which two seem to be new.

E. V. Huntington (*ibid.* 271–5) starts from the ordinary "intuitive" process used by applied mathematicians in "setting up" an integral—for example, in the problem of finding the attraction of a rod—and finds that a sufficient condition that the process referred to leads to a correct result is that the functions in the integrand are continuous ; it is not necessary to consider any questions of uniformity. The condition seems very important from the point of view of principles. Thus the theorem known as "Duhamel's" can be dispensed with—a fortunate circumstance, as the theorem is sometimes false. The paper is connected with papers by W. F. Osgood (1903), R. L. Moore (1912), G. A. Bliss (1914), and an interesting suggestion by a student (B. Graham, *ibid.* 265–71).

E. B. Van Vleck (*Trans. Amer. Math. Soc.* 1917, **18**, 326–30) shows that a general proof of the "momental" theorem in integration of Haskins and Dunham Jackson (see SCIENCE PROGRESS, 1916, **11**, 268) can be obtained by reduction to Stieltjes's theorem of moments. Incidentally it appears that every Lebesgue integral can be thrown into the form of a Stieltjes integral.

W. H. Young (*Proc. Lond. Math. Soc.* 1917, **16**, 175–218) defines a new kind of integral which is non-absolutely convergent and not necessarily continuous. Absolutely convergent integrals, taken with respect to discontinuous functions of bounded variation, are in general discontinuous functions ;

also the extension of these integrals so as to embrace the case where the convergence is not absolute will not have the effect of removing the discontinuity of the integral, even if we adopt the postulates formulated by Harnack and E. H. Moore, supposing that these conditions are modified so as to take account of the work of Lebesgue and the results of the theory of integration with respect to discontinuous functions. Young, confining himself for simplicity to the case in which the function with respect to which we integrate is the variable itself, considers certain generalisations of the usual processes in dealing with functions which possess Harnack points, so that a Lebesgue integral does not exist. With these generalisations it is to be noticed that the process of integration by parts is allowable and the second theorem of the mean is valid.

Young (*Proc. Roy. Soc.* 1917, A, **93**, 28–41) gives a short account of some of the formulæ which are fundamental in the modern theory of multiple integrals, integration being with respect to a function of bounded variation of two variables.

B. H. Camp (*Amer. Journ. Math.* 1917, **39**, 311–34) discusses multiple integrals over infinite fields, and shows that, for functions whose integrals over finite fields exist, the three definitions of J. Pierpont (1906), C. de la Vallée-Poussin, and G. H. Hardy (1902) are equivalent when any one of them exists absolutely. Thus Hardy's concept is more general, since it alone of the three does not presuppose absolute existence. Camp makes certain applications to Fourier's double and quadruple integrals.

W. F. Osgood (*Bull. Amer. Math. Soc.* 1917, **23**, 404), communicates a theorem on the singular points of analytic transformations which will shortly be published elsewhere.

R. L. Borger (*Bull. Amer. Math. Soc.* 1917, **23**, 287–90) proves a theorem about two real functions of the real variables x and y which satisfy conditions immediately suggested by Riemann's conditions of the analytic character of a function of a complex variable. Borger's theorem is proved without integration and by means of a theorem of Kowalewski, and an immediate consequence of it is that, if any function of a complex variable possesses a finite derivative at each point of a simply connected closed region, then this derivative is continuous, all the derivatives of the function exist, and the function is developable in a power series.

The proof of the existence of the inverse of an analytic function is made to depend, in Weierstrass's theory, on representation by power series, and, in Cauchy's theory, on the Jacobian of the real and imaginary parts of the function with reference to the real and imaginary parts of the variable. S. Beatty (*ibid.* 347–53) proves this existence starting from Goursat's conception of an analytic function, and in the method of the proof the theory of sets of points is used.

Beatty (*Amer. Journ. Math.* 1917, **39**, 257–62) derives the complementary theorem in the theory of algebraic functions from the Riemann-Roch theorem. In the work of J. C. Fields (1916) several proofs of the complementary theorem, of which the Riemann-Roch theorem is a particular case, are given.

K. B. Madhava (*Journ. of Indian Math. Soc.* 1917, **9**, 141-8) derives some interesting formulæ connected with the Zeta function, and, in particular, its addition-theorem and a generalisation of it.

W. H. Young (*Proc. Roy. Soc.* 1917, A, **93**, 42–55) obtains theorems including as particular cases the results on the order of magnitude of the coefficients of a Fourier series obtained by Lebesgue in 1910.

In 1899 the late Willard Gibbs pointed out without proof a hitherto unobserved phenomenon in the behaviour of the approximation curves for a particular Fourier's sine-series at a point of discontinuity, and Gibbs's work has been developed by Bôcher in 1906 and Gronwall in 1912. H. S. Carslaw (*Amer. Journ. Math.* 1917, **39**, 185–98) obtains a number of interesting properties of the approximation curves of another particular sine-series by quite simple methods, and shows that all the features of Gibbs's phenomenon follow immediately from these properties. In conclusion, the extension to the general case of Fourier's series is given, but the method does not differ materially from that of Bôcher.

G. N. Watson (*Proc. Lond. Math. Soc.* 1917, **16**, 150–74) investigates the theory of Kapteyn series—series in which the terms are multiples of Bessel functions—in which the work of Nielsen (1904), Debye (1909, 1910), and others is taken as starting-point.

Sir Joseph Larmor (*ibid.* 8–42), starting from the question as to the possibility of mathematical representation of such irregular graphs as those in the vast collections of meteoro-

logical, statistical, and other observations, discusses from a practical point of view the Fourier harmonic analysis and gives some optical illustrations. "The tendency of recent abstract analysis on related matters has been to explore the general qualities of the various types of infinite assemblages, rather than to determine the quantitative relations of average or mean which offer themselves for the purpose of physical theory, when the material is too complex for study in detail" (p. 37). In some of the notes added at the end of the paper (pp. 40–2) points of purely mathematical interest are dealt with.

J. W. Nicholson (*Nature*, 1917, **100**, 15–16) gives an account of, with some reflections on, the Addresses on connected subjects of E. W. Brown to the American Mathematical Society and this one of Sir Joseph Larmor to the London Mathematical Society in 1916. Brown's Address, which was on the relations of mathematics to the natural sciences, indicated the types of work really needed by the pure mathematician in this respect.

P. R. Rider (*Amer. Journ. Math.* 1917, **39**, 241–56) treats the problem of the calculus of variation for three dimensions in a different form from Gernet (1902) and Bliss and Mason (1908), and uses the results in considering certain generalised definitions of angle and solid angle (cf. SCIENCE PROGRESS, 1917, **12**, 11).

A. Dresden (*Trans. Amer. Math. Soc.* 1917, **18**, 373–8) uses methods analogous to those in a paper of his published in 1916 (see SCIENCE PROGRESS, 1917, **12**, 11) to obtain formulas for the second derivatives of the extremal-integral arising in the theory of the integral $\int F(y\,;\,y')dt$.

E. W. Chittenden (*Amer. Journ. Math.* 1917, **39**, 263–71) studies certain relations treated by Hildebrandt (1912), in his paper on the functional calculus of Fréchet.

Some recent papers by F. L. Hitchcock may be mentioned: "A Classification of Quadratic Vector Functions" (*Proc. Nat. Acad. Sci.* Washington, 1915, **1**, 177–83), "Quaternion Investigation of the Commutative Law for Homogeneous Strains" (*Proc. Roy. Soc. Edinburgh*, 1915, **35**, 170–80), "On the Operator Nabla in Combination with Homogeneous Functions" (*Phil. Mag.* 1915, **29**, 700–8), and "A Classification of Quadratic Vectors" (*Proc. Amer. Acad. Arts and Sciences*, 1917, **52**, 369–454).

In papers of 1885 and 1886, Poincaré showed that a solution

of a certain kind of linear homogeneous ordinary differential equation is in general asymptotic, in the sense of Poincaré, to a diverging power series in a sector of the complex plane in which it is analytic, the asymptotic representation being valid for the variable approaching infinity in an appropriate direction. A step towards the ideal of obtaining a single expansion of such character that it is capable of exhibiting the asymptotic properties of a function near infinity and of yielding at the same time a convenient and workable representation of it in the finite part of the plane is given by factorial series and certain immediate generalisations of them (Nörlund, 1914). In 1916, R. D. Carmichael (*Trans. Amer. Math. Soc.* 1916, **17**, 207–32; SCIENCE PROGRESS, 1917, **11**, 456–7) pointed out that such factorial series are instances of a large class of series with simple properties, and in the *Bull. Amer. Math. Soc.* for 1917 (**23**, 407–25) he showed how several important series in mathematical literature are included as special cases of series of the form just referred to, and in which the nth term is a constant multiplied by $g(x+n)/g(x)$, where $g(x)$ is a given function of x. A further generalisation of this series is also considered.

H. S. Carslaw (*Proc. Lond. Math. Soc.* 1917, **16**, 84–93) shows, in a second paper on the Green's function for Poisson's equation, that, with the same assumptions as are involved in the usual treatment of the differential equations of mathematical physics, the Green's functions in question can be obtained immediately from an integral equation, when the region with which we are dealing is finite.

G. B. Jeffery (*ibid.* 133–9) shows that Whittaker's general solution of Laplace's equation provides a ready means of expressing a given potential function in terms of different harmonics—spherical, cylindrical, and spheroidal.

Geometry.—J. E. Rowe (*Bull. Amer. Math. Soc.* 1917, **23**, 405–7) investigates the projection of a line section upon a rational plane cubic curve.

A. B. Coble (*Trans. Amer. Math. Soc.* 1917, **18**, 331–72) gives the third part of his investigations on point sets and allied Cremona groups. The first two parts were published in the same *Transactions* for 1915 and 1916.

F. R. Sharpe and V. Snyder (*ibid.* 402–14) obtain a classification of the possible (2, 2) point correspondences between two planes, and describe the important features of each type.

L. Bianchi (*ibid.* 379–401) solves the problem of determining all the cases for which a certain property relating to what the author calls the "facettes" of surfaces applicable to quadrics, holds true in all deformations of the surfaces S in question. In this part of the theory of transformations of deforms by flexure is an extension of the concepts introduced by Sophus Lie in his researches on the transformations of surfaces of constant curvature.

ASTRONOMY. By H. SPENCER JONES, M.A., B.Sc., Royal Observatory, Greenwich.

Stellar Evolution.—In these notes reference was made recently (SCIENCE PROGRESS, **12**, 14, 1917) to a paper by Mr. J. H. Jeans on "The Part Played by Rotation in Cosmic Evolution," the conclusions arrived at being that the existence of binary systems, of spiral nebulæ and possibly also of ring nebulæ, could be explained by evolution from a rotating mass of gas, but that it was impossible to explain in this way the existence of our solar system. This paper should be studied in conjunction with a cognate paper by Mr. Jeans, "The Motion of Tidally Distorted Masses, with special reference to Theories of Cosmogony" (*Mem. R.A.S.* **72**, pt. i. 1917), in which the tenability of the tidal theory of planetary evolution is discussed. This theory supposes the near approach of two masses of matter. Each by itself, in the absence of rotation, will assume the spherical form, but the near approach of the two masses will raise tides in them which will have their maximum amplitudes at the ends of the diameters on the common axis. Thus, at any rate at first, the bodies will assume a spheroidal form, just as a rotating mass does. The problem is to determine whether a stage is at length reached at which instability will occur and one of the bodies throw off a satellite. The discussion is again based on Poincaré's theory of linear series and points of bifurcation. Considering first the case in which the primary is homogeneous and incompressible, it is found that, if the tidal action causes the eccentricity to exceed a certain definite value, instability sets in and the primary rapidly changes its shape; if the tidal forces diminish sufficiently quickly, the primary may regain stability and gradually sink back into a spherical form. Otherwise, furrows will develop

in it, and the mass may ultimately break up into several parts. If the mass is incompressible, the cataclysm does not occur so readily; with a homogeneous incompressible gas, it will not occur at all. Mr. Jeans divides such encounters of two masses into two classes, transitory and lasting, the former being those in which the tidal forces come and go before the major axis of the spheroid increases by more than 10 per cent. Lasting encounters are extremely rare; for the stellar universe as a whole, and excluding star clusters, they occur on the average about once in 4×10^{18} years. It follows that the large proportion of binary stars in the sky cannot be explained by lasting encounters. On the other hand, a transitory encounter will not produce a binary system, unless the initial density was below about 10^{-19}. This value is so improbably small that it seems that in general binaries cannot have been formed by tidal action.

Similar conclusions hold for nebulæ, and in addition it is found that tidal action does not in any case give rise to the well-known spiral form, but rather to a curve of a boomerang shape. As regards the solar system, although it is possible to conceive it as having been produced by intense tidal action, the odds are very heavy against this being so. If formed in this way, our system must be regarded as a sort of freak in the sky; as far as we are aware, it is so, and since its existence cannot be explained on the rotational hypothesis, it does not seem unreasonable to suppose it to have been formed in this way. The most probable conclusion to be drawn from Mr. Jeans's investigations is, therefore, that binary systems and spiral nebulæ, in general, have evolved from a mass of rotating gas, and that our solar system has probably been evolved from a gaseous mass by the intense tidal actions of another mass.

The Aurora Borealis.—The corpuscular theory of the aurora first advanced by Goldstein and subsequently developed by Paulsen and Birkeland, supposes that the sun is continually emitting into space charged particles, somewhat of the nature of cathode rays. Such of these as enter the magnetic field of the earth are converged by the action of the latter towards the earth and, entering the upper layers of the atmosphere either directly, or indirectly through the production of secondary cathode rays, produce the luminescence observed as the aurora. The theory has the observational support that the curve showing

the frequency of auroral displays runs parallel to that representing the solar activity as evidenced by the sun-spots and prominences. The theory has been investigated theoretically by Carl Störmer, and a summary of the results so far obtained is given by him in *Terrestrial Magnetism*, **22**, 23 and 97, 1917.

The problem is a difficult one to attack mathematically, and, for simplicity, the assumptions were made that the earth is a uniformly magnetised sphere, that no force other than the earth's magnetic force acts on the particles, and that their velocity is much greater than those of the earth and sun, so that the relative motion of the latter can be neglected. The investigation has involved the tracing, by numerical and graphical methods, of an immense number of different orbits of particles; it has been continued since 1903, and has included two expeditions to Bossekop in 1910 and 1913, for auroral observations, determination of heights of auroral curtains, etc.

The theory, combined with the fact that the earth's magnetic axis does not coincide with its axis of rotation, has been found adequate to explain the following auroral phenomena: There are southern and northern limits for the aurora borealis and aurora australis respectively, beyond which auroræ seldom or never occur; the large majority of auroral displays occur within two well-defined belts; they are of a sudden and variable character; they show a tendency to recur about the same time on two successive days and also after about twenty-seven days, the period of the sun's rotation; auroræ occur at the greatest distance from the earth's magnetic axis at the time of intense magnetic storms. The existence of several auroral curtains at the same time can be attributed to the charged particles possessing several different velocities.

The theory, therefore, explains satisfactorily most of the phenomena observed. There is one outstanding difficulty—the situation of the belts of maximum auroral frequency. These are about 23° from the magnetic axis. The theory gives only 6° for β-rays of radium and 18° for α-rays, so that it would appear that negatively charged particles cannot account for the aurora. It is possible that it is due entirely to the positively charged α-particles. Carl Störmer suggests alternatively that since theory indicates that a large number of particles bend round the earth on the afternoon and night side, the belt so formed may be of sufficient intensity to draw the position of

maximum frequency down to that observed : investigation of this point shows that, without any great impossibility, this might happen even for negatively charged particles, and that the belt required for this purpose will not exert an appreciable magnetic force on the earth's surface. Further investigation may be expected to throw more light on this point.

The Temperature of Space.—It is not generally realised that the notion of temperature in free space has in itself no meaning. In order to make the matter definite we must suppose a small test body introduced, and we can then define the temperature of free space as the temperature of thermal equilibrium of this body, in accordance with our usual method of defining temperature. M. Ch. Fabry, in *Astroph. Journ.* **45**, 269, 1917, points out that matters are not so simple as they appear, for the temperature attained by the test body will depend on its emissive and absorptive powers at various wave-lengths. The temperature attained may vary enormously, and be either lower or higher than that attained by a black body. If the body absorbs only long wave-lengths its temperature will be low; if only short wave-lengths, it will be high. Thus, supposing the radiation is produced by the sun (taken as equivalent to a black body at temperature 6,000°), and that the testing body is at the earth's distance and has one absorption band, then the temperature attained by it will be 1980° if the absorption band has wave-length 0·4 microns, and only 130° if the wave-length is 10 microns. It is, therefore, quite possible that gaseous masses may have selective absorption for short wave-lengths and will therefore reach a high temperature. The high temperature found by M. Fabry for the Orion nebula may thus be explained, as also may the emission spectra of comets' tails.

The Dependence of Stellar Motions on Absolute Magnitudes.—A paper of the highest importance by W. S. Adams and G. Strömberg dealing with this subject has appeared in the *Astroph. Journ.* **45**, 293, 1917. It is well known that stars of large proper-motion have high velocities in space. This may be due either (1) to the fainter stars moving more quickly than the brighter, or (2) to the nearer stars moving more quickly than those more distant. These two alternatives are investigated with the aid of hypothetical parallaxes determined by Adams's method from the spectral characteristics. It is found, in the first place, that

the radial velocity is a function of the absolute magnitude, increasing by about 1·5 kms. for a decrease in brightness of one magnitude. Further investigation shows that this result is not a distance effect, for stars at the same distance from the sun exhibit the effect; that it is not dependent upon the particular frequency law assumed by the velocities, as the component velocities calculated under no assumption as to the nature of law also show the effect; and finally it is found that the K and M stars have mean velocities about 1·0 or 1·5 kms. higher than the F and G stars of the same absolute magnitude. Thus the dependence of stellar velocities upon type, discovered by Campbell, is shown to be a comparatively secondary phenomenon, the greater part of his observed results being attributable to the magnitude phenomenon.

The average mass of the bright stars is greater than that of the intrinsically fainter stars, and the result found in this paper is therefore in agreement with what would be anticipated on the kinetic gas theory of the Universe, assuming approximate equipartition of energy.

The following is a selection from amongst the most important papers recently published:

The Solar System.—MAUNDER, MRS. A. S., Sun-spots in a High Solar Latitude, *M.N., R.A.S.*, **77**, 621, 1917. This paper announces the discovery at the Royal Observatory, Greenwich, of small short-lived spots, in very high solar latitudes, beyond the previously accepted limits.

ROSS, F. E., New Elements of Mars, *Ast. Papers of the American Ephemeris*, **9**, pt. ii. 1917. Newcomb's table for Mars showed large discrepancies in R.A. Ross has traced this to Newcomb's adopted eccentricity being too small. In the paper the mass of Venus is also deduced, from the variations it produces in Mars and the Earth. The value obtained is 1/(403,490+2,400).

ROSENBAUM, L., Détermination de l'Orbite de la Comète 1915 *a* Mellish, *Astron. Jakttag. Och. undersök ön Stockholms Observatorium*, **10**, No. 5, 1917.

SVARDSON, J., Détermination de l'Orbite de la Comète 1914 *c* Neujmin, *ibid.* 10, No. 6, 1917.

LODGE, SIR OLIVER, Astronomical Consequences of the Electrical Theory of Matter, *Phil. Mag.* **34**, 81, 1917.

Eddington, A. S., Note on Sir Oliver Lodge's Suggestions, *Phil. Mag.* **34**, 163, 1917.

Lodge supposes that the extra electrical inertia due to the motions of matter is not subject to gravitation. The velocity of a planet relative to the sun is compounded with that of the motion of the solar system as a whole through space, which gives an inertia alternatively increasing and decreasing. This leads to changes in the perihelion and eccentricities of the orbits. The anomaly in the motion of the perihelion of Mercury might thus be explained. Eddington shows that, if this is so, other anomalies in the motion of the Earth and Mercury would be introduced, so that the suggestion is untenable.

Variable and Binary Stars.—Shapley, Martha B., Light Curves and Orbital Elements of *TT* Lyræ and γ Camelop., *Astroph. Journ.* **46**, 56, 1917.

Baker, R. H., The Eclipsing Binary *Z* Vulpec., *Laws Obs. Bull.* No. 26, 1916, The Eclipsing Binary ω Herculis, *Laws Obs. Bull.* No. 28, 1917.

Cummings, Edith H., The EclipsingBinary, . Cassiop., *Laws Obs. Bull.*, No. 27, 1917.

Wilson, H. C., Light Curve of *T* Androm., *H.A.* **80**, No. 8, 1917.

Russell, H. N., Fowler, M., and Boston, M. C., Comparisons of Visual and Photographic Observations of Eclipsing Variables, *Astroph. Journ.* **45**, 306, 1917.

Osthoff, H., Bemerkungen zu Argelander's Methode der Schätzen's der Sternhelligkeiten, *Ast. Nach.* 4897–98, 1917. A valuable discussion of the various sources of error, mainly of a physiological nature, which are liable to occur in the use of Argelander's step method of comparing the brightness of stars.

Lau, H. E., Untersuchungen über die Farben der Fixsterne, *Ast. Nach.* 4900–01, 1917.

Stellar Distribution, etc.—Miller, J. A., Determination of the Parallaxes of 50 Stars, *Sproul Obs. Pubs.*, No. 4, 1917. This paper contains the first series of parallax results obtained at the Sproul Observatory together with a description of the instrument and of the methods employed. A 24-inch refractor is employed and the method of determination follows generally the photographic method as employed by Schlesinger. The

results are of the high order of accuracy now generally obtained with the photographic method. The programme includes all visual binaries with well-determined orbits, some spectroscopic binaries, some objects whose hypothetical parallaxes are large, some stars with well-determined parallaxes (for comparison with other determinations), a few stars with large proper-motions or whose determined parallax values are discordant and a few objects of special interest.

Dyson, Sir F. W., and Thackeray, W. G., The Systematic Motions of the Stars between Dec. +24° and Dec. +32°, *M.N., R.A.S.* **77**, 581, 1917. This is a study of about 12,000 proper-motions of stars in the Greenwich 1910 catalogue.

Pease, F. G., and Shapley, H., The Distribution of Stars in 12 Globular Clusters, *Astroph. Journ.* **45**, 325, 1917. Counts of stars on photographs of the clusters have been made to test the presence of galactic planes. Distinct evidence was obtained in the majority of cases.

Sanford, R. F., Some Relations of the Spiral Nebulæ to the Milky Way, *L.O.B.* No. 297, 1917.

Bailey, S. I., The Northern Milky Way, *H.A.* **80**, No. 4, 1917. A series of nine plates covering the northern half of the Milky Way, uniform with a similar series for the southern half, published in *H.A.* **72**, No. 3. The scale is 1° = 6 mm. The two series together give a continuous photographic representation of the Milky Way on a uniform scale. Notes on points of interest are added.

Pease, F. G., Photographs of Nebulæ with the 60-inch Reflector. *Astroph. Journ.* **46**, 24, 1917; *Mt. Wilson Conts.*, No. 132. A valuable series of nebulæ photographs with long exposures, with descriptive notes. Some of the nebular forms shown are very remarkable.

Sampson, R. A., Note on the Southern Magnitude Distribution with special reference to the Perth Astrographic Zone, *M.N., R.A.S.* **77**, 603, 1917. Evidence as to the galactic condensation in support of the value deduced by Seares is adduced from the Perth astrographic results.

PHYSICS. By James Rice, M.A., University, Liverpool.

The main stream of research in physics is at present directed towards the elucidation of the problem of radiation and the

attainment of knowledge concerning the structure of the atom. Several papers of importance have recently appeared in the *Physical Review*, bearing on these subjects. In the August number is printed Prof. R. A. Millikan's address to the American Physical Society, delivered last December. It is the facts of radiation, he remarks, which have provided reliable information about the inner structure of the atom. Even in the days when one relied entirely on the spectroscopy of luminous and ultra-violet radiation, it was the complexity of the spectra even of simple gases which prevented the physicist from assenting to the dogma of the indivisible atom and caused him to insist that the atom must have an intricate structure, as intricate, in Rowland's phrase, "as a grand piano." When the facts concerning X-rays and γ-rays gradually became known, and especially the recent work on X-ray spectroscopy, a sort of rough outline began to show itself. The work of Barkla on secondary X-radiations and of Rutherford on the scattering of alpha rays in passing through matter gradually focussed scientific opinion on the nuclear atom, consisting of a central, positively charged body of excessively minute dimensions surrounded in the outer regions of the atom by a number of negative electrons equal to about half the atomic weight; the "diameter of the nucleus," *i.e.* the diameter of that portion of the atom which is found by experiment to be impenetrable to alpha rays, is not over a ten-thousandth of the "diameter of the atom," *i.e.* the average distance of approach of the centres of two atoms in a thermal encounter. It was, however, the now famous researches of the young British physicist, Moseley (whose death in Dardanelles campaign has ended what promised to be a brilliant career), which revealed some facts about the subatomic world with a definiteness and certainty never attained before (*Phil. Mag.* December 1913, January 1914). Moseley's work, checked and extended by De Broglie (*Comptes Rendus,* 165, 87, 352 (1917)), proves that there exist but ninety-two elements from the lightest, hydrogen, to the heaviest, uranium, and that these are built up one from the other by the successive addition of one and the same electrical element to the nucleus. This conclusion rests on Moseley's discovery that the square roots of the characteristic X-ray frequencies of the elements progress by almost equal steps from lightest to heaviest; this law he proved in a general way for the α and β lines of

hard or K-series of characteristic rays, and also for the α and β lines of the softer series, the L-series. It is this progression which justifies us in assigning a series of "atomic numbers" to the various atoms, differing from and more fundamental than the atomic weights. Our present series of atomic numbers is simply this Moseley series of steps based on square roots of frequencies, although such a series had been suggested, but not with such definiteness, by other workers, notably van der Broek, who based his suggestion on the changes in radio-active substances accompanying the loss of alpha and beta particles. Millikan proceeds in this paper to draw some important conclusions as to atomic structure which are rendered extremely probable by Moseley's law.

The first is this. We assume (1) that the ordinary law of inverse squares holds for the forces exerted by the atomic nucleus on negative electrons near it; (2) that the electrons in the atom rotate in circular orbits; (3) that the observed highest frequencies of radiation are proportional to the highest orbital frequencies of the electrons. The first assumption, whose truth is so well known in celestial regions, has been amply verified in the subatomic world by the work of Rutherford and his pupils on the scattering of alpha rays; the second assumption is justified by the demands of stability, the existence of the Zeeman effect, the phenomena of magnetism, especially the recent work of Barnett (*Phys. Rev.* 6, 239 (1915) and July (1917)) and Einstein and de Haas (*Verh. d. Deut. Phys. Gesell.* xvii. (1913)), which well-nigh demonstrates the existence of permanent, and therefore *non-radiating*, electronic orbits; the third assumption is probable from *à priori* considerations, and, as will be seen presently, necessary from certain theoretical considerations. From Moseley's law and these assumptions, one deduces, without any appeal to the quantum theory, that the distances from the nucleus of each type of atom to the orbit of the inmost electron is inversely proportional to the positive charge on the nucleus, *i.e.* to the atomic number. For let E be the intensity of force due to a nucleus at unit distance, then the force on an electron e in an orbit of radius a is eE/a^2; if this electron has a mass m and an orbital frequency n, then

$$eE/a^2 = (2\pi n)^2 m.a$$

Hence for two types of atoms we have

$$(n_1/n_2)^2 = E_1 a_2^3 / E_2 a_1^3 \quad \ldots \quad (1)$$
$$= N_1 a_2^3 / N_2 a_1^3$$

where N_1 and N_2 are the atomic numbers. By Moseley's law, using assumption (3) above

$$\sqrt{n_1/n_2} = N_1/N_2 \quad \ldots\ldots \quad (2)$$

Hence

$$a_2/a_1 = N_1/N_2 \quad \ldots\ldots \quad (3)$$

Moseley discovered that the L-series of lines for the elements progress by steps just as the K lines, the frequencies being in each case about one-eighth as great. This shows that if there is an inmost electronic orbit, there must be a second orbit in all elements, whose radius is given by equation (1) above to be about $(8)^{2/3}$ or four times as great as that of the first.

Further, it is of interest to inquire, where the highest frequency of the K-series would fall for the lightest known element, hydrogen, using the ascertained highest frequency for a heavy element like tungsten and Moseley's law—equation (2) above. This is easily done; for since for hydrogen N is unity, we simply divide the observed highest frequency for tungsten by the square of its atomic number. The atomic number of tungsten is 74; the wave length of the highest frequency radiation given out by tungsten close to $\cdot 18 \times 10^{-8}$ cm., and hence the shortest wave length which could be emitted by hydrogen is $\cdot 18 \times 10^{-8} \times (74)^2$ cm., or approximately 100 micromillimetres. Now the head of the ultra-violet series of hydrogen lines recently discovered by Lyman is at 91 micromillimetres. Considering the uncertainties in the measurements, and the fact that (2) above is not quite an exact statement of Moseley's law, the agreement between these two results is good. It seems fairly certain, then, that this Lyman series of ultra-violet lines is nothing but the X-rays of hydrogen, K-series; further, it seems a just inference from similar considerations that the ordinary series of hydrogen lines in the visible regions (Balmer's series) with its head at 365 micromillimetres is just the X-rays of hydrogen, L-series. Likewise the series in the infra-red recently discovered by Paschen is the M-series of the hydrogen X-rays, corresponding to series of X-rays softer even than the L-series, evidence for which has been obtained recently by electronic bombardment of a

few of the elements of high atomic number. In fact, we have here a revelation of the whole series of elements from hydrogen to uranium, all producing spectra of remarkable similarity, at least so far as their K and L radiations are concerned, but scattered regularly through the whole frequency region, from the ultra-violet, where the K-lines (Lyman series) for hydrogen, are found up to frequencies for uranium (92)[2] or 8,646 times as great. This similarity is due to the fact that the atoms are all similar structures, in their inner portions at least, with nuclei which are exact multiples of the "positive electron," surrounded in each case by electronic orbits which have practically the same relations in all the elements, the radii of these orbits being inversely proportional to the central charge or atomic number.

A further result from the *experimental facts* summarised in Moseley's law is that we are driven logically to some form of quantum theory as soon as we begin to introduce energy relations; for as the electrons are limited to *orbits of particular radii*, there must be a sudden or explosive loss of energy whenever an electron is obliged, either by shock from a cathode ray or by radio-active processes, to change its orbit. It must radiate when it passes from an outer to an inner orbit the difference of the energies which it possesses when remaining in each of these orbits, and this difference is a finite and *definite* amount depending on the structure of the atom itself and not on the influence which provoked the orbital change and the subsequent radiation. One must of course mention here, as another powerful support to these views, the extraordinary success of the atomic model devised by Bohr, shortly before Moseley's work had appeared and forced us to adopt the essential elements of Bohr's theory. In fact this model, based on the assumption of non-radiating and fixed electronic orbits—an assumption remarkably consonant with Moseley's results—has survived the most critical tests to which any theory can be subjected; by means of it one can calculate from the values of other known constants, such numbers as the Rdyberg spectroscopic constant and the diameter of the hydrogen atom with remarkable accuracy (in the first number to ·1 per cent.). It also deduces from the existence of the well-known Balmer's law for the series of hydrogen lines, a dynamical principle which seems likely to play a fundamental part in the new dynamics of the

atomic world which is being slowly built up, viz. the atomic nature of angular momentum. Successes such as these do not attend purely empiric assumptions. The whole of this work is clearly pointing to the view that the structure of the atoms of hydrogen and helium (the lightest atoms known) must resemble closely the *inner* structure of all atoms, even the heaviest, that region in fact which lies closest to the nucleus.

In the English Journals perhaps the most interesting paper which has appeared in the past few months is one by Sir Oliver Lodge. It will be found in the August number of the *Phil. Mag.*, and is followed by two communications from Prof. E. Eddington on it, printed in the September and October numbers. In this paper the author seeks to account for the well-known discrepancy in the perihelion motion of the planet Mercury by retaining the assumption of the fixed ether and avoiding the relativity postulate. In the January number of SCIENCE PROGRESS there was a reference in these notes to Einstein's gravitational theory founded on a generalisation of his original relativity postulate, and mention was made there of the remarkable success which this theory had achieved in removing this discrepancy between the observed movement of the perihelion of Mercury's orbit and that calculated on the usual Newtonian principles as due to the perturbations of the other planets. There is, unfortunately, no doubt that Einstein's method is not easy to explain or grasp, while Lodge's suggested explanation certainly is. It rests on the well-known assumption that some part at least of the inertial mass of a body is of electromagnetic origin, and that this electromagnetic inertia increases somewhat with the velocity of the body through the ether (assumed stagnant). The solar system as a whole moves through space, and so any planet must, during half of its rotation round the sun, be moving relative to the sun in a direction making an acute angle with the sun's way through space, and during the remaining half its motion will make an obtuse angle; hence during one-half a revolution the planet's velocity through the stagnant ether will be greater than that of the solar system as a whole; during the other half it will be less. Thus the *inertial* mass of the planet will vary, increasing and decreasing alternately above and below an average value. If now, as Lodge assumes, this variable part of the inertial mass is unaffected by gravitation, the gravitational attraction is constant at the

same distance and follows rigorously the inverse square law. The result will be the same as if the inertial mass remained constant and the intensity of gravitational attraction were increased slightly during one half a revolution. The result will be a "slewing round" of the orbit independent of the perturbations of other planets. The author subjects the matter to rigorous analysis and after making a reasonable assumption for the value of a constant succeeds in accounting for the discrepancy referred to above. Unfortunately, as Prof. Eddington points out in the two succeeding numbers of the *Phil. Mag.*, while Lodge's method resolves the discrepancy existing between observation and Newtonian Theory in the case of the perihelion of Mercury's orbit, it also introduces discrepancies in the case of Venus and the Earth which do not exist. Einstein's Theory, with all its difficulty and its revolutionary conceptions, is free from this defect.

PHYSICAL CHEMISTRY. By Prof. W. C. McC. Lewis, M.A., D.Sc., University, Liverpool.

Molecular Heats of Gases.—Bjerrum (*Zeitschr. Elektrochem.* 1911, **17**, 731) appears to have been the first to consider in some detail the question of the molecular heats of gases, by taking into account the motion of the molecule as a whole and the motions of the constituent atoms. It has long been recognised that a material body is capable in general of three types of motion, viz. motion in respect of translation, rotation and vibration. To each type there corresponds a certain number of degrees of freedom. In the case of translation the number of degrees of freedom is known to be three, and, corresponding to this, the molecule of any gas must possess energy to the amount 3/2 RT. This is true whether the molecule be monatomic or polyatomic. It follows that the *minimum* value for the molecular heat of any gas is 3/2 R. or 2·98 calories per degree. The measurements of Pier, in the case of the monatomic gas argon, have shown that this substance actually possesses the value 2·98, and, further, this quantity is independent of the temperature. It follows, therefore, that a monatomic molecule does not possess any rotational energy. It is obvious, of course, that it is incapable of vibration. Since the monatomic molecule, *i.e.* the atom itself, is evidently incapable of

rotation, it has to be regarded as a massive point. One would expect this to be true of any atom.

In dealing with a diatomic molecule, Bjerrum ascribes to it three degrees of freedom in virtue of translation, two in virtue of rotation, and one in virtue of vibration, *i.e.* vibration along the line joining the two atoms. As our present purpose is to consider more especially the question of *rotation* of gas molecules, we are not further concerned with the treatment of vibrations which Bjerrum gives in terms of the quantum theory. Rotations are regarded in the first place as being governed by the equipartition principle of classical statistical mechanics, *i.e.* the amount of kinetic energy per degree of freedom is 1/2 RT, and, as rotational energy is regarded as being entirely kinetic,[1] the amount of energy in the rotational form in a diatomic molecule is RT. The rotation of this type of molecule is analogous to that of a cylinder rotating around its major axis. In spite of the analogy, one can scarcely avoid the conclusion that rotational energy is here attributed to each atom, a conclusion which it is difficult to reconcile with the results obtained in the case of argon.

In molecules containing three or more atoms, Bjerrum considers that there are three degrees of freedom in respect of rotation. He is obviously treating such molecules in exactly the same way as we would treat a solid sphere. On summing the various energy terms due to the different types of motion, Bjerrum has succeeded in accounting, more or less satisfactorily, for the observed molecular heats of a number of gases. We are rather inclined to think, however, that the number of degrees of freedom chosen is a little arbitrary. Whilst recognising the value of Bjerrum's contribution to the subject, it is obvious, at the same time, that the problem is by no means solved. This will be apparent when we come to consider the views recently put forward by Krüger.

Krüger (*Ann. Physik*, 1916, [4], **50**, 346; *ib.* **51**, 450) takes

[1] As a matter of fact, Bjerrum has shown that the potential energy of rotation is a negligibly small fraction of the kinetic energy. In a later paper (*Nernst Festschrift*, 1912, p. 90) Bjerrum regards rotational energy as a quantity which has to be treated from the standpoint of the quantum theory. He there indicates how this accounts for the broadening of certain lines in the shorter end of the infra-red spectrum, such broadening being due to a number of fine lines lying on either side of the principal line itself.

up the position that molecules as well as atoms of gases are quite incapable of rotation. In place of rotation Krüger substitutes precessional vibrations, the molecule being essentially gyroscopic. These vibrations are totally distinct from the "normal" vibration of two atoms along the line joining their centres. Krüger's treatment deals with diatomic molecules of the Rutherford-Bohr type, that is, a molecule consisting of two atomic nuclei with two or more electrons rotating round the line joining the atoms. As a result of collisions with other molecules, Krüger considers that these electrons may suffer displacements and give rise to vibrations perpendicular to their line of motion, with the result that the atoms themselves move round in small orbits, *i.e.* the molecule possesses motion of the precessional type. Krüger points out that such motion is entirely kinetic and involves two degrees of freedom, the corresponding energy term being RT, which is identical with that postulated by Bjerrum on the basis of rotation. As far as diatomic molecules are concerned, therefore, Krüger's view is as capable of taking account of the molecular heats as is the view of Bjerrum. Krüger's treatment possesses, however, the very considerable advantage that it does not postulate atomic rotations, whether the molecule be monatomic or not. Further, Krüger has shown that the precessional vibrations which are to be ascribed to a monatomic molecule (argon) correspond to frequencies so large that they need not be considered in connection with molecular energy content and molecular heats. In other words, we arrive at the logical conclusion that a monatomic gas possesses translational energy alone.

It may be mentioned that the precessional vibrations here considered may be treated from the quantum standpoint, just as the "normal" vibrations of the atoms may be treated. That is, the energy content, in virtue of precessional vibrations, decreases with falling temperature, and converges to zero as the temperature reaches zero absolute. Krüger has also shown that the vibration frequencies involved in precessional motion lie in the further infra-red region, at about 30 μ in the case of hydrogen, and that the so-called rotation spectrum (Bjerrum, *Nernst Festschrift*, 1912) is really due to precessional vibrations. Bjerrum, it may be remarked, ascribes the principal lines in the *short* infra-red spectrum (λ less than 10 μ) to the "normal" vibrations of the atoms along their line of junction. This con-

clusion, which is of course not traversed by Krüger's considerations, appears to be quite valid.

On the whole, Krüger's view serves to bring the behaviour of monatomic and diatomic gases into very much closer relation and accord than had hitherto been the case. Considerable experimental work is still necessary, however, before we can decide between the theory of rotations and the theory of precessional vibrations in polyatomic molecules.

References to Papers on Other Subjects

A Contribution to the Theory of Solution, E. J. Hartung, *Trans. Far. Soc.* **12**, 66, 1917. Deals with measurements on liquid mixtures, *i.e.* density, heat capacity, and heat changes. The data are very valuable.

The Effect of Pressure on the Equilibrium Constant of a Reaction in a Dilute Solution, W. C. McC. Lewis, *ibid.* **12**, 314, 1917. Particular attention may be drawn to the discussion on this paper, especially the contribution by J. Rice.

Solvent Effect and Beer's Law, A. W. Stewart and R. Wright, *Journ. Chem. Soc.* **111**, 183, 1917.

The Le Chatelier-Braun Principle, Lord Rayleigh, *ibid.* **111**, 250, 1917.

The Effect of Freezing upon certain Inorganic Hydrogels, H. W. Foote and B. Saxton, *Journ. Amer. Chem. Soc.* **38**, 588, 1916; *ibid.* **39**, 1103, 1917.

The Influence of various Gases on the Photo-electric Effect in Potassium, G. Wiedmann, *Verh. Deutsch. Phys. Ges.* **18**, 333, 1916. Shows that the presence of hydrogen gas is essential to the production of the selective effect.

Electrode Potentials of Tin and Lead, A. A. Noyes and Kebe Toabe, *Journ. Amer. Chem. Soc.* **39**, 1537, 1917. The relative potentials are determined from measurement of the equilibrium point of the reaction between lead, tin and the perchlorates of these metals.

INORGANIC CHEMISTRY. By C. Scott Garrett, D.Sc.

Since our last report the shrinkage in volume of the papers published recording work in the sphere of inorganic chemistry has become more pronounced than ever. This tendency has been observable during the whole course of the war, and now that the ranks of the Allies have been swelled by the adhesion of the United States of America, practically the only outstanding scientific nation, we may expect a still further diminution in the volume of research being published. Scientific workers have perforce turned their labours to matters of more immediate practical and national importance, and the results of these labours, while they are very valuable in themselves, do not usually mark steps in the advance of general scientific theory, and are not as a rule incorporated in scientific literature. Indications of the trend of such work are to be obtained from

technological sources, and occasionally in special cases marked advances will receive some recognition in the public press. Two such cases have occurred recently, and are deserving of mention in these notes, although, as regards accurate scientific details, we have little to record. As regards one case, we have on a previous occasion pointed out the unenviable position of Great Britain in the matter of the synthetic production of nitrates or combined nitrogen compounds, and that she alone, of all the leading nations, had taken no steps towards the development of this essential industry, preferring to look to sea-borne supplies from South America. It was urged that the Government should grant assistance toward this end, which was at least as important as the dye-industry. It is, therefore, satisfactory to learn that private enterprise has at last stepped into the breach in the shape of the International Nitrogen and Power Company, London, who have received the assistance of that enlightened body, the Manchester Corporation, in whose city new works for the fixation of nitrogen are to be situated. An announcement was made to this effect in *The Manchester Guardian* of June 23, and further details appear in *Chemical News*, **116**, 9 (1917). It is stated that the process will be a new one, based on the discovery of an Englishman, and, unlike the somewhat costly Haber-catalytic ammonia process, and the still more costly Baekeland and Eyde, the Schönhurr and the Pauling electric arc processes requiring cheap water-generating power, will be capable of being worked, and economically worked, anywhere in England. In connection with this same subject of nitrogen fixation, we might mention that a very succinct review of one type of reaction for the purpose, namely cyanide formation, appears as an editorial in the *Journ. Soc. Chem. Indust.* **36**, 480 (1917), and further that the pros and cons of the various processes, from a technological point of view, are ably and fully set forth in the *Journ. of Indust. and Engineering Chem.* **9**, 829–841 (1917) in the Report to the United States Government of the Nitrate Supply Committee. See also *Journ. Soc. Chem. Indust.* **36**, 1081 (1917). It will be remembered that we previously intimated that our American cousins attributed such importance to this problem that a committee including Dr. C. L. Parsons, general secretary of the American Chemical Society, was despatched to Europe to investigate what practical advances had

been made in this matter of the economical fixation of nitrogen. His report is highly instructive and significant for us, and he greatly favours a synthetic ammonia process by the General Chemical Company, similar to that of Haber, but using lower pressures. The economy of this process has been demonstrated, and is slightly superior to that of the Haber process. Another process, a cyanide one, by the Nitrogen Products Company, according to the same authority, shows so great promise that he recommends the Government to assist financially active experimentation along its general lines.

If America, to whose threshold the nitre fields lie adjacent, is so impressed with the need for advance in the development of this chemical industry, it needs no apology on our part to lay stress on its development in this country from which the nitrate fields are separated by half the globe.

As regards the other case, this is concerned with the discovery of a method for obtaining potash economically from hitherto untapped sources. For a long period the world has been practically dependent for its potassium on the Stassfurt deposits of Germany, and it has been one of the smug replies of our enemy to threatened economic boycott that the rest of the world must reopen negotiations with her on account of its need of these supplies. It was, then, a matter for general satisfaction when, on June 28, Dr. Addison, then Minister of Munitions, announced in the House of Commons, during his survey of the work of the Ministry, that "thanks to the ingenuity of Mr. Kenneth Chance and other gentlemen working with him, a process has been discovered whereby great quantities of potash may be obtained, and the development of the scheme is now in operation with the assistance of the Ministry. We shall be able to provide every ounce of potash that the glass trade requires, as well as very largely to meet the needs of agriculture."

We sincerely hope that, in the working out, this forecast of new conditions will be fully justified. No details have so far been made available, but one may safely hazard the guess that the new source of quantities of potash lies in the feldspars or similar minerals. It is, therefore, interesting to note that during the last few years very considerable advance in potash recovery from such a class of minerals has been accomplished in America. In this connection, the joint communication of

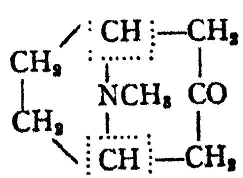
Frazer, Holland and Miller before the American Institute of Chemical Engineers, details of which are published in the *Journ. Indust. and Eng. Chem.* **9**, 935 (1917), is highly instructive. These investigators have avoided all the costly heat treatments of the feldspar minerals, which have proved to be economically unsound. They find that an artificial leucite can be obtained by heating the finely ground feldspar with strong caustic soda up to a temperature of about 275-300° C. Soluble sodium silicate is a by-product of this reaction, from which caustic soda can be reobtained by causticising with lime and filtering off the precipitated calcium silicate.

The artificial leucite is insoluble in water, and contains all the potassium and aluminium and two-thirds of the original silica of the feldspar, and yields up its constituents one at a time by proper treatment with acid. The potassium is more loosely held in the leucite than the aluminium, and the mineral readily yields soluble potassium salts on digesting with acids, which if not in excess leave all the aluminium as insoluble silicate. This residue is somewhat analogous to kaolinite, but will yield soluble aluminium sulphate with sulphuric acid along with gelatinous silicic acid, which can be rendered insoluble by evaporation. The yield of potassium is stated to be practically theoretical, and of aluminium about 86 per cent.

The economic success of these investigations on large-scale operations as a source of potassium will greatly depend on the value of this aluminium recovery.

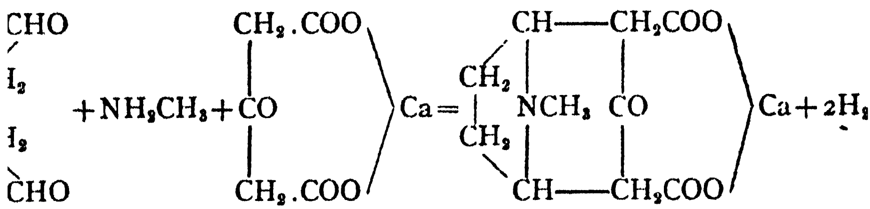
ORGANIC CHEMISTRY. By P. HAAS, D.Sc., Ph.D., St. Mary's Hospital Medical School, London.

A NEW and very ingenious synthesis of tropinone from comparatively simple substances is described by Robinson (*J. Chem. Soc.* 1917, 762). As may be seen from formula I.



tropinone has a symmetrical molecule and imaginary hydrolysis at the points indicated by the dotted lines, should yield

succinic dialdehyde, methylamine and acetone, and the author has now found that tropinone could actually be obtained in small yield by the condensation of these three substances in aqueous solution. A better yield, however, amounting to 42 per cent. of the theoretically possible, was obtained by replacing the acetone by the calcium salt of acetone dicarboxylic acid and allowing the condensation to proceed in aqueous solution for about fifty hours according to the following scheme :



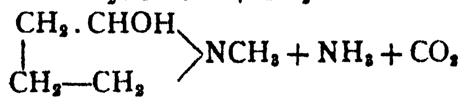
II

The resulting calcium tropinone dicarboxylate II. loses two molecules of carbon dioxide when acidified and heated, yielding tropinone I. It is hoped to improve the conditions of this synthesis, so as to make tropinone an easily accessible substance and thereby to render the technical preparation of homatropine and tropacocaine feasible and also to bring within the range of possibility the technical synthesis of atropine. The author has found a convenient method of identifying small quantities of tropinone depending on the formation of a characteristic crystalline derivative by condensation with piperonal in aqueous alcoholic solution.

In a subsequent paper, entitled " A Theory of the Mechanism of the Phytochemical Synthesis of Certain Alkaloids " (*J. Chem. Soc.* 1917, 876), the same author elaborates the above synthesis of Tropinone with a view to offering a possible explanation of the formation of most of the important alkaloids within the plant. It is pointed out that most of the laboratory syntheses of alkaloids involve experimental conditions differing entirely from those obtaining in a plant, or, on the other hand, it has frequently been assumed that plants have at their disposal enormously powerful reagents which are able to bring about reactions which cannot be reproduced in the laboratory, whereas in the author's opinion insufficient attention has so far been paid to the possibility of the formation of highly reactive intermediate products which can undergo a number of comparatively

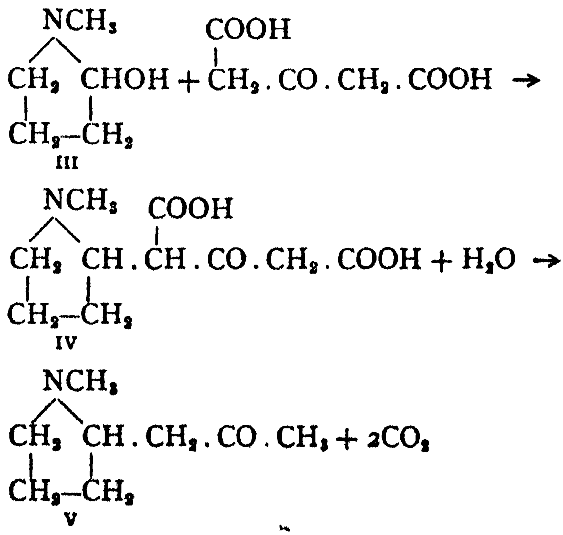
simple transformations. As an example of this he cites the synthesis of tropinone above mentioned, and further shows that by the condensation of formaldehyde with diaminoacids such as ornithine or lysine it is possible to account for the formation of alkaloids for the pyrrolidine or piperidine groups. Thus, for example, a compound of the formula III. could be formed by interaction between ornithine and formaldehyde according to the following equation :

$$NH_2.CH_2.CH_2.CH_2.CHNH_2.COOH + CH_2O =$$



III

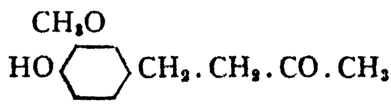
Compound III. would then, by condensation with acetone dicarboxylic acid and subsequent elimination of carbon dioxide, yield the alkaloid hygrine V.



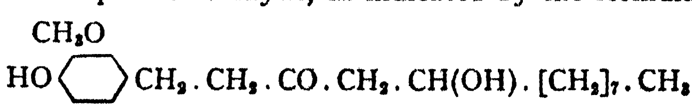
Compound IV. may also be the progenitor of nicotine by further condensation with formaldehyde and ammonia. Similarly, by the application of simple reactions such as aldol condensations, oxidation, or dehydration the author is able to account for the formation of such complex alkaloids as the pelletierines, sparteine and the opium alkaloids belonging to the piperidine, quinuclidene and isoquinoline groups respectively. It is not possible within the limits of this article to do justice to this very

important contribution to the theory of alkaloid synthesis, but it may be pointed out, before leaving this subject, that the starting materials for building up all these complex substances are formaldehyde, ammonia, aminoacids and acetone dicarboxylic acid, the first three of which are of course generally assumed to be present in the plant, while the latter may be produced by the oxidation of citric acid, although other possible sources are also indicated.

Three papers dealing with the Pungent Principle of Ginger are published consecutively in the same number of the *Journal of the Chemical Society*, one coming from Japan and the other two from Manchester. The first paper, by Nomura (*J. Chem. Soc.* 1917, 769), deals with the identification and synthesis of a new ketone Zingerone isolated from ginger by extraction with ether and shaking the extract with 2 per cent. sodium hydroxide. The ketone was proved to have the constitution represented by the following formula,



and a number of its reactions and derivatives are described. Lapworth, Pearson and Royle (*J. Chem. Soc.* 1917, 777) have also succeeded in isolating the same ketone, but have shown that it probably does not occur as such in the ginger, but is in reality a constituent of the oleo-resin gingerol, which is the true pungent principle of ginger, and that it is combined with a saturated aliphatic aldehyde, as indicated by the formula



This substance is readily hydrolysed by baryta and other alkalies, and the fact that Nomura treated his ether extract with caustic soda would account for his obtaining the Zingerone apparently directly from the ginger.

The question of the constitution of the urea molecule is once more raised by Werner (*J. Chem. Soc.* 1917, 863), who has subjected the reaction between urea and nitrous acid to a careful study, as a result of which he finds that the generally accepted equation

$$CON_2H_4 + 2HNO_2 = CO_2 + N_2 + 2H_2O$$

is incorrect and that a certain amount of cyanic acid is produced, varying according to the experimental conditions. The author supports the view recently expressed by Rây, Dey and Ghosh (*J. Chem. Soc.* 1917, 414) that urea is not attacked by pure nitrous acid alone, but finds that "when a salt of urea is produced by the presence of a sufficiently strong acid, it is immediately attacked by nitrous acid, because an amino-group is thereby presented for such attack." The facts are accounted for by assuming that the constitution of urea in aqueous solution is represented by formula VI. and that in contact with a strong acid it undergoes rearrangement according to the following equation :

$$\underset{\text{VI}}{HN:C\begin{array}{l}\diagup NH_3\\ \;\;|\\ \diagdown O\end{array}} + HX = \underset{\text{VII}}{HN:C\begin{array}{l}\diagup NH_2.HX\\ \diagdown OH\end{array}}$$

The resulting salt VII. then reacts with urea as follows :

$$HN:C\begin{array}{l}\diagup NH_2.HX\\ \diagdown OH\end{array} + HNO_2 = N_2 + HNCO + 2H_2O + HX$$

The cyanic acid produced is decomposed in two ways as fast as it is produced according to either of the two following reactions :

$$HNCO + H_2O = NH_3 + CO_2$$
$$\text{or}\quad HNCO + HNO_2 = CO_2 + N_2 + H_2O$$

The fact that a certain amount of nitrogen is liable to be fixed in form of an ammonium salt explains why the reaction cannot be made use of for estimating urea in aqueous solution.

Appended is a list of some other important papers which, for reasons of space, it is not possible to review.

CUNNINGHAM and DORÉE, The Chemistry of Caramel, *J. Chem. Soc.* 1917, 589.

FISCHER and BERGMANN, Further Syntheses of Glucosides, etc., *Berichte*, 1917, **50**, 711.

KOMPPA and ROSCHIER, Complete Synthesis of Fenchene, *Chem. Soc. Abstracts*, 1917, (i), 466.

LA FORGE and HUDSON, Sedoheptose, a New Sugar from *Sedum spectabile*, *J. Biol. Chem.* 1917, **30**, 61,

PAULY, Synthesis of Peptides in the Animal Organism, *Zeitsch. physiol. Chem.* 1917, **99**, 161.

GEOLOGY. By G. W. Tyrrell, A.R.C.Sc., F.G.S., University, Glasgow.

Structural and Dynamical Geology

Oldham, R. D., The Structure of the Himalayas, and of the Gangetic Plain, as Elucidated by Geodetic Observations in India, *Mem. Geol. Survey of India*, 1917, **42**, pt. 2, pp. 153.

MacMillan, W. D., On the Hypothesis of Isostasy, *Journ. Geol.* 1917, **25**, 105–11.

Bowie, W., Our Present Knowledge of Isostasy from Geodetic Evidence, *ibid.* 422–45.

Wood, H. O., Notes on the 1916 Eruption of Mauna Loa, *ibid.* 322–36, 467–88.

Jaggar, T. A., Vulcanologic Investigations at Kilauea, *Amer. Journ. Sci.* 1917, **44**, 161–220.

Washington, H. S., Persistence of Vents at Stromboli and its Bearing on Volcanic Mechanism, *Bull. Geol. Soc. Amer.* 1917, **28**, 249–78.

The examination of geodetic evidence in the Gangetic and Himalayan region by Oldham shows that the general distribution of the excesses and defects of gravity agrees best with the assumption of a somewhat rigid crust resting on a denser yielding layer ; but that neither this, nor any other hypothesis of support, has yielded a complete and satisfactory explanation of the origin of the Himalayas, and of the deep alluvial trough of the Gangetic plain. The theory of isostasy in relation to geodetic evidence is further discussed by MacMillan and Bowie.

The papers of Wood and Jaggar contain perhaps the closest observations of volcanic phenomena that have ever been made ; and it is only by such constant and detailed study on the spot that any real insight into volcanic mechanism will be obtained. The " gas-fluxing " hypothesis of the origin of volcanic vents enunciated by Daly has been applied by Washington to explain the curious persistence in location of the six Stromboli vents over a period of considerably more than a century. Their unusual number, small size and contiguity over a restricted area, their want of synchronism, and difference in type of activity, are also best explained on Daly's hypothesis.

Regional and Stratigraphical Geology

Sinclair, J. H., The Cretaceous of Alberta, *Bull. Geol. Soc. Amer.* 1916, **27**, 673 84.

Martin, G. C , The Triassic Rocks of Alaska, *ibid.* 685–718.

Arber, E A. N., The Structure of the South Staffordshire Coalfield, with Special Reference to the Concealed Areas and to the Neighbouring Fields, *Trans. Inst. Min. Eng.* 1916, **52**, pt. 1, 35–70.

Staples, E. H., Some Effects of the Master Folds upon the Structure of the Bristol and Somerset Coalfields, *ibid.* 1917, **52**, pt. 2, 187–98.

KIDSTON, R., CANTRILL, T. C., and DIXON, E. E. L., The Forest of Wyre and the Titterstone Clee Hill Coalfields, *Trans. Roy. Soc. Edin.* 1917, **51**, pt. 4, 999–1084.

MACNAIR, P., The Hurlet Sequence in the East of Scotland, and the Abden Fauna as an Index to the Position of the Hurlet Limestone, *Proc. Roy. Soc. Edin.* 1917, **37**, pt. 2, 173–208.

KIDSTON, R., and LANG, W. H., On Old Red Sandstone Plants showing Structure, from the Rhynie Chert Bed, Aberdeenshire. Part I. *Rhynia Gwynne-Vaughani*, Kidston and Lang, *Trans. Roy. Soc. Edin.* 1917, **51**, pt. 3, 761–84.

HORNE, J., The Discovery of Silicified Peat Beds in the Scottish Old Red Sandstone, *Scott. Geogr. Mag.* 1917, **33**, 385–92.

REED, F. R. C., The Ordovician and Silurian Brachiopoda of the Girvan District, *Trans. Roy. Soc. Edin.* 1917, **51**, pt. 4, 795–998.

GRABAU, A. W., Comparison of American and European Lower Ordovician Formations, *Bull. Geol. Soc. Amer.* 1916, **27**, 555–622.

RAYMOND, P. E., Expedition to the Baltic Provinces of Russia and Scandinavia, Part I. Correlation of the Ordovician Strata of the Baltic Basin with those of Eastern North America, *Bull. Mus. Comp. Zool. Harvard*, 1916, **56**, No. 3, 179–286.

TWENHOFEL, W. H., Part II. The Silurian and High Ordovician Strata of Esthonia, Russia, and their Faunas. Part III. An Interpretation of the Silurian Section of Gothland, *ibid.* 289–354.

GREEN, J. F. N., The Age of the Chief Intrusions of the Lake District, *Proc. Geol. Assoc.* 1917, **28**, 1–30.

TOMLINSON, C. W., The Middle Palæozoic Stratigraphy of the Central Rocky Mountains Region, *Journ. Geol.* 1917, **25**, 112–34, 244–57, 373–94.

RANSOME, F. L., Some Palæozoic Sections in Arizona and their Correlations, *United States Geol. Survey*, Prof. Paper 98-K, 1916, 133–66.

BAILEY, E. B., The Islay Anticline (Inner Hebrides), *Q.J.G.S.* 1917, **72**, pt. 2, 132–64.

NOBLE, L. F., and HUNTER, J. F., A Reconnaissance of the Archæan Complex of the Granite Gorge, Grand Canyon, Arizona, *United States Geol. Surv.* 1916, Prof. Paper 98-I, 95–113.

DRESSER, J A., Part of the District of Lake St. John, Quebec, *Geol. Surv. Canada*, 1916, *Memoir* 92, pp. 88.

HERON, A. M., The Geology of North-Eastern Rajputana and Adjacent Districts, *Geol. Survey of India*, 1917, *Memoirs*, **45**, pt. 1, pp. 128.

Again this quarter a very large amount of stratigraphical work has come to hand. Arber finds that while the South Staffordshire Coalfield agrees closely in its palæobotanical horizons with that of Warwickshire, the productive measures of each field, as well as those of the Welsh borderland fields, originated in separate basins. The higher red unproductive measures of the Keele and Enville Series are found to be continuous over the whole of these areas, and are believed to be of Stephanian rather than of Middle or Upper Permian age.

Macnair's work on the limestone horizons in the Lower Limestone Series of the Scottish Carboniferous has now been

carried into the East of Scotland, where he demonstrates, on lithological and palæontological grounds, the continuity of all the horizons delimited in the west. The two lowermost, however, are not typically developed owing to the prevalence of estuarine oil-shale conditions of deposition over the eastern area during that period.

The discovery of plant-bearing cherts in the Old Red Sandstone of Rhynie, Aberdeenshire, by D. W. Mackie of Elgin, is of more than ordinary importance to both geologists and palæobotanists. The plants have their structures perfectly preserved, and Drs. Kidston and Lang have made the utmost use of this unique opportunity of studying the microstructure of plants of this early age. They have separated a new class, *Psilophytales*, to include *Rhynia* and certain of the species collected under the name of *Psilophyton princeps*. Dr. Horne explains the geological import of this discovery. The Rhynia occurs in the position of growth, and no less than six peat-beds are to be distinguished within the eight feet of chert. The geological bearing of the occurrence of these land plants upon the hypothesis of the fluviatile origin of the Old Red Sandstone is obvious (see SCIENCE PROGRESS, October 1917, p. 333).

Reed's work on the brachiopod fauna of the Ordovician and Silurian rocks of Girvan emphasises the American characters of the fauna of the Stinchar limestone and of the Balclatchie Beds (Ordovician); but these traits are lost in the succeeding beds, and in the Silurian, in which the European facies is practically unmixed with foreign elements. The predominance of local and peculiar species is a distinctive feature, and suggests a considerable degree of isolation of the Girvan area during Lower Palæozoic times.

Grabau recognises three main centres of Lower Ordovician sedimentation in western Europe, a Baltic and a Mediterranean area, separated by the great peninsula of Armorica, and both communicating with the Ordovician Atlantic. The third area, North Scotland and Western Scandinavia, was entirely separated from the Atlantic by the old Caledonian land mass which extended across to Newfoundland. The arrangement and succession of the early Ordovician rocks indicate an eastward advance of the Atlantic waters, with a concomitant lateral spreading and inundation upon the shores of Armorica and Caledonia. A second great event was the retreat of the late

Arenig Sea, until the Baltic region became dry land, and Scandinavia was joined to Armorica. These movements appear to have been simultaneous in Europe and North America.

The work of Raymond and Twenhofel has been directed to many of the same problems as that of Grabau and Reed. Twenhofel states that coral reefs exercised a controlling influence in the development of the lithology and stratigraphy of the famous Silurian area of Gotland.

The unravelling of the Palæozoic and Pre-Cambrian stratigraphy of the Rocky Mountains and Great Basin regions is being prosecuted with vigour, as shown by the important papers of Tomlinson, Ransome, Noble and Hunter.

Bailey's paper on the Islay Anticline deals again with the vexed questions of the succession and structure of the Highland Schists. The complexity of the district forbids any short statement of his results, but Mr. Bailey's interpretation is at variance with that advanced by the Geological Survey in the official memoir upon the district, especially as regards the structure of the northern part of the island.

In Rajputana Mr. Heron distinguishes an Archæan series of schists, crystalline limestone, and quartzite, intruded by granites (Aravalli system), which is separated by violent unconformity from the overlying Delhi system of thick limestones and quartzites. The great break is probably near the horizon of the Eparchæan unconformity of the North American geologists.

Petrology—Igneous Rocks

Daly, R. A., The Geology of Pigeon Point, Minnesota, *Amer. Journ. Sci.* (4), 1917, **43**, 423-48.

Bowen, N. L., The Problem of the Anorthosites, *Journ. Geol.* 1917, **25**, 209-43.

Tyrrell, G. W., The Picrite-Teschenite Sill of Lugar, Ayrshire, *Q.J.G.S.* 1917, **73**, pt. 2, 84-131.

—— Some Tertiary Dykes of the Clyde Area, *Geol. Mag.* (6), 1917, **4**, 305-15, 350-6.

Holmes, A., Albite-Granophyre of Carrock Fell, *ibid.* 403-7.

Smith, H. G., The Lurgecombe Mill Lamprophyre, *Q.J.G.S.* 1917, **73**, pt. 2, 77-83.

Lacroix, A., Les Laves à hauyne d'Auvergne et leurs enclaves homœogènes: importance théorique de ces derniers, *Comptes Rendus*, 1917, **163**, 581-8.

Bowen, N. L., The Sodium Potassium Nephelites, *Amer. Journ. Sci.* 1917, **43**, 115-32.

Pigeon Point on Lake Superior is one of the classic cases of the development of "red rock" (micropegmatite) in a gabbroid magma. Originally described by Bayley as a great dyke, it is

now shown by Daly to be a sill, thereby greatly simplifying the discussion of its origin and differentiation. Daly believes in the origin of the red rock by the assimilation of sediments, and Bayley's original views are much elaborated. The assimilation is believed to have been masked by differentiation, which had two phases, one controlled by gravity, the other by gas tension.

Bowen extends the hypothesis of gravity differentiation to the origin of the anorthosites. These mono-mineralic rocks show no signs of ever having been at the temperature necessary for the crystallisation of plagioclase, and consequently have probably never been molten as such, but have been formed simply by the collection of plagioclase crystals from a complex melt. The method of accumulation considered most likely is that of the separation by gravity of the mafic constituents from a gabbroid magma, while the plagioclase crystals remained suspended. The consequences of this theory are shown to be in accord with the evidence obtained by the field study of the anorthosites of the Adirondack Mountains and of the Morin area of Canada.

The Lugar sill also provides striking evidence in favour of gravity differentiation. Roughly speaking it consists of a sheath of teschenite, enclosing a large mass of ultrabasic rock which grades from theralite at the top, through picrite, to peridotite. This gradation is characterised by an increasing proportion of olivine downward. This is interpreted as being due to the progressive sinking of the early-formed olivine crystals, which accumulated toward the base, forming a peridotic stratum.

Smith describes a biotite-lamprophyre dyke intrusive into Carboniferous shales. A remarkable feature is the inclusion of xenoliths carrying corundum, staurolite, and magnetite.

Lacroix proposes three new names for the long-suffering petrographer. His *ordanchite* is the prevalent type of hauyn-tephrite occurring on Mont Dore, Auvergne. *Tahitite* is a variety of hauyn-tephrite much richer in hauyn and alkalies than ordanchite. Among numerous other granular xenoliths occurring enclosed in the Mont Dore lavas, Lacroix distinguishes *mareugite*, which may be described as hauyn-bytownite-gabbro. Its abundance as xenoliths is so great that it must occur *in situ* beneath the volcano.

Natural History of Sedimentary Rocks

VAN TUYL, F. M., The Origin of Dolomite, *Iowa Geological Survey, Annual Report*, 1916, **25**, 251-422.

NORTON, W. H., A Classification of Breccias, *Journ. Geol.* 1917, **25**, 160-94.

JEFFREY, E. C., Petrified Coals and their Bearing on the Problem of the Origin of Coals, *Proc. Nat. Acad. Sci. Washington*, 1917, **3**, 206-11.

KINDLE, E. M , Recent and Fossil Ripple-mark, *Geol. Surv. Canada*, 1917, *Mus. Bull.* **25**, pp. 121.

BUCHER, W. H., Large Current-ripples as Indicators of Palæogeography, *Proc. Nat. Acad. Sci. Washington*, 1917, **3**, 285-91.

KINDLE, E. M , Some Factors affecting the Development of Mudcracks, *Journ. Geol.* 1917, **25**, 135-44.

In his exhaustive monograph on the origin of dolomite, Van Tuyl shows that the great majority of stratified dolomites have been formed by the alteration of limestone. A little dolomite may be a chemical precipitate or of clastic derivation. It is doubtful whether organisms have ever given rise to more than weakly dolomitic limestone, and the importance of marine or surface leaching has been greatly over-emphasised. The most extensive dolomitisation of limestone has been effected under the sea by replacement. Dolomitisation by ground-water is believed to be only local and imperfect.

Kindle's work on ripple-mark will become the standard publication on that phenomenon. He elucidates the criteria distinguishing the various kinds of wind, current, and wave ripples ; and has made use of several ingenious methods of studying and securing records of ripple-marks under water. It is shown that fossil ripple-marks may be diagnostic of marine or lacustrine conditions, and that they have considerable palæogeographic significance.

MINERALOGY AND CRYSTALLOGRAPHY. By ALEXANDER SCOTT, M.A., D.Sc., University, Glasgow.

Anisotropic Liquids.—Since Lehmann's extensive investigations of the doubly refracting liquid obtained by melting cholesteryl benzoate a large number of memoirs on the subject have appeared and many theories propounded to explain the phenomenon. Voigt (*Physik. Zeit.* **17**, pp. 76-87, 128-35, 152-61, 1916) summarises the experimental work which has been done and discusses at length these various theories. From a critical consideration of all the investigations of isolated drops and homogeneous layers and of the physical properties

of these, he concludes that anisotropic liquids are in many ways analogous to uniaxial crystals, particularly with reference to properties such as double refraction, dichroism and electrical conductivity. There are, however, certain definite differences which are probably connected with the nature of the units, the element of the anisotropic liquid being molecular and that of the crystal atomic. There is no evidence that the molecules, even when arranged in parallel directions, are in the form of a space-lattice. The fact that the external forms of anisotropic liquids have no connection with ordinary crystal faces, which seems to be justified by the work by Grandjean and Friedel (*Compt. Rend.* **151**, pp. 442–4, 1910) is due to the difference between the cohesive forces in the two cases ; in the latter there is the strong " chemical affinity " between the atoms, in the former the weaker attraction between contiguous molecules. Hence the external form of the liquid approximates to a sphere.

Moll and Ornstein (*Proc. Akad. Wetensch. Amsterdam* **19**, pp. 1315–27, 1917) have by means of a thermopile and galvanometer, devised a new method of determining the extinction of anisotropic liquids and have investigated the effects of both longitudinal and transverse magnetic fields. They find that the crystals tend to be orientated in the direction of the lines of force, but that the glass walls of the tube and temperature-effects introduce perturbing forces so that the extinction is variable.

Svedberg (*Ann. Physik.* **49**, pp. 437–55, 1916) has measured the electrical conductivity of various anisotropic liquids at different temperatures, and a theoretical discussion of his results is given by Voigt (*ibid.* **52**, pp. 222–8, 1916). The former (*Kolloid Zeit.* **18**, pp. 54–6, 101–6, 1916, **20**, pp. 73–6, 1917, **21**, pp. 19–21, 1917; from *Jour. Chem. Soc.* **110**, p. 306, p. 383, 1916, **112**, p. 249, p. 439, 1917) has investigated some chemical reactions in anisotropic solvents. The velocity of reaction measured by means of the electrical conductivity changes abruptly at the temperature of isotropic fusion, and is also diminished when the liquid is under the influence of a magnetic field.

Lehmann (*Ann. Physik.* **51**, pp. 353-90, 1916) describes the modifications of homogeneous anisotropic liquids induced by spirally twisting the solid previous to the formation of the liquid phase. The same author (*ibid.* **52**, pp. 445–77, 527–40, 541–52, *Science Abs.* **20**, A. p. 446) deals with the structure

of inhomogeneous liquids, where the inhomogeneity arises from the contiguity of solid crystals, from admixture and from alterations in structure.

Gaubert (*Compt. Rend.* **163**, 392–4, 1916) has obtained anisotropic liquids by the evaporation of solutions of anisal-p-amidoazotoluene in ether, chloroform, benzene, etc., and finds that the forms obtained are not the same as those obtained by the fusion of the pure solid. He has also determined (*ibid.* **164**, pp. 405–7, 1917) the rotary power of some salts of cholesterol. While the relation between the rotation-dispersion and the absorptive properties is confirmed, the results regarding the sign of the rotation are different from those obtained by Stumpf (*Physik. Zeit.* **11**, p 780, 1910).

In a series of papers, Grandjean has confirmed and greatly extended the observations made by Mauguin (*Compt. Rend.* **156**, p. 1246, 1913) with reference to the orientation of anisotropic liquids on crystals. The former (*Bull. Soc. franc. Min.* **39**, pp. 164–213, 1916) finds that when a drop of an anisotropic liquid such as azoxyphenetole, azoxyanisol or anisaldazine, is placed on a good cleavage surface of a mineral such as blende, rock-salt, pyrophyllite and so forth, the orientation of the anisotropic liquid often bears some simple relation to the symmetry of the crystal. The three liquids named orientate themselves so that the unique axis lies in some definite direction parallel to the cleavage face, so long as the latter approximates to the so-called "perfect cleavage," that is, so long as the crystal surface is a perfect plane. Other substances, however, have a tendency to orientate themselves so that the optic axis is normal to the cleavage surface; this may be due to some disturbing influence such as minute particles of solid, deformations in surface of liquid, etc. In the second paper (*Compt. Rend.* **164**, pp. 105–7, 1917) the variation in the orientation of the optic axis is discussed and the conclusion is reached that this variation is much greater than the variation in direction of the cleavage face and consequently than the deformation of the lattice of the crystal. In a third paper (*ibid.* pp. 636–9) the orientation of salts of cholesterol and the oleates on crystals is described. It is found that, in the case of the positive liquid form of cholesteryl caprinate on the cubic cleavage (100) of rock-salt, the optic axis is parallel to the diagonals of the cube face and that, on the cleavage of zinc-

blende, it is parallel to the longer diagonal of the rhombic dodecahedron. The transformation to the negative form is retarded on the crystal surface, and, where it does occur, the optic axis is often perpendicular to that in the still positive crystals. The retarded transformation generally occurs about the temperature of isotropic fusion. In all the other liquids the optic axis is normal to the crystal surface.

The same author (*ibid.* 280–3) has applied a modified form of the magnetic theory of Langevin and Weiss to anisotropic liquids and concludes that, at the point of contact of these with solid bodies, the anisotropic condition should persist above the temperature of isotropic fusion, and therefore after the remainder of the liquid has become isotropic. This is explained by the existence of local molecular fields produced by the solid round the points of contact and equivalent to the exterior field of the magnetic theory. That this is in agreement with the facts is proved by the experiments described in another paper (*ibid.* 431–4). There it is shown that, on heating an anisotropic liquid above the temperature of isotropic fusion, and then allowing to cool, the doubly refracting regions appear at the same points, and with the same orientation, as before. On passing the transition temperature during heating, these regions can still be detected in polarised light, and those residual traces, which the author terms "pellicles," have the same orientation as the original anisotropic regions.

Crystal-growth.—The accuracy of the experiments made by Becker and Day in 1905, to show that a crystal growing under a load developed a linear force in the direction of the load, was questioned by Bruhns and Mecklenburg in 1913. The former authors have therefore repeated these experiments (*Journ. Geol.* **24**, pp. 313–33, 1916) and have shown that their results were correct, and that the comparative method of the latter does not test the point at issue. Taber (*Proc. Nat. Acad. Sci.* **3**, pp. 297–302, 1917) has obtained definite evidence that pressure phenomena accompany the growth of crystals of ammonium nitrate. The importance of this in connection with the growth of crystals in veins and concretions in rocks, which are difficult to explain otherwise, is indicated.

Wright and Hostetter (*Journ. Wash. Acad. Sci.* **7**, pp. 405–17, 1917) have made numerous observations on the growth of crystals under strain with reference to the reversibility of the

equilibrium relations. The theory that each particle which is deposited on the crystal is in the same state of strain as the crystal in its immediate neighbourhood is confirmed, so that the relations between crystal and liquid are always reversible.

Rawdon (*Met. and Chem. Eng.* **15**, pp. 406–8, 1916) makes some interesting observations on the twinning of crystals of electrolytically deposited copper. The tendency to twinning increases with the current density and the effect of annealing indicates that the electrolytically deposited crystals form under conditions of stress.

Schubnikov (*Zeit. Kryst. Min.* **54**, pp. 261–66, 267–72, 1914, from *Jour. Chem. Soc.* **112**, ii. p. 449, p. 450, 1917) in the first paper deals with the influence of temperature variations on the growth of crystals, and explains the growth of some faces at the expense of others in this way, in preference to the theory of varying solubility of the different crystallographic forms. In the second paper he has investigated the relation between supersaturation and external symmetry, and finds that the latter tends to increase with the degree of supersaturation.

The formation of striæ and vicinal forms is discussed mathematically by Grinakovskii (*Jour. Russ. Phys. Chem. Soc.* **48**, pp. 974–85, 1916, from *Chem. Abs.* **11**, p. 1070, 1917), who concludes that it is due to the disturbance of the equilibrium conditions, particularly of the thermal equilibrium of the contact layer of liquid and crystal.

Polymorphism.—In a number of papers Bridgman has made an elaborate study of polymorphic changes. In the case of the nitrates of the monovalent metals (*Proc. Amer. Acad.* **51**, pp. 581–625, 1916) it is found that the rubidium, cæsium and thallium salts are similar, but the behaviour of silver nitrate is very different. The ammonium and potassium nitrates are both very complicated, and one new type of the former and two of the latter, stable only under high pressure, have been obtained. From a consideration of the phase diagrams, especially at high pressures, it is concluded that the alkali nitrates have complete similarity of structure. In a second paper (*ibid.* **52**, pp. 57–8, 1916) the velocity of polymorphic transformation for many substances has been measured by determining the isothermal time rate of change of pressure during the transformation. The velocity is not the same for the two directions in a reversible transformation, while there

is in the region of the transition conditions a definite range for each substance, throughout which the velocity is apparently zero. In a final paper (*ibid.* **52**, pp. 91–187, 1916) the author collects the results and discusses the general mechanism of polymorphic change. The substances examined behave in very diverse ways, and it is evident that many more data, especially with respect to the crystallography of the forms, are necessary before a satisfactory explanation is reached. Certain analogies with the behaviour of liquids under high pressure are pointed out. The author suggests the restriction of the term—polymorphic—to those substances which exist in several forms with reversible transformations.

Wright (*Jour. Amer. Chem. Soc.* **39**, pp. 1515–25, 1917) has made a complete study of the various crystalline forms of menthol. The latter exists in four modifications, three of which undergo monotropic transformations to the stable hexagonal α-form which melts at 40·5°, the other forms, β, γ, δ, melting at 35·5°, 33·5° and 31·5° respectively. Spherulites develop readily in the crystallisation of all four forms, and these change from spherical to ellipsoidal at the transition temperatures of the monotropic forms.

Mineralogy.—Taber (*Bull. Amer. Inst. Mining Eng.* 1916, pp. 1973–98) discusses the genesis of asbestos. During the formation of serpentine, the volume change induces strains in the rock and the chrysotile develops in the fractures thus formed. The mineral structure is due to the inhibition of growth in certain directions by the external conditions so that an extremely prismatic form develops. This is criticised by Brauner, Dresser, Graham and Merrell (*ibid.* 1917, pp. 397–405), who hold that the extrapolation of laboratory results on the growth of fibrous minerals is not justified on account of the fact that in nature the influence of the wall rock is important, and further that the cavities in which the veins form are due to shrinkage and not to pressure.

Clarke and Wheeler (*U.S. Geol. Surv. Prof. Paper* **102**, pp. 1–56, 1917) describe the mineralogical nature of the inorganic parts of marine invertebrates. Magnesium salts are found to be more common than is generally supposed, and are usually in isomorphous mixture with calcite, not aragonite. No satisfactory explanation of the distribution of calcite and aragonite in shells is reached.

Ledoux (*Bull. Soc. franc. Min.* **39**, pp. 232–80, 1916) discusses from the theoretical point of view the geometrical properties of solid solutions, with reference to the monoclinic and orthorhombic pyroxenes. Larsen (*Amer. Miner.* **2**, pp. 17–19, 1917) points out that massicot consists of two different crystalline forms of lead oxide, and suggests certain modifications in nomenclature in order to prevent confusion.

BOTANY. By E. J. SALISBURY, D.Sc., F.L.S., East London College, University, London.

General.—The current volume of the *Journal of the Royal Horticultural Society* contains an interesting paper by E. A. Bunyard on the History and Development of the Red Currant. Three species are regarded as being concerned, viz. *Ribes vulgare, R. rubrum* and *R. petræum*. The garden Red Currants are probably the outcome of breeding between these wild species. The *macrocarpum* section, however, appears to have originated in a mutation from *R. vulgare*.

Various writers have called attention to occasional specimens of *Myrica gale* on which both male and female flowers were present on the same shoot, and one instance has been recorded in which the flowers were hermaphrodite. Davey and Gibson (*New Phytologist*, vol. xvi, 1917) have made a detailed study of the distribution of sex in this species, and find that all gradations between the male and female condition are to be observed. Moreover, shoots which in one year bore female catkins subsequently bore male catkins, and the reverse change was also noted. Several intermediate forms were recognised. In some, male and female catkins occurred on the same shoot, the former being usually situate below, and the latter above. In the transition region the catkins were sometimes androgynous. In other cases all the catkins were androgynous, and in these the lower part of the shoot was usually male and the upper female, hermaphrodite flowers sometimes being present where the two sexes meet.

In still other cases most of the catkins were composed of protogynous hermaphrodite flowers, each of the latter consisting of a normal ovary surrounded by three to four stamens and bearing at the base two bracteoles. Similar changes of sex have been observed in other species, and here as there nutrition is perhaps an important factor in their determination.

Taxonomy.—Small (*New Phytologist*, 1917) gives an historical résumé of the views which have been held regarding the taxonomy and interrelationships of the *Compositæ*, in which connection he emphasises the great value of the work of Cassini. The view taken is that the *Senecioneæ* are the primitive group from which the tribe of the *Chichorieæ* have arisen.

Morphology.—M.M. Lignier and Tison (*Bull. Soc. Bot. Fr.*, 1913) described an abnormal flower of *Gnetum scandens* and from its structure concluded that the normal female flower of this genus probably represents a simple, perhaps compound, axis provided with two nodal cupules. This axis is terminated by a plurilocular ovary (the innermost envelope) enclosing a single orthotropous ovule. The late Prof. Pearson has discussed their results, and summarises the evidence as to the morphology of the female flower (*Trans. Roy. Soc. S. Africa*, 1917). He concludes that : the two outer envelopes are homologous with the capsule of the spike ; the innermost envelope probably consists of a modified leaf-pair homologous with the capsule, and the axis of the flower is homologous with that of the spike. In other words, the spike and female flower are modifications of the same primitive structure, which may have consisted of a terminal nucellus surrounded by a single ovular envelope followed by a ring of male flowers and one or more pairs of modified leaves. The ovule and the innermost envelope are regarded as strictly homologous throughout the group and the resemblance of the protective structures to the perianth of Angiosperms as not indicative of phylogenetic relationship.

Ecology.—Mr. W. R. B. Oliver contributes to the *Transactions of the New Zealand Institute* (vol. xlix.) an instructing account of the vegetation of Lord Howe Island. The soil is of two types, viz. that of the limestone area, which is sandy and dry, and a moister volcanic soil rich in humus. The total recorded species of vascular plants number 209, of which 70 are endemic. Of these latter one-quarter are related to New Zealand species and about one-sixth to those of Australia. Several well-defined formations and associations are recognised. Of these temperate evergreen river forest occupies the largest area and occurs from sea-level up to 300 metres. It is either dominated by *Ficus columnaris* or *Acicalyptus Fullagaris*, both of which trees have "plank" buttresses to their trunks and leaves with thick cuticle and aqueous tissue. Below the arboreal

dominant is a tier about 15 m. high composed chiefly of Palms (*e.g.* Howea spp.), Screw-pines and a number of dicotylous trees. Lianes are here abundant, but epiphytes rare. Of the ground flora *Carex gracilis* and *Pteris comaris* may be particularised, but the floor of the forest is frequently bare of vegetation, a feature attributed in the case of the Acicalyptus Forest to the prevalence of pigs. Above an altitude of 300 m. the High Forest gives place to Mountain Low Forest, also of Palms, Screw-pines and trees, but with an abundance of *Hedyscepe canterburyana* instead of Howea spp. As we ascend, this upland vegetation is in turn replaced by Moss Forest characterised by a dense shrub layer above which project Palms, abundant Tree-ferns and *Dracophyllum Fitzgeraldi*. Tussock plants are common, and there is a great variety of ferns, but where the undergrowth is sparse the ground may be covered with mosses. The outstanding feature of the Moss Forest is, however, the universal abundance of epiphytes both Phanerogamic and Cryptogamic.

On exposed ridges and mountain cliffs the vegetation consists, as in our own country, of Scrub, which here reaches a height of from 1–2 m. Only occasionally is this sufficiently sparse to permit of ground flora; more usually it comprises a dense growth of mixed shrubs. Similar scrub, but of different composition, also occurs marginal to the Lowland Forest not far above high-water level.

On dry, rocky, exposed ridges of the northern hills, where scrub is not developed, a herbaceous growth is found which includes several members of the British flora, viz. *Cynodon dactylon, Gnaphalium leuteo-album, Oxalis corniculata, Polycarpon tetraphyllum* and *Sonchus oleraceus*.

The sand dunes are characterised by *Spinifex hirsutus* and *Ipomœa pes-capræ*. The author appends a complete list of the recorded species, together with notes upon their distribution and habitats.

According to Mr. Gordon ("Life History and Eradication of Bracken," *Trans. Highland and Agricultural Soc. of Scotland*, 1916), the Bracken now has a much wider distribution in Scotland than formerly. It particularly favours pockets of deep soil and southern slopes, but is intolerant of very moist or acid conditions. The rapid increase of this plant during recent years is attributed to reduction of the head of

cattle, injudicious burning of the heather and the absence during recent years of prolonged winter frosts. The methods to be adopted for its eradication are persistent cutting and spraying. The former is most effective if carried out during early July and again in August. But spraying with 2·5 per cent. to 5 per cent. solution of sulphuric acid, twice in the season, is not only more effective, but involves an outlay which is about one-third of that expended upon cutting.

Saxton has given a brief account of the vegetation in the neighbourhood of Manubic (*Trans. Roy. Soc., S.A.*, 1917) and describes the anatomy of the leaf in two typical Savannah grasses viz. *Erianthus Sorghum* and *Digitaria sanginualis v. ciliaris*. Both show a very xerophytic structure, the most striking feature being the prominent development of water storage tissue, especially in the latter species.

PLANT PHYSIOLOGY. By FRANKLIN KIDD, M.A., D.Sc., St. John's College, Cambridge. (Plant Physiology Committee.)

General Outlook.—The centre of activity in plant physiological research and development to-day is undoubtedly in the United States of America. It can easily be seen that in the near future the whole world will be faced with a very urgent necessity for the extension and organisation of the science of plant industry and production on a scale complementary to that of the mechanical revolutions of the past century. In this, as always, the game will be to those who are efficient and to those who are prepared. For the wise, who remark the signs of these days, the present is a time of stocktaking. In the business of review and generalisation, which is leading to the creation and staking out of new territory in questions of agricultural research and plant industry, not the least concerned is the plant physiologist, as the current scientific journals of different countries show. The following may be cited as examples from American botanical and plant physiological literature, indicating the general tendency now beginning towards a vital change of outlook.

Plant Ecology and its Relation to Agriculture, by Dr. Warren G. Waterman (*Science*, vol. xlvi. Sept. 7, 1917, p. 223). Great emphasis is laid on the fact that plant ecology largely overlaps what should be the field of the plant physiologist. The

branch of ecology, which the author puts in the first place and terms *autecology*, deals with the living plant as an individual and with its response under natural conditions to the various factors of its environment. The influence of environmental factors upon the various metabolic activities and upon growth is to be studied under this head, and secondly the changes occurring in the structure of plastic tissues. Proceeding, it is laid down that ecology, and in particular the plant physiological aspect of ecology, has an important function in agriculture, and it is pointed out that what has to be studied is "the preparation of optimal conditions," under which head the soil, the atmosphere, the selection and breeding of stock and the choice of time and location are involved; and, secondly, the "preservation of optimal conditions," which includes the control of soil moisture, light, temperature, root interference and disease. It is clear at a glance what a tremendous and almost untouched field lies here before the plant physiologist, especially when it is remembered that each crop and species will have to be studied separately and questions of selection and adaptation to climate also considered, all these being of first economic importance.

Liebig's Law of the Minimum in Relation to General Biological Problems, by Prof. Henry D. Hooker, Jr. (*Science*, vol. xlvi. Aug. 31, 1917).—This striking paper, contributed by a botanist of standing, is again an indication of the new and revitalising tendency which is carrying plant physiologists to the study of the living plant as a whole and to the investigation of its growth and metabolic response to environmental conditions in each phase of its life-cycle. The main content of the author's communication is that Liebig's Law of the Minimum, of which Blackman's theory of limiting factors is an application, has no meaning in relation to the final results of the great complex of processes making up the general metabolism of the plant. The theory of limiting factors has been of much use in the study of single plant processes, in particular as applied by Blackman in his researches upon carbon assimilation, and Prof. Hooker holds that it is justified, as an expression of the Law of the Minimum, when applied in this way to isolated processes. But when the experimenter comes to deal with the resultant of metabolic processes as expressed in growth and development, the Law of the Minimum no longer holds in

any consideration of environmental or internal factors. "The most interesting thing about this Law of the Minimum," says Prof. Hooker, "is not how it works, but when it does not work." There is a fundamental discrepancy between the Law of the Minimum and Galton's Law of Averages. In every case where Galton's law holds the Law of the Minimum does not hold. The resultant size or weight of an organism shows that these are not determined by the limiting factor of its environment. The factors giving the largest values are utilised to some extent to alleviate the influence of the limiting factors. Compensation or integration takes place. Prof. Hooker then outlines very briefly the results and outlook to be obtained from a realisation of this principle of integration. Response to stimuli, development, evolution, and biotic succession are the main integrating causes which will have to be considered in facing the physiological problems of the living plant,—its growth, maturation and general metabolism. When individual races and associations are concerned the limiting factor becomes merely the stimulus to which the whole system responds. The final result always approximates to an average of all the resources at disposal.

The Next Step in the Improvement of Wheat Cropping, by Prof. H. L. Bolley (*Science*, vol. xlvi. July 20, 1917).—We meet again here with the same demand for plant physiological organisation and development; and in this case in direct connection with its value as an applied science. The demand made, which, if carried into effect, Prof. Bolley considers would have such striking and beneficial results as to gain the immediate support of the agricultural community, is no less than that a large and trained body of physiologists should be put on a field-crop survey to locate seed of the highest weight and colour quality, free from disease and weather effects, and to locate also the various soils upon which such seed should be sown. Further, it would be necessary to see that the seed is procured and sown in suitable soils, and that the necessary disinfection of the seed is carried out.

The John Hopkins University Circular, 1917.—In this circular, under the head of "Contributions to Plant Physiology," from the Department of Plant Physiology, Prof. Burton E. Livingston, in an Introduction, outlines in a masterly manner the aims of modern plant physiology, and touches on the

methods of teaching and research adopted in the department to meet modern requirements and the new demand for a rigorous outlook growing up among plant physiologists. The teaching policy of the department is to place teaching as subsidiary and secondary to research. Membership is confined to graduates, preferably from other institutions, and the research is carried out on a general scheme in which all combine on a democratic basis. He points, in the first place, to the double scope of the application of Plant Physiology. In the general application of physiological knowledge to the formation of what may be called a philosophy of the universe, plant physiology, as a pure science, is almost if not quite as valuable as animal physiology. In its practical application, on the other hand, just as animal physiology debouches upon the fields of medicine, surgery, hygiene, animal husbandry, etc., so plant physiology contributes most to human physical welfare in the fields of agriculture, forestry, fermentation operations, bacteriology, etc. Prof. Livingston contrasts descriptive physiology with dynamic physiology, which relates life processes either individually or in their resultant to determining conditions within and without the plant. Owing to the complexity of the processes and the number of the conditions, each operating at so many possible intensities, dynamic plant physiology has remained a largely untrodden field. Methods have for the most part still to be devised. But it is from this side of plant physiology, which aims at quantitative control, that practical applications will spring.

The two main aspects of dynamic physiology, upon which attention has so far been concentrated in Prof. Livingston's Laboratory, are the *water relations* of plants and their *inorganic salt relations*. The problems involved are the simplest and yet the most fundamental in their bearing upon plant growth, and upon agricultural and forestal production. Their application is seen at once in questions of drainage, irrigation and fertilising practice. The principal conditions that affect the water loss from plants, together with methods and apparatus for their quantitative estimation, have occupied Livingston and his co-workers, and their results during the last few years have been noteworthy. The conditions as they are now defined by them are four: (1) the evaporating power of the air, (2) the intensity of absorbed radiant energy, (3) the internal transpiring power

of the plant, and (4) the water-supplying power of the soil. In the quantitative study of these controlling conditions, which is absolutely necessary, the porous-cup atmometer, the radio atmometer, the auto-irrigator have now been devised and perfected mostly in Livingston's Laboratory, while the method of relative transpiration, the cobalt chloride paper method, and soil osmometers have been employed, studied and modified.

With regard to the inorganic salt relations of plants, Livingston has turned his attention to water culture and sand culture. Both in his laboratory and elsewhere the whole question of water culture technique has come recently under a rigorous review. Lastly, "climate" has been analysed in its effect upon growth, and in an interesting series of papers an attempt has been made to work out a series of Climatic Indices for growth over the whole of the United States. They are based on reasonable approximations derived from meteorological data and on quantitative standard physiological experiments on growth-rates (*Phys. Researches*, 1916, vol. i., Nos. 8 and 9).

Among further American publications which indicate the modern tendency towards a new outlook in plant physiology, the following must be referred to: *Plants as a Physical System*, by L. J. Briggs (*Journ. Washington Acad. Sci.* 1917, **7**, p. 89); *Imbibition and Growth*, by MacDougal & Spoehr (*Proc. American Phil. Soc.* 1917); and *A Quarter-century of Growth in Plant Physiology*, by B. E. Livingston (*The Plant World*, 1917, **20**, p. 1–15). Under the heading of general outlook certain publications in this country also are of importance—e.g. *Carbon Assimilation*, by Ingvar Jörgensen and Walter Stiles (William Wesley & Son, 1917); *Soil Conditions and Plant Growth*, by E. J. Russell (Third Edition, London; Longmans, 1917) and *Analysis of Agricultural Yield. III. The Influence of Natural Environmental Factors upon the Yield of Egyptian Cotton*, by W. L. Balls (*Phil. Trans.* 1917, **208**, B., and also Parts I. and II. *Phil. Trans.* B. 1915). In Dr. Balls' work, devoted to a single crop over a span of years and to an experimental analysis of the conditions controlling its growth and production throughout its life, we come at once into the presence of the new aspect of plant physiology, to which I have alluded as about to emerge, and which may be called the Science of the Living Plant. The results obtained do not fail to impress the reader. Amongst

the most striking is the discovery that the daily and weekly fluctuations in the number of flowers (upon which, of course, the cotton yield depends) are directly controlled by weather conditions which operated *a month before the flowers opened.* Obviously here we have a new principle, of unknown importance and implication, in the physiological study of the living plant. The author employs the term *predetermination* to characterise it. The work of Dr. Balls, which deals also with soil-fertility, soil texture, soil depth, shortage of soil-water, over-watering, root-asphyxiation and climate, will go far to emphasise the importance and fruitfulness of the change in general outlook to which plant physiology is rapidly advancing.

With regard to further recent plant physiological papers and research results, reference should be made to "Physiological Abstracts," 1917, vol. ii. nos. 1–8 in which some 500 short abstracts have appeared since April last.

The general conclusion to be drawn from a survey of recent advance in plant physiology appears to be that the subject is about to become one of the basal pure sciences underlying a complex system of industrial application in the business of plant growth, cultivation, and cropping. But it is also clear that the plant physiology of to-morrow will scarcely be recognisable in relation to the plant physiology of yesterday, and even of to-day. The plant physiology of the past fifty years, in many quarters, may well be likened to that type of adult which is spoken of as having never seen a childhood. It has, in the scientific world, scarce been recognised as a distinct science at all. There is even as yet no single British scientific journal, among hundreds devoted to other particular aspects of research, devoted to it. Its followers have been largely made up of botanists, morphologists, systematists and biochemists who have turned their attention to physiological investigations upon isolated processes and isolated organs in a manner mimicing the parallel researches of animal physiologists. In the result the subject is now out of touch with the living and growing plant, developing under natural and healthy conditions. There exists an enormous and almost untrodden realm of physiological observation and experiment concerned with the living plant as a whole, and in relation to its environment throughout its complete life-cycle. It is such observations which must form the basis of a true science of plant physiology,

and it is such observations which for the most part have still to be made.

ZOOLOGY. By CHAS. H. O'DONOGHUE, D.Sc., F.Z.S., University College, London.

Protozoa.—The papers include "Observations on Blastocystis Hominis" by Swellengrebel (*Parasit.* July 17) and "Observations on *Entamœba gingivalis* from the Human Mouth, with a note on the Trichomonad Flagellate *Tetratrichomonas buccalis,*" by Goodey and Wellings (*ibid.*).

Invertebrata.—Stephenson has furnished a useful paper "On the So-called Pharyngeal Gland-cells of Earthworms" (*Quart. Jour. Micro. Sci.* vol. 63, August 17). These cells are not gland-cells in the ordinary sense of the word, and do not communicate with the pharynx; it is therefore proposed to call them "chromophil cells." They contain an unequally distributed deeply staining substance and are incompletely covered with a sort of capsule of flattened cells. "The cells are to be looked on as of peritoneal origin; that is to say, they are modifications of the original lining cells of the cœlonic cavity. Hence the absence of a capsule in the early stages; and hence the original limitation of the cells to the superficial portion of the pharyngeal mass. The main function of the cells is probably metabolic."

Other papers include: "Some Cestodes from Japanese Selachians, including Five New Species," by Yoshida (*Parasit.* July 1917), and "Sur Quelques Nématodes des Oiseaux de la Russie," by Skrjabin (*ibid.*).

"Notes on Asteroidea" and "A New Genus and Sub-genus of East Indian Sea Stars," both by Fisher (*Ann. and Mag. Nat. Hist.* August 17), and "Multiplication by Fission in Holothurians," by Crozier (*American Nat.* September 17).

Cameron has carried out an intensive study of "The Insect Association of a Local Environment Complex in the District of Holmes Chapel, Cheshire" (*Trans. Roy. Soc. Edin.* vol. lii. April 17), and he summarises his observations as follows: "In any given locality the composition of insect association is determined by a complex of factors, which may be classified as follows, viz. physical, physiographical, topographical and vegetational. Primarily it is dependent upon the ecological type of the vegetation." An enumeration of the insects

inhabiting the soil throws considerable light on the habits of ground-feeding birds and a comparison of the fauna of two areas differing in soil and vegetation helps to solve the question as to the amount these differences can be explained by varying edaphic factors. The criterion of the true surroundings of an insect is taken to be the place in which it breeds.

"The Relation between the Hatching of the Eggs and the Development of the Larva of *Stegomyia fasciata* (*Aëdes calopus*) and the Presence of Bacteria and Yeasts" has been fully investigated by Atkin and Bacot (*Parasit.* July 17). It is found that moulds are of no use to the developing larva, and indeed detrimental, probably owing to the fact that they utilise the available food if not actively harmful. The larvæ on the other hand feed on bacteria and yeasts, which form a sufficient diet even in the absence of other foods. They grow very poorly in the absence of such organisms, and it is practically impossible to bring them to the adult stage in sterile solutions. This information may be of service in keeping down this species of mosquito, and conversely it is certainly useful to the Sanitary Inspector who wishes to hatch out his flies, and failure to do this in the past has probably resulted from the water being deficient in bacteria. Other papers include: "The Chromosome Complex of *Culex pipiens*, Part II. Fertilisation," by Taylor (*Quart. Jour. Micro. Sci.* vol. lxii. August 17), and "Notes on Nycteribiidæ, with Descriptions of Two New Genera," by Scott (*Parasit.* July 17). The last gives an account of these bizarre and extremely highly modified Diptera which are only found parasitic on Bats. It not merely gives a description of the new genera, but also a very useful summary of the biology of these strange creatures, their habits, their reproduction and general life-history.

Other papers include: "A Revision of the Wasps of the Genus *Tachytes* inhabiting the Ethiopian Region" (*Ann. and Mag. Nat. Hist.* July 17) and "Notes on the *Braconidæ* in the British Museum" (*ibid.* September 17), both by Turner; "Descriptions and Records of Bees," by Cockerell (*ibid.* September 17); "Notes on the Hymenopterous Families *Bathylidæ* and *Rhopalosomidæ*," by Turner and Waterston (*ibid.* July 17); and "New Records of Natal Bees," by Cockerell (*ibid.* September 17).

"The Cytoplasmic Inclusions of the Germ-cells," by

Gatenby (*Quart. Jour. Micro. Sci.* vol. lxii. August 17) is a well-illustrated account of these little-known bodies as they occur in Lepidoptera, with particular reference to *Smerinthus populi* and *Pieris brassicæ*, and sets forth several interesting results. The micromitosome is very probably present in the primordial germ-cell, for it has been traced back with certainty as far as the secondary spermatocyte, and is also found in the female cell. It apparently divides in all cell divisions. The mitochondria are at first similar in the male and female cells, but they meet with a different fate subsequently in the male. Here the mitochondral body is remarkable for the formation of chromophobe and chromophile zones, whose purport is fully discussed. Various remarkable changes in the macromitosome of the sperm are recorded. Acroblasts have been demonstrated in early growth stages of the spermatocyte, and an account given of the complex changes that they undergo during the various mitoses as well as their probable origin. The centrosome in the early spermatid divides into two, and, as far as the available evidence goes, one of them appears to be lost.

The same author, writing on a similar theme, describes "The Degenerate (Apyreme) Sperm-formation of Moths as an Index to the Inter-relationship of the Various Bodies of the Spermatozoon" (*ibid.*).

Other papers include: "New Heterocera from Dutch New Guinea" and "New Races and Aberrations of *Heliconius*" (*Ann. and Mag. Nat. Hist.* July 17), and "New Lepidoptera from Waigen, Dutch New Guinea, and Biak" and "Two New Species and New Genus of *Sphingidæ*" (*ibid.* September 17), all by Joicey; "Descriptions of New Pyralidæ of the Sub-families *Hydrocampinæ, Scoparianæ,*" by Hampson (*ibid.*); "New Geometridæ in the Joicey Collection," by Prout (*ibid.* July 17); "New Indo-Malayan Species of Lepidoptera," by Swinhoe (*ibid.* August 17); "Some Remarkable Melanic Aberrations among the *Aeræinæ* in the Millar Collection of Butterflies of the Durban Museum," by Barker (*Ann. of the Durban Mus.* vol. i. July 17); "Some apparently Undescribed South African Heterocera," by Janse (*ibid.*).

"Note on the Sub-genus *Paradownesia* Gestro," by Maulik (*Ann. and Mag. of Nat. Hist.* July 17); "A Systematic Revision of the African Species of the Coleopterous Family *Erotylidæ*," by Arrow (*ibid.* August 17); "Descriptions of some Ethiopian

and Australian Homoptera," by Distant (*ibid.*); "On the Occurrence of a Pseudoparasitic Mite (*Cheletiella parasitivorax*, Méguin) on the Domestic Cat," by Hirst (*ibid.* July 17); "Remarks on Certain Species of the Genus *Demodex* Owen (the Demodex of Man, the Horse, Dog, Rat and Mouse)," also by Hirst (*ibid.* September 17).

An account of certain "Experiments and Observations on Crustacea. Part IV. Some Structural Features Pertaining to *Glyptonotus*," by J. Tait (*Proc. Roy. Soc. Edin.* vol. xxxvii. July 17). The large size of this Isopod renders it a very suitable object for elucidating certain points in its functional anatomy that are not readily determinable in smaller species. It is suggested that the median split in the thoracic segments is to allow of a distension of the body. The cephalosome, consisting of the head and two thoracic somites, contains a well-developed endophragmal skeleton which constitutes a strong maxillo-sternal framework capable of independent movement. *Glyptonotus* is carnivorous, and the fore-gut is not provided with a gastric mill, but involutions of its wall aid in the swallowing of food. The same author has also provided "Experiments and Observations on Crustacea, Part V., a Functional Interpretation of Certain Structural Features in the Pleon of Macrurous Decapods" (*ibid.*).

Other papers include: "The Malacostraca of Durban Bay," by Stebbing (*Ann. of the Durban Mus.* vol. i. July 17).

Vertebrata.—"The Homologies of the Muscles related to the Visceral Arches of the Gnathostome Fishes," are discussed at length in a well-illustrated paper by Allis (*Quart. Jour. Micro. Sci.* vol. lxii. August 17). It appears probable that primitively the muscles of the visceral arches in Gnathostome Fishes were represented by a simple constrictor muscle in each arch, which had associated with it a branchial bar lying on its internal side. Two distinct lines of differentiation lead from this condition, one represented by the Teleostomi and the other by the Plagiostomi, while the Holocephali and Dipneusti are apparently intermediate between them to some extent. The four typical elements of the bar in Teleostomi are arranged in practically a single plane. In the Plagiostomi, however, we find the dorsal and ventral elements directed postero-mesially at a marked angle to the middle and so forming a sigma-shaped bar. In

Holocephali and Dipneusti, the dorsal elements are directed postero-mesially and the ventral ones antero-mesially. The constrictor muscle is found in its most primitive condition in the Selachii. In Teleostomi "the dorsal and ventral ends of the constrictores become the levatores and the transversi and obliqui dorsales and ventrales. The levator of the ultimate arch is a slender muscle, and may secondarily acquire insertion on the shoulder girdle. It is the homologue of the large musculus trapezius of the Selachii. The ventral portion of the constrictor of the ultimate or fifth branchial arch of the Teleostomi is modified."

Other papers include: "Additions to the Fish Fauna of Natal," by Regan (*Ann. of the Durban Mus.* vol. i. July 17);

"Remarks on the Midwife Toad (*Alytes obstetricans*), with reference to Dr. P. Kammerer's observations," and "On a Second Species of the Batrachian Genus Amphodus," both by Boulenger (*Ann. and Mag. Nat. Hist.* July 17);

"The Moulting of the King Penguin (*Aptenodytes patagonica*)," by Ewart and Mackenzie (*Trans. Roy. Soc. Edin.* vol. lii. June 17).

Mummery has given an account of his investigations "On the Structure and Development of the Tubular Enamel of the *Sparidæ* and *Labridæ*" (*Phil. Trans. Roy. Soc.* vol. B, 208, September 17). The enamel of the teeth in certain fishes shows a strongly marked striation from the outer surface, the striæ not reaching to the dentine layer. This paper is an investigation of these striate structures and their functional significance. They are shown to be produced by a regular series of tubules quite independent of the enamel tubules, suggesting a closer analogy with the true enamel of the osseous fishes. There appears to be considerable evidence to show that the tubes have a calcifying function, and are probably concerned with the conveyance of the inorganic salts to the organic matrix of the enamel; granules having actually been demonstrated in the tubes. Considerable difference is encountered in the structure of the enamel organ in the *Labridæ*, but there is no doubt it is a secreting organ.

Other papers include: "The Groups of the Small and Medium-sized South American Felidæ," "A New Genus of Ursidæ" (*Ann. and Mag. of Nat. Hist.* July 17) and "The Genera of *Hapalidæ* (Marmozets)" (*ibid.* September), all by Pocock;

" The Geographical Races of *Galago crassicaudatus*," and " On Small Mammals from the Delta of the Parana " (*ibid.* July 17) " On the Arrangement of the South-American Rats allied to *Oryzomys* and *Rhipidomys* " and " Two New Rats of the *Rattus confucianus* Group " (*ibid.* August 17); " A New Agouti from the Moon-mountains " (*ibid.* September 17), all by Thomas: " A New Musk-like *Mustela* from Java," by Robinson and Thomas (*ibid.*): " A New Bat of the Genus *Otomops* obtained at Durban," by Chubb (*Ann. of the Durban Mus.* vol. i. July 17); " Evidence of Multiple Factors in Mice and Rats," by Little (*American Nat.* July 17).

General.—" Rats and Evolution " is a subject dealt with by Hagedoorn and Hagedoorn (*Amer. Nat.* July 1917). After a full investigation of the various species of rats and their occurrence, the authors conclude: " All those causes which tend to reduce the potential variability of a group of organisms tend to make varieties or species. Such causes are isolation, migration, adaptation, selection and especially the fact that, either periodically or regularly, the number of individuals of one generation is very much smaller than that of the preceding one. This cause of purification of the type which we see in operation everywhere (think of the numbers of house-flies a year in the first and last generations) operates quite regardless of adaptation or fitness. To this cause, working upon variation, may be ascribed numerous characteristics for which we can invent no earthly use, and for which nevertheless species are pure."

Morgan treats of " The Theory of the Gene " (*ibid.* September 1917). This paper attempts to define and explain what is meant by a genetic factor and at the same time to meet certain criticisms that have been put forward against various of the hypotheses in genetics. It is pointed out that it is necessary to bear in mind that the mechanism of the distribution of the genes is to be differentiated from the effect produced by the genes through the embryo. The cytoplasm also takes a considerable part and is closely bound up with the environment, which may even alter its constitution. Consequently it leads to confusion to attempt to deduce the mechanism of distribution from the nature of the embryonic reaction.

Other papers include: " Differentiation by Segregation and Environment in the Developing Organism," by V. Danchakoff (*ibid.* July 1917); " Biocharacters as Separable Units

of Organic Structure," by Osborn (*ibid.* August 1917); and "Studies on Inbreeding. VII. Some Further Considerations Regarding the Measurement and Numerical Expression of Degrees of Kinship," by R. Peare (*ibid.* September 1917).

PALÆONTOLOGY. By W. P. PYCRAFT, F.Z.S., A.L.S., F.R.A.I., British Museum (Natural History), South Kensington, London.

THE interpretation of the strange, spiral clusters of teeth belonging to certain primitive Palæozoic sharks—Campodus, Helicoprion and Edestus—is a task of no small difficulty, but a material advance has been made this year in the decipherment of these puzzling relics by Dr. A. Smith Woodward (1), whom Fortune favoured by throwing in his way a finely preserved fragment of a jaw—apparently the pterygoquadrate—with its attached teeth, belonging to a species of *Edestus* which proves to be new to Science, and to which has been assigned the name of *Edestus newtoni.* The value of this find, however, lies not in the fact that it adds a new name to the number of known species, nor to the fact that only once previously have such teeth been found attached to their supports; but in the clue it has afforded as to the evolutionary history of these singular forms of teeth.

In more than one instance there have been found with these teeth other small "orodont" teeth whose presence, in such an association, has been regarded as accidental. Hence the smaller teeth were ascribed to a totally different fish, to wit, *Campodus*, or *Agassizodus*. Such teeth are present in the fragment now under consideration, and Dr. Smith Woodward expresses his conviction that they are not only part of the same dental system, but that they furnish the guide to the course of evolution which has given rise to the larger, spirally coiled, symphyseal teeth. He shows, in short, that the teeth of the *Edestus* type are to be regarded as derived from the *Campodus* type, and in this transition four well-marked phases are to be seen. The first stage begins with the enlargement of the middle part of the crown, and the reduction of its sides: the second shows a further increase in the size of the crown, and an enlargement of the hinder portion of its root. In the third stage the crown consists of no more than the laterally compressed middle part of the tooth. In the final stage the

root is of enormous size, having apparently grown at the expense of the crown, which is now reduced to a quite subordinate position. This last stage is seen in the teeth of *Edestus minor*.

The new species, which was obtained from the Millstone Grit of Brockholes, Huddersfield, represents an extremely generalised type of *Edestus*, the most primitive yet discovered. Hence the light it throws on the peculiarities of succeeding genera and species.

We are indebted to Dr. Smith Woodward for enlightenment this year on the teeth of yet another group of fishes—the Pycnodonts. These, the "coral-fishes" of Mesozoic seas, bear a striking superficial likeness to the coral-fishes of our own seas; a likeness, however, to be ascribed to adaptation to similar modes of feeding and environment. The incisor teeth are closely similar to those of living coral-fishes; and this likeness in the matter of the dentition, it is now pointed out (2), extends also to the claw-shaped pharyngeal teeth such as are found, for example, in *Balistes*. Thus it comes about, Dr. Woodward points out, that we must regard the isolated pharyngeal teeth known to Palæontologists as those of *Ancistrodus* as the prehensile teeth of Pycnodonts; so that "*Ancistrodus*" must henceforth disappear from our text-books.

For some years past Mr. Barnum Brown has been assiduously adding to our knowledge of the Ceratopsia. During the present year he has described (3) the skeletons of two species new to Science, of the horned dinosaur *Monoclonius*, from the Belly River formations (Upper Cretaceous). An instructive insight into the habits of these huge reptiles is afforded by his remark that while there is no record of two or more specimens of *Triceratops* being found together, no less than eleven individuals of *Monclonius* and *Ceratops* have been found intermingled, suggesting that the Belly River genera were probably gregarious. Of the species to which he assigns the name *Monoclonius cutleri* only the posterior half of the skeleton was recovered, but in association with the femur was found a large piece of tuberculated skin which apparently formed part of a ventral armature.

The second species, to which he has given the name *M. nasicornis*, is represented by a complete skeleton, and this has enabled him to give a restoration showing the appearance of

the animal during life. This differs, in several important particulars, from that given years ago by Marsh of the nearly related Ceratops. The most noticeable features in the new restoration are the greater depth of the body above the hips, and the more digitigrade form of the feet wherein the toes turn outwards.

In 1903 Prof. Henry Fairfield Osborn described a small bipedal, cursorial dinosaur, newly discovered in the Bone-cabin quarry, Wyoming. At that time he was convinced that these remains were those of a bird-catching dinosaur, which accordingly he named *Ornitholestes*, and he supplemented his description by a lively restoration of the creature in the act of seizing an *Archæopteryx* with its forefeet. Recent discoveries of allied dinosaurian forms have caused him to re-examine these feet, and as a result he now modifies his original conceptions and restoration of this singular reptile. The recent discoveries just alluded to were made after a searching examination of the complete skeleton of an " Ornithomimine " disosaur which was acquired by the American Museum of Natural History in 1914. This specimen came from Hell Creek, Montana. For more than a quarter of a century the remarkably bird-like hind limbs had been known, and now, at last, came the rest of the skeleton. The discovery of the skull, remarks Prof. Osborn (4), " occasioned one of the greatest surprises in the whole history of vertebrate palæontology, because it proved that both in head and limb structure this animal was non-raptorial." Surprise is largely a matter of temperament ; Prof. Osborn, for years past, has spared no effort to obtain the complete skeleton and he had already made up his mind as to the form it would present, even almost to details. Hence doubtless it was rather disconcerting to find edentulous jaws, instead of serried ranks of teeth. But a similar loss of teeth has occurred over and over again in all sorts of animals. The fore-limbs, however, most certainly are remarkable, though not more so than those of *Ornitholestes*, and we venture to think it will be long before any satisfactory interpretation of their function is arrived at. At present the problem defies solution. Prof. Osborn, in his *Memoir*, adds to his own the attempts of his colleagues to portray this animal as in life, but they are none of them at all convincing. But in fairness to Prof. Osborn it must be stated that they are given merely as suggestions. No greater

puzzle has ever presented itself to the palæontologist than this, so that if only as a record of its existence this *Memoir* will be treasured, and we must all be grateful to him for the lucid and admirable way in which he has presented his facts.

The restoration of extinct animals presents many pitfalls. How diverse are the results obtainable from the same material is well illustrated in the case of that huge dinosaur *Diplodocus*. In the British Museum of Natural History its skeleton, mounted by Dr. Holland of the Carnegie Institute, is posed after the manner of an elephant, the limbs being placed in a vertical plane. Though there be some who consider this to be correct, there are many who think otherwise. Among these is the Rev. H. N. Hutchinson, who has recently constructed a model posed after the fashion of the typical reptile, with the belly but just raised off the ground and segments of the limbs bent sharply upon one another. On the whole this attempt is more convincing than its predecessor, but it still leaves much to be desired. Happily, Mr. Hutchinson has placed his views on this subject on record (5), illustrating his remarks by numerous text figures, and two plates. At the same time he summarises the views of others who have written on this theme, without apparently giving any very serious thought to the problems presented!

Only two skeletons of that most remarkable fossil bird *Archæopteryx*, it will be remembered, have ever been found. The first brought to light was acquired, more than half a century ago, by the British Museum, and was described by Owen. The second specimen, discovered some years later, now rests in Berlin. Dame's account of it was very thorough, but it was not till many years after—twenty years ago, to be precise—that he decided to expose the portions of the pelvis which yet lay beneath the surface of the slab. This year Dr. Smith Woodward decided, in like manner, to expose the hidden portions of the pelvis in our own specimen. His account of what he found (6) will be read with lively satisfaction by all who are interested in such themes. Briefly, we now know the posterior portion of the ilium must have been cartilaginous during life, while the pubes, which were long and slender bones, were united terminally by a long symphysis. Thus although this pelvis is wholly avian in character, in this particular it is

unique. But Dr. Woodward's examination did not end here. He also dug out the shoulder-girdle, which presents features of exceptional interest, more especially in regard to the coracoid, which is again unlike that of any other known bird, and recalls that of the reptile. The new facts gleaned from this exploration will doubtless form the starting-point for further investigations in regard to the course of evolution of the typical avian shoulder-girdle. Dr. Smith Woodward's investigations were carried out in conjunction with Dr. Branislav Petronievics, who is preparing an extensive *Memoir* on the subject.

The Bighorn basin of Wyoming (Lower Eocene) has at last revealed to us what manner of bird was the *Diatryma*. Since 1874 palæontologists have known of the existence of a giant bird rivalling all but the largest Moas in size, but no more than fragments, obtained at various places, and at rare intervals, were known till last year, when this tantalising state of affairs was ended by the discovery of a nearly complete skeleton, including the most desired element of all, the skull. An able account of these remains has now been published by Dr. W. D. Matthew and Mr. W. Granger (7). As was to be expected, *Diatryma* was flightless, and the wing-bones prove to be as degenerate as in the modern Cassowary, or Emu. But no one would have anticipated a skull so remarkable as this bird possessed. It was of enormous size, and bears a most striking resemblance to that of the singular South American *Phororhacos*, with which, however, it seems to have no very close affinity. The authors have shown that it is in no way related to the Struthious birds, though its precise position among the Neognathæ is still indeterminable. While the authors admit at least remote relationship with the *Cariama*, they refuse to regard it as a member of the Gruiformes, a conclusion which is somewhat contradictory. They hold that it must be assigned an Order to itself, a proposition which is reasonable enough, but its adoption does not rule out the possibility of Gruiform affinities.

The importance of the kitchen-middens of ancient man cannot be overrated. Mr. Gerrit Miller, of the Smithsonian Museum, Washington, has shown, for example (8), that the bones they picked and threw aside not only furnish us with valuable information in regard to animals now extinct, but may

also enlighten us as to ancient land areas now submerged. His analysis of bones of mammals from Indian sites in Cuba and Santo Domingo will, for this reason, be valued not only by the systematist, but also by those who are concerned with these wider problems. Mr. Miller has himself, in this paper, which is chiefly concerned with the bones of extinct rodents, drawn deductions of great interest as to the former connection of these islands with the mainland.

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ANTHROPOLOGY. By A. G. Thacker, A.R.C.Sc.

A new periodical was founded last year in the United States, which is to be specifically devoted to the problems arising out of the contact of the negro and the white man, more particularly, of course, the questions which thus confront America. The new publication is entitled *The Journal of Negro History*, is published quarterly, and is under the editorship of Mr. Carter G. Woodson. It is to be sold at the modest price of one dollar per annum.

The *Journal of the Royal Anthropological Institute* for the second half of 1916 (vol. xlvi. part ii.) contains a number of articles of considerable interest to the student of social, as distinct from physical, anthropology. The first contribution is the Huxley Memorial Lecture for 1916, which was delivered by Sir J. G. Frazer, and dealt with the subject of "Ancient Stories of a Great Flood." The lecturer described in detail

the Babylonian, Hebrew and Greek legends, and he quoted the Biblical story at length, separating it into its two component elements. The Babylonian legend, it may be noted, was derived from a Sumerian source, and was thus extremely ancient. Mr. H. D. Skinner contributes to the same number of the Journal the second instalment of his review of "The Evolution of Maori Art," and deals this time with pendants. Three articles deal with different aspects of the ethnology of Papua. These are "The Magic of the Kiwai Papuans in Warfare," by Gunnar Landtman; and "Baloma: the Spirits of the Dead in the Trobriand Islands," by Dr. Bronislaw Malinowski, of Cracow; and the "The Kabiri, or Girara District, Fly River, Papua," by Dr. A. C. Haddon. This Kabiri district is a swampy area lying between the Fly River and the Aramia affluent of the Bamu, and it is flooded for the greater part of the year. The villages of the primitive natives are built on the low hillocks which intersect the region, some protection from the damp thus being obtained. Communication between the hillocks is kept up by means of canoes. The Journal also contains a short but highly interesting paper by R. S. Rattray describing some archæological discoveries which he made in Togoland, under the title "The Iron Workers of Akpafu."

As is so often the case, the book-reviews are some of the most interesting features in recent numbers of *Man*. The reviews are in many cases hardly less than short essays. In particular, a review by Prof. A. Keith of Osborn's *Men of the Old Stone Age*, in the May number, should be mentioned; and also a review of *In Far North-East Siberia*, by I. W. Shklovsky, in July, and another on Westervelt's *Hawaiian Legends of Volcanoes*, written by Dr. E. S. Hartland, also in the July number. In *Man* for May Prof. T. E. Nuttall has an article on the Piltdown Skull, in which he attempts to adjudicate on the famous controversy about that relic, without, however, entering into any very profound details. On the whole, he inclines towards Prof. Keith's views of the cranial features rather than towards Dr. Smith Woodward's. In the same number Harold Peake replies effectively to some criticisms of his paper on "Racial Elements concerned in the First Siege of Troy," to which I referred in these notes last October.

Volume III. of *Memoirs of the American Anthropological Association* includes some interesting papers, as will be seen

from the titles: (1) "The Idea of Fertilisation in the Culture of the Pueblo Indians," by H. K. Haeberlin. (2) "The Indians of Cuzco and the Apurimac," by H. B. Ferris. (3) "Mocassins and their Relation to Arctic Footwear," by Gudmund Hatt. (4) "Banaro Society. Social Organisation and Kinship System of a Tribe in the Interior of New Guinea," by Richard Thurnwald. The first number of Volume IV. (that is, January to March, 1917) is, however, of more general anthropological interest, and consists of a long paper by Dr. E. Sidney Hartland on "Matrilineal Kinship, and the Question of its Priority." The writer begins by giving a clear history of the study of Mother-right, from the time of the publication of J. J. Bachofen's work, *Das Mutterrecht*, in 1861. The theory that the matrilineal organisation of society is more ancient than the patriarchal state was also worked out about the same time and apparently quite independently by McLennan and L. H. Morgan, and it has since been very widely accepted. The question is, however, whether the matrilineal condition has priority universally or whether there are some peoples who constitute exceptions to the general rule. Dr. Hartland considers in detail the chief cases, amongst the Central Australians and certain North American tribes, which have been claimed as exceptions, in that their manners and customs are said to reveal no traces of an earlier condition of mother-right. Dr. Hartland comes to the conclusion that these supposed exceptions will not bear examination, and that the matrilineal organisation is almost certainly the primitive condition amongst all peoples.

ARTICLES

THE DENSITY OF LIQUIDS

By JOSEPH REILLY, M.A., D.Sc. (N.U.I.), and Prof. W. N. RAE, M.A. (Cantab.)

One of the most frequent and important of the physical determinations carried out in the laboratory is that of the density of liquids and solutions. The chief methods in use are (*a*) the pyknometer method; (*b*) the method of hydrostatic weighing; (*c*) the hydrometer method. By the pyknometer method it is possible to obtain the density of a liquid correct to one part or even less in a million, when great precautions are taken in weighing, in regulating temperature and in allowing for the effects of humidity. By Lamb and Lee's (1) refinement of the hydrostatic method it is possible to obtain results correct to one unit in the seventh decimal place. It is questionable, however, whether the amount of impurity in a liquid can ever be reduced to such a small amount. The hydrometer lends itself to a degree of accuracy which does not exceed one part in a thousand.

In making up solutions for density determinations, the components must be always weighed out, and in the calculation of the components of the solution, weights corrected to vacuum should be used. Methods, such as fractional distillation, freezing, etc., to be used in purifying the substances will be dictated by their chemical nature and the nature of the impurities known or suspected to be present. No general rules can therefore be laid down, except that it is advisable to consult the literature on previous determinations with the same substance, before deciding on the method of purification to be adopted. Suitable precautions must also be taken to prevent contamination after purification, *e.g.* with moisture in the case of hygroscopic substance.

The vessel used in the pyknometer method is some form of specific gravity bottle; a large number of different forms have been suggested, of which the most useful are perhaps:

(i) A modified form of the Gay-Lussac S. G. bottle described below; (ii) Wade and Merriman's form of the Gay-Lussac pyknometer; and (iii) Bousfield's pyknometer.

(1) The ordinary specific gravity bottle, with the addition of a ground-on cap covering the stopper and neck, gives very satisfactory results when used with care. The ground-on cap is an advantage; it prevents a volatile liquid from losing an appreciable weight in the balance case. Experiment seems to show that evaporation occurs less by way of the capillary in the stopper than by the ground-joint between the stopper and the neck. When the determinations are made below the room temperature, allowance must be made for the expansion of the liquid as it is heated to the temperature of the balance-case. This is provided for by a small cylinder ground to fit on to the end of the stopper and small enough to go inside the cap. By using the apparatus in the manner to be described, no difficulty should be experienced in obtaining agreement to the fifth place.

The liquid for density determination is prepared as follows. As much as possible of the dissolved air must be removed, since, apart from any possible effect on the density, when the liquid is put in the warm thermostat there is a tendency for dissolved air to be evolved, and to cling to the glass in the form of very minute bubbles. These are difficult to remove, and, if allowed to remain, a low value for the weight of the liquid is obtained. One method of removing the air is to place a beaker containing the liquid in a vacuum desiccator attached to the water-pump by means of a safety device made by fitting to a flask a rubber stopper through which passes a vertical tube about 33 ins. long, bent down parallel to itself. Mercury is put in the filter-flask, the side-tube is connected to the pump, and the other to the desiccator. The latter is then cut off from the moist air. At the same time, should the water pressure fail, the water cannot be sucked back into the desiccator. From ten to twenty minutes is sufficient to remove the greater portion of the dissolved air. The liquid is then poured into the weighed S.G. bottle (previously thoroughly cleaned and dried—the usual precautions having been taken) until it is level with the top of the neck (taking care that no air-bubbles are left), and the stopper is inserted. The stopper should always be in the same position relative to the neck,

advantage being taken of natural or artificial marks to ensure this condition, and undue pressure should be avoided (some workers use exactly the same pressure each time by putting the bottle on a compression balance and pushing the stopper down so that the pointer of the balance moves to a definite mark (4), but we have not found this to be necessary). For lower temperatures the cup is placed on the stopper and is half filled with the liquid ; this is omitted for higher temperatures than that of the room. The cover is replaced, and the bottle is fastened to a metal holder which keeps it in a vertical position in the thermostat. The holder consists of two parts made to slide on a metal rod about eight inches long and held in position by small thumb-screws. The lower part consists of a short support to which are attached three short arms bent upwards and covered with rubber tubing. The upper piece consists of a short support terminating in two rubber-covered arms which encircle the upper part of the bottle below the neck : the holder must not be fastened so tightly as to deform the bottle. The time allowed in the thermostat depends on the accuracy desired ; if accuracy to the fifth place is required, the temperature of the liquid must not differ from that of the bath by $\frac{1}{100}$ of a degree, and one hour is generally sufficient before the final adjustment is made (Wade and Merriman recommend two hours) ; (2) to obtain results accurate to the fourth place, twenty minutes to half an hour is quite enough. The cap is now removed and carefully dried both inside and out. The excess of liquid on the top of the stopper is then removed so that the meniscus is just visible on looking through the stopper from the side, and the top is quite dry. The bottle is now taken from the bath and quickly dried round the neck, and the cap replaced. The outside of the bottle is wiped with a cloth moistened with alcohol, dried, and the bottle placed in the balance-case, and after half an hour weighed with the usual precautions (7). The liquid is then poured out and the bottle cleaned, the final washing being done with distilled water. It is filled with water from which the dissolved air has been removed by connecting with the filter pump ; the weight of water is then obtained in exactly the same method as was used in obtaining the weight of liquid. If the highest accuracy is desired, it is necessary to determine the water content on each occasion,

otherwise slight errors may occur owing to a change in the weight or in the volume of the bottle, due either to a reduction in the size of the stopper by the repeated grinding, or to a slow change in volume similar to that observed in thermometers, or to a slight change in the temperature of the thermostat. Silica pyknometers are sometimes used, and they possess some advantage in certain kinds of work. A specific gravity bottle with a double-bored stopper which allows ready adjustment of level of liquid and minimum exposure to air, is sometimes used (Tate's modification). This form of apparatus has advantages when hygroscopic and volatile liquids are used.

It may be of interest to refer to the method of determining the density of a liquid at its boiling point. The pyknometer used for such determinations consists of a thin glass bulb preferably of Jena glass, capacity 2–3 ccs. The bent tube attached is of capillary bore. The tube, after cleaning and weighing, is filled as follows: the bulb is first heated and then cooled, and placed in a vessel containing some of the heated liquid of which the specific gravity is required: after a short time the bulb is partly lifted out of the liquid, the open end of the capillary tube being still under the liquid; liquid is thus forced into the bulb as the latter cools. The liquid in the bath is now heated to a higher temperature and the operation of heating and cooling the bulb repeated. This operation is carried out several times with the bath at a higher temperature each time. The bulb is finally obtained full except for a minute air-bubble. Sometimes filling is done by placing the apparatus in a small vessel which can be partially exhausted, and by dipping the open end of the capillary under the liquid contained in the vessel. On allowing air to enter the latter, some liquid will have entered the pyknometer. This operation is repeated several times until the bulb is practically full.

The pyknometer is suspended by a platinum wire in a wide-mouthed boiling flask so that it is just above the surface of the liquid (this liquid being the same as that in the pyknometer). The flask is next heated until the liquid it contains is raised to its boiling point. With the increase in temperature, the liquid in the pyknometer expands, and all the small air-bubbles are expelled; the boiling is stopped when the pyknometer and its contents have reached a constant tem-

perature; the apparatus is then quickly cooled and the pyknometer dried and weighed. To make allowance for the changes with temperatures in the volume of the apparatus, it is necessary to know the weight of the pyknometer empty and also its weight full of water at two temperatures.

If the pyknometer contains v ccs. water (*wt*, w grms.) at temp. t, and v^1 ccs. water (*wt*, w^1 grms.) at temp. t^1, and v_θ ccs. of liquid (*wt*, w_θ) whose density is required at temp. θ (boiling point of this liquid), (*i.e.* Δ_θ), then:

$$v^1 - v = 3av(t^1 - t)$$
$$v_\theta - v = 3av(\theta - t)$$
$$v_\theta - v = \frac{(v_1 - v)(\theta - t)}{(t^1 - t)} \qquad \text{where } k = \frac{(v_1 - v)}{(t^1 - t)}$$
$$= k(\theta - t)$$
$$\Delta_\theta = \frac{w_\theta}{v_\theta} = \frac{w_\theta}{v + k(\theta - t)}$$

(ii) Wade and Merriman's modification of Gay-Lussac's pyknometer (2) consists of a small, flat-bottomed flask with a capillary neck expanding at the top to a small cup which is closed by a rubber stopper; there is also a special filling apparatus. With a 25 cc. pyknometer having a neck of 0·7 mm. internal diameter, an error in setting of 0·1 mm. corresponds to an error of about one in the sixth place. The india-rubber stopper is used with the idea of preventing the small vapour leak which occurs with ground-glass joints; but rubber absorbs some vapours, *e.g.* chloroform, so readily, and is altogether so difficult a substance to weigh exactly that the authors prefer to use a well ground-in glass stopper instead. The kind of losses to be expected by this leak are shown by the following figures below:

	Bousfield pyknometer 70 c.c.	S.G. Bottle with cap 50 c.c.
Weight after 45 min. in balance-case .	114·3722 grams.	66·6592 grams.
Weight after 90 min. in balance-case .	114·3643 "	66·6588 "
Loss in 45 min.	0·0079 "	·0004 "
Temp. of balance-case = 25° C. Ether was the liquid used.		

In this method the stopper can always be inserted to exactly the same extent in the bottle. It can also be filled easily without letting the liquid come in contact with moist

air. The filling apparatus consists of a glass bulb to which are connected three tubes: one of these passes from the bottom down through a rubber stopper to the pyknometer; the other two project from the top of the bulb, one vertically, to which is attached a two-way tap so that connection can be made to the pump or to a tube for admitting dry air, the other projecting horizontally and then bending downwards to the round-bottomed flask containing the liquid. Filling is carried out by first exhausting and then tilting the apparatus so that the liquid runs towards the pyknometer; on admitting dry air some liquid is forced into the pyknometer and by repeating the process it can be completely filled. A small air-bubble is introduced with the aid of a drawn-out tube, the pyknometer is emptied, and is then attached in an inverted position, to the filling apparatus. By repeating the exhaustion and filling with dry air, the liquid can be returned to the flask without coming into contact with moist air. The repeated admission of dry air and its subsequent exhaustion is a rapid and efficient method of drying the apparatus. The adjustment in the thermostat is made by removing with filter paper the excess of liquid in the cup, and then the liquid in the capillary down to the mark by means of a rolled-up piece of filter paper, or a finely drawn-out tube in which the liquid rises by capillarity. A lens can be used to view the position of the meniscus. Air is blown into the cup to remove vapour and the stopper is put in place.

A combined specific gravity bottle and dilatometer has been described by Browne (6) for determining the small changes of volume which occur when sugar solutions undergo inversion or when sugar solutions are mixed. The apparatus consists of a narrow tubular body holding 30 ccs. connected at the bottom with a capped graduated capillary tube and contracting at the top to a small opening. The latter is slightly funnel-shaped and is ground on its inner surface so as to receive a thermometer which is also ground so as to fit tightly on insertion. The displacement of the thermometer is about 7 ccs., and this leaves a capacity of 23 ccs. for the instrument when stoppered.

The scale upon which the changes of volume are measured is graduated so that one division equals 0·001 cc. and by means of a magnifying-glass reading can be made to 0·0001 cc. The

apparatus can be used in the same manner as the ordinary pyknometer.

(iii) Bousfield pyknometer (3) consists of a U-tube holding from 70 to 250 ccs. Each arm of the U terminates in a capillary neck expanding to a cup at the top. The ends of the U are connected by a piece of solid glass which serves to attach a platinum wire to hang up the apparatus in the balance-case or the thermostat. A piece of lead hung on the lower part of the U keeps it vertical, and so assists in the adjustment. Sometimes the solid piece of glass is replaced by a narrow tube with the idea of facilitating the filling of the apparatus; there is, however, little difficulty in filling by suitably tilting the apparatus, while the tube acts as a trap for bubbles. Glass stoppers are provided and ground-in glass tubes are sometimes used to assist in filling by suction. This apparatus has the advantage of having a large volume and a relatively large surface so that thermal equilibrium is rapidly attained; it is also easy to clean. It has the disadvantage of having two ground-glass stoppers with the increased chance of leakage, and of the larger air-space in the cups which may lead to some uncertainty in the vacuum correction. Perhaps it would be worth while to use two sets of stoppers, shallow ones leaving a large air-space in the cups for use in the thermostat, and deeper ones almost filling the cups for use during the weighing.

For accurate work it is advisable to correct to vacuum each of the weighings separately, using the values of the temperature and pressure recorded at the time of weighing, rather than to use one of the formulæ for reducing the final specific gravity to vacuum; this entails more arithmetical labour, but gives more reliable results. The method of calculation is shown in the following example:

Weight of bottle	= 31·7426 grams at 16°C. and 747 mm. pressure.
Weight of bottle full of water	= 101·6849 grams at 18·5° C. and 742 mm. pressure.
Weight of bottle full of liquid under examination	= 157·4127 grams at 19·9° C. and 739 mm. pressure.

These three weights (in grams) corrected for errors in the weights used, become:

31·7492 101·6971 157·4290.

The densities of the air at the times of the three weighings are then found by interpellation in Wade and Merriman's table (2).

Temp.	Mass of 1 cc. of air at the following pressures in 1				
	740	750	760	770	780
25° C.	0·001146	0·001162	0·001177	0·001193	0·001208
20° C.	0·001168	0·001184	0·001199	0·001215	0·001230
15° C.	0·001190	0·001206	0·001222	0·001238	0·001254
10 °C.	0·001211	0·001228	0·001244	0·001261	0·001278

The values found are: 0·001197, 0·001178, 0·001166 grams. the weight of the bottle in vacuo = W_2.

$$W_v = W_{a1}(1 + S/D - S/D').$$
$$= 31·7492\,(1 + 0·001197/2·50 - 0·001197/8·4).$$
$$= 31·7599$$

the apparent weight of the bottle under the conditions of the other two weighings can now be calculated.

$$31·7599 = W_{a2}\,(1 + 0·001178/2·50 - 0·001178/8·4).$$
$$W_{a2} = 31·7494.$$
and $31·7599 = W_{a3}(1 + 0·001166/2·50 - 0·001166/8·4).$
$$W_{a3} = 31·7495.$$

The apparent weight of the water can now be obtained: $101·6971 - 31·7494 = 69·9477$; and that of the liquid $157·4290 - 31·7495 = 125·6795$; while the approximate value of the density is $125·68/69·95 = 1·797$;

These weights are now corrected to vacuum.

$$W_1 = 69·9477\ \ (1 + 0·001178/0·998 - 0·001178/8·4) = 70·0206.$$
$$W_2 = 125\,6795\,(1 + 0·001166/1·797 - 0·001166/8·4) = 125·7436.$$

and so $\text{S.G.}\,\frac{20^\circ\text{C.}}{20^\circ\text{C.}} = 1·795809.$

and $\text{D} = \text{S.G.}\,\frac{20^\circ\text{C.}}{4^\circ\text{C.}} = 1·795809 \times D_{water} = 1·795809 \times 0·998230 =$

1·792630

It should be noted that since there are seven significant figures eight figure logs are necessary.

The three main objections to the pyknometer method, when extreme accuracy is required, are (1); (*a*) The difficulty of keeping a large volume of unstirred water at a constant and fixed temperature; (*b*) The difficulty in removing or correctly allowing for the effects of humidity in weighing

the apparatus. (*c*) The reduction of sensibility of the balance when weighing a relatively heavy load.

The method of hydrostatic weighing is sometimes used in density determinations. The Mohr-Westphal balance is an example of this method. With this instrument specific gravities can be directly read to the fourth decimal place. The hydrostatic method was employed in another manner by Osborne, McKelvey, and Bearce (5) with results in very good agreement with pyknometer determinations. The sinker used was of Jena glass and was 33 cms. long and 1·3 cm. external diameter; it was ballasted with mercury, and before sealing was well annealed at 450° C. The sinker was suspended, by means of a platinum wire, 0·3 mm. in diameter, in the densimeter tube which was only slightly larger, and was attached to the bottom of one pan of a good balance standing on a shelf above the thermostat. The wire was covered, by electro-deposition, with a layer of dull gold at the point where it entered the liquid. This device was found to ensure the wetting of the suspension and the prevention of slight sticking which otherwise would occur. Platinum black on the fine platinum suspension wire has also been used. These devices reduce surface tension effects, but do not completely eliminate them. To the wire was attached a plug which fitted into a hole in the top of the densimeter cover, and the proportions were so arranged that when the balance was arrested the plug filled the hole, while the sinker just rested on the bottom of the densimeter. When the beam was released the sinker was raised off the bottom, and, the plug being removed from the hole, it left the sinker freely swinging. By this means the liquid was only exposed to the air during the actual weighing. The chief difficulties were due to alteration of the concentration of the solution by evaporation and by absorption of water from the air. Also, unless the liquid was first carefully freed from dissolved air, small bubbles formed on the sinker which could not easily be removed. These writers state that the density of alcohol is altered by eight in the fifth place by saturation with air at 25° C. The volume of the sinker was first determined by weighing it in air and then in air-free distilled water; then by making use of the known densities of the air and of the water, its volume was obtained. Next, the sinker was weighed in the liquid, and, having been removed,

the suspension was weighed and the sinker was replaced and again weighed: the mean value of the difference gave the apparent weight of the sinker in the liquid. The density was calculated from the expression

$$D_4^t = \frac{\left(S - \frac{W_1 - w + W_2 - W}{2}\right)\left(1 - \frac{d}{8 \cdot 4}\right).}{V_t}$$

where S = the mass of the sinker.

V_t = the volume of the sinker at t° C. (the temperature of the thermostat).

W_1 and W_2 = balance readings with the sinker on.

w = balance reading with the sinker off.

d = the density of the air.

The average variation from the mean was 2 or 3 in the sixth place.

As shown by Lamb and Lee (1) the displacement or hydrostatic method has not the three drawbacks which the pyknometer method already mentioned possesses. The liquid can be kept at a constant temperature, as it is possible for it to be stirred. The humidity effects do not interfere, as the float is surrounded by the liquid at constant temperature and so a definite surface condition will result. Thirdly, the load on the balance is small, since the weight of the float is neutralised by the buoyant effect of the liquid. To overcome the surface tension effects caused by the wire in the suspension method, a submerged sinker method has been devised by Pisati and Reggiane, who use a sinker of known specific gravity and add distilled water until the sinker hovers in the liquid or add small platinum weights to the sinker until the same state is obtained. Richards and Shipley (1) make temperature the variable factor and obtain results correct within 1 in 5,000,000. It is difficult to vary the buoyancy of the sinker by very small amounts (variations as small as ·01 milligram being necessary) while slight temperature alterations are also difficult to bring about. Lamb and Lee have devised an extremely accurate apparatus in which they have avoided these difficulties. They placed a piece of soft iron in the bulb of the sinker and by means of an electric current sent through a properly placed external circuit exert an electro-magnetic

attraction in a vertical direction upon the bulb, and thus produce displacement of the sinker in a vertical direction. This attraction produces the effect of added weights. The error of the method, it is claimed, is less than one unit in the seventh decimal place.

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THE AGE AND AREA LAW

A FUNDAMENTAL LAW OF GEOGRAPHICAL DISTRIBUTION

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A THEORY of geographical distribution has been brought forward in a series of recent papers (10–19) by Dr. J. C. Willis, which should radically change the outlook on many problems of evolution. It has been expressed variously by the author at different stages in the controversy which has arisen as a result of his work.

The law was first stated (15, p. 340) in the following form : "The commonness of a species depends upon its age from the time of its arrival in, or evolution in, the country.[1] The commonness of any individual species will, of course, depend upon its degree of adaptation to local conditions and upon many things, such as the sudden appearance of new factors like diseases, which can only be regarded as chance." As the result of further study and some controversy, it was restated later (17, p. 456) thus : "The geographical distribution of a species (*i.e.* the area which it includes within its outer localities) within a fairly uniform country not broken by serious barriers depends upon the age of that species within that country." As the result of further controversy, the law is now stated (18, p. 206) to be subject to the qualifying phrase " so long as conditions remain constant," and the following causes are enumerated (*loc. cit.*) which may modify the operation of the fundamental principle of age and area :

Chance (the operation of causes as yet not understood) ;
Action of man in opening up a country, cutting of forest, exploring, making fires, etc., etc. ;

[1] As this applies to the average species, and not necessarily to any particular one, the species must be taken in groups of about 20 (cp. 15, p. 325).

Interposition of barriers, such as mountains, broad rivers, deserts, arms of the sea, sudden changes of climate from one district to the next, and the like ;
Geological changes, especially if involving change of climate ;
Serious changes of climate ;
Natural selection [1] ;
Local adaptation (a species may have a peculiarity which is useful in one country and valueless in another) ;
Dying out of occasional old species ;
Arrival of a species at its climatic limit ;
Density of vegetation upon the ground at the time of arrival of a species ;
Presence or absence of mountain-chains in the land over which the species has to travel in arriving ;
Relative width of union between the country of departure and that of arrival (the wider it is the more rapid may be the spread of the species in the new country) ; and so on.

It is recognised that this list is not complete, but it is claimed that, although some of these causes probably come into action in almost every case of any one species, the total result is not a differentiating action, and the different effects of these various modifying causes acting in so many different directions cancel out when large numbers of species are dealt with. The remaining effect after the cancellation is shown to be due to some mechanical cause, such as age, which acts on all species, genera and families alike, and which is, " at any rate, independent of morphological and biológical qualities."

Willis founded his hypothesis on a large mass of statistics of the estimated distribution of species in Ceylon (15), but has confirmed it as a law by dealing with the measured distribution of species in New Zealand (17). At present, although recent researches (19) extend the operation of the law to the lower plants and (unpublished work) to some groups of animals, most of the facts are drawn from the known distribution of the flowering plants, chiefly in Ceylon and New Zealand. Dr. Willis's chief point lies in the emphasis he lays upon the obvious commonplace that, as a rule, if a species is not dying out, the longer it exists in any given country the wider will

[1] Natural selection enters to a large extent into the determination of the commonness of a species within its area of occupation (see 17, p. 448).

be its distribution in that country. It will be conceded by most botanists that, as a rule, the species of Angiosperms are not dying out, and, as the law of age and area applies only as a fundamental, liable to be altered by a variety of causes in particular instances, it follows that it applies, *as a rule*, to Angiospermous species. It must be remembered that there are numerous exceptions to the law of gravity and to most other fundamental laws, and these are explained by modifying causes. The law of gravity is not considered disproved because a balloon or an aeroplane rises.

By including a whole-hearted adherence to the theory of the origin of species by mutation, an undue emphasis in his first papers on the invalidity of natural selection as a cause of the origin of species, and on the conclusion that endemic species are, as a rule, younger than non-endemic species, Willis has somewhat obscured the issue and brought upon his theory the criticisms of natural selectionists and those who consider endemic species to be as a rule relics of ancient floras. He has also to face those ecologists who, by intensive study of micro-species and their environment, seek to establish causal relations between the concomitant circumstances of certain modifications of environment and the distribution of these micro-species.

By a mass of statistics which are distasteful to the biological mind, and which leave the biologist still sceptical and dis-

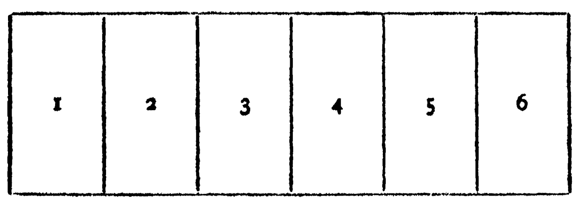

FIG. 1.

trustful, Willis proves that practically all genera and all families have the same type of distribution. It is, however, unnecessary, except as the last stage of the proof, to put the facts in a statistical form. If the area *ABCD*, fig. 1, be any given region into which species arrive from outside regions at *AD*, then the first group of species to arrive would have

spread and occupied the area *ABCD* if a sufficiently long interval had elapsed between the time of their arrival and the time of the analysis of the flora, and if no great barrier such as a mountain-range or great river intersected the area. Similarly, the second group of arrivals would extend over the area *AklD* and so on. The most recent arrivals would naturally occupy the area *AabD*. Now, since the several areas all overlap in the area *AabD, that region should have the greatest number of non-endemic species, i.e.* species which also occur outside *ABCD, and the numbers should decrease regularly to a minimum in kBCl.*

If, instead of arriving from outside regions, the species were evolved within the region, not by a differentiating cause such as natural selection but by chance mutations, then, since the greatest number of species occurs *in AabD*, there will be the greatest chance of species arising in this region. If the laws of chance govern the origin of species *the number of species which are endemic, i.e.* confined to *ABCD, will be greatest in the area AabD, and the numbers should decrease regularly to a minimum in kBCl.* New Zealand is an area somewhat similar to *ABCD*, and Willis has proved that the two predictions in italics hold good for the distribution of species in these islands. For the sake of simplicity, the point of arrival has been placed at the end, instead of somewhere in the region *df*, as it is in New Zealand. It appears probable, therefore, that the laws of chance do actually govern the origin of species, and that the species spread more or less mechanically as a rule.

Considering further the number of non-endemic species occupying the six areas, 1, 1–2, 1–3, 1–4, 1–5, 1–6, on the age and area hypothesis a species which occupies only area 1 or *AabD* is a more recent arrival than one which occupies the whole area *ABCD*, and the other species occupying the areas intermediate in size are intermediate in age (in the country). If the region *ABCD* was connected with the source of its non-endemic flora a long time ago (geologically), many of the species will have had time to occupy the whole area, and the number of non-endemic species *limited to the area AabD*[1] will be smaller than that of the species occupying the whole area

[1] This is to be distinguished carefully from the total number of species in the area *AabD*.

ABCD (supposing the rate of immigration to have been more or less constant) and the number of species occupying intermediate areas will be intermediate also, forming a decreasing series from *ABCD* or 6 unit areas to *AabD* or 1 unit area. This has been found to be the case for both Ceylon and New Zealand.

Considering now the case of the endemic species, there is no reason to suppose that new species have ceased to be produced so that we can postulate a relatively constant supply. If the laws of chance govern the origin of species we can picture the production of new species from an immigrant at a constant rate in the following way: while occupying area, it gives a species, α, while occupying areas 1 and 2 it gives a species, β, in area 1 and another, γ, in area 2, and so on, until, while occupying the areas 1–6, it gives 6 species. Species α will meantime have spread (possibly over areas 1–5) and so will β and γ and other intermediate species to a smaller degree. There will, therefore, be one endemic species occupying 5 unit areas, two occupying 4 unit areas, three occupying 3 unit areas, four occupying 2 unit areas, and five occupying 1 unit area, while six will be just beginning to spread. From this it will be seen that the number of endemic species occupying a small area is naturally larger than the number occupying a large area, while the intermediate areas possess intermediate numbers of endemic species forming an increasing series from 5 unit areas to 1 unit area.[1] This is exactly the opposite of what occurs with the non-endemic species (see above) and the gradation in opposite directions of the numbers of species occupying the various sizes of areas is exactly what Willis finds by comparing the endemic and non-endemic floras of Ceylon and of New Zealand.

Among the many predictions made and confirmed by Willis about the flora of New Zealand were three which may be given thus: if the area *ABCD* was separated from the source of its non-endemic flora at a much earlier period than a similar area *A'B'C'D'*, then the average area occupied by the species in *ABCD* would be greater than the average area occupied by those in *A'B'C'D'*. This would be the result of more non-endemic species having had time to spread and occupy most of the areas 1 to 6 in *ABCD*, while new species would

[1] The actual area might be *AabD*, *abdc*, *cdfe*, *efhg*, or *ghlk*.

not arrive sufficiently rapidly to give proportionate increase in the number of species occupying only area 1 or areas 1 and 2 Given that the rate of spreading of a species is rapid compared with the rate of origin of new species, the endemics as well as the non-endemics will show a larger proportion of widely distributed species in *ABCD* than in *A'B'C'D'*. By comparing the average area occupied by the species in Ceylon and in New Zealand and the numbers of widely distributed non-endemic and endemic species in these two islands, Willis has confirmed these predictions.

Another prediction made with the aid of the age and area law and verified in the New Zealand flora is that if the species enter *ABCD* at *f* instead of at *AD*, then if the area of distribution of any one species extends to the line *cd* it will probably extend to *gh* also, if to *ab* then to *kl* also, etc. Similarly with endemic species, those produced early in the history of the flora would extend over the greatest area; being produced early they would have arisen near *f*, so that if the area occupied by an endemic species includes area 1 or area 6, the whole area of the species is likely to be large. This also has been verified.

Therefore, the age and area theory, having been subjected to the test of prediction and verification, may be taken as proved, and its universal application subject to modifying causes being proved by published and unpublished work it remains no longer a theory, and must be regarded as a law.

It is necessary to point out that even with the age and area law there may be endemic species which are older (in the country) than some recent arrivals. For instance, an endemic may be old enough to have spread all over *ABCD*, while an immigrant may be so recently arrived that it has spread only over *AabD*. This, however, does not interfere with the general statement that the number of endemic species increases with their rarity, while the number of non-endemic species decreases with their rarity, nor with the conclusion that, therefore, the endemic element in a flora is, on the whole, a younger element than the immigrant element. Students of endemism are apt to concentrate their attention on the peculiar relic endemics (which undoubtedly exist) thus producing the impression that the whole endemic flora is composed of such species.

This part of Willis's work has recently been criticised very

shrewdly by Sinnott (8), who gives statistics of the growth form of the species (trees, shrubs, and herbs) of Ceylon and Peninsular India, which prove that the woody forms, especially trees, are much more abundant in the endemic than in the non-endemic flora. He also cites similar circumstances in a number of small islands and Australia. The predominance of woody forms in insular endemic floras has been remarked upon by all those who have dealt with the subject, but whether this is due to these floras being ancient, as Sinnott and others suggest, or whether it is due, in part at least, to some specific effect of insularity, remains to be decided. It is more than probable also that herbaceous species more frequently possess greater powers of dispersal and would reach isolated areas more readily than the arborescent species, thus raising the percentage of herbaceous forms in the non-endemic flora, but in no way invalidating the age and area law, which is applied by Willis only to age within the given region. Another objection raised by Sinnott to the age and area law is that "it necessarily implies a greater antiquity for the herbaceous than for the woody vegetation of the earth." The age and area law deals, however, only with age within the country, not with absolute age except in the case of endemics.

Sinnott, however, admits more than Willis maintains when he says (8, pp. 214–15) that "there is doubtless much truth in Willis's main contention that, other things being equal, the longer a species lives, the wider the range it will cover. The chief argument on which the hypothesis is based is the fact, which in the face of the data presented cannot well be doubted, that endemic types have comparatively narrow ranges and non-endemic types comparatively wide ones." In fact, his point of view is summed up in his concluding sentence (8, p. 215), where, after mentioning one or two additions to the list of "modifying causes" of the age and area law in relation to endemism, he says, "The purpose of the present paper is to point out certain of these complexities and to show that no single hypothesis like that of 'age and area,' however valuable it may be in explaining certain facts, can be used as a key to the whole problem."

The hypothesis that endemics are as a rule recent receives confirmation in a recent contribution by Taylor (19), who, after an examination of the endemic flora of the vicinity of New

York, concludes that while fourteen of these species are the result of specific or generic instability (*i.e.* recent mutations), two have arisen by adaptation of wider spread species to the local environment, and only five by the dying out of previously more widely distributed species.

The mutation controversy is involved, but at present only one contributor, Ridley (7), has attempted to uphold the theory of natural selection to the exclusion of mutations. Ridley criticises the age and area hypothesis on various grounds. He takes the very common species of Willis and Trimen to mean species which are abundantly represented by individuals instead of species which are widespread, and gives various cases of such locally abundant species which have died out in particular localities, especially as the result of the advent of man. He claims also to prove that these species may disappear without a geological catastrophe. As he quotes volcanic action and the glacial period as non-catastrophic occurrences, it is difficult to follow his argument. It is also difficult to understand what Mr. Ridley thinks of the case for mutations as he states in his summary that "the mutation theory . . . is not in accordance with the facts," while in the text of the paper he gives a very good account of the action of natural selection in killing out injurious "mutations" and of the origin of species by mutation. "This theory," he says, "can be tested and proved by the study of mutations." He proceeds, moreover, to quote well-developed spines, a change in stature from about 6 in. to about 10 ft., etc., as "one or two examples of infinitesimal variations."

Willis has replied in a recent paper (18) by acknowledging that the action of man and other subsidiary causes may modify the action of the law of age and area. "Mr. Ridley quotes about seventy cases in various connections. Many of these, *e.g.* those on p. 555, are excellent illustrations of what I have said (16, p. 22) that a very small accident may kill out a species in the class VR." "VR" means very rare, and among the cases mentioned is that of ***Didymocarpus Perdita***, Ridley. The note on this species by Ridley is, "I found two plants of this on a bank in the centre of Singapore surrounded by extensive cultivation. It has never been seen again." It seems not impossible that the description of this species by its author involved its destruction, an excellent example for the

arguments of both Ridley and Willis that incipient species are easily killed out. Willis, however, " for the sake of argument," gives all Ridley's objections concerning the distribution of these seventy species their *maximum* effect, and shows that the resulting corrections of the figures for the distribution as a whole leaves the case for age and area as strong as before.

In a still more recent contribution (19) Willis gives five further examples of the action of age on area. Taking the orchids of Jamaica and dividing the islands of Jamaica and Cuba into a number of areas approximately 6¾ miles square, he shows that the endemic species as a rule occupy about 3 squares, *i.e.* the average for all the endemics is 3 squares, while the average for the species extending only to Cuba is 4·5 squares, and for the wider-spread species 5·7 squares. It is clear, therefore, that the distribution of the orchids in Jamaica follows the age and area law. As an example of an island flora rich in endemics he takes the flora of the Hawaiian Islands and points out that 74 out of the 149 non-endemic species occur on all the chief islands, while only 41 out of the 581 endemic species cover that area. The average area occupied by the non-endemics is about twice that of the endemic species, so that here again the age and area law is at work.

Callitris is taken as an example of a Conifer. The genus is endemic in Australia and Tasmania. One species occupies the area covered by the genus, a second group of two species is less widespread, a third group of eight species is still more local, while there are seven species which occupy very small areas. This is as near to the age and area phenomenon as is to be expected in a single genus of less than twenty species.

Two examples are given of the distribution of ferns. The endemic ferns of New Zealand cover on an average a smaller area than the non-endemic ferns, but the difference is not so marked as in the Angiosperms. A very interesting point in view of the recognised relative antiquity of the ferns is that the endemic ferns occupy on the average about twice the area occupied by the endemic Angiosperms. The endemic ferns also take no notice of Cook's Strait, while the endemic Angiosperms are markedly divided into two groups by that Strait. The deduction is made that Cook's Strait originated between the dispersal of the endemic ferns and the dispersal of the

endemic Angiosperms, in much the same way as it is shown to have done between the dispersal of the non-endemic and that of the endemic Angiosperms.

The same points are brought out in the fifth essay, a study of the distribution of the ferns of the Hawaiian Islands. The endemic ferns again occupy a greater area than the endemic Angiosperms and a lesser area than the non-endemic ferns. The non-endemic ferns, as in New Zealand, occupy on the average about the same area as the non-endemic Angiosperms, a fact which Willis explains by the constant arrival of non-endemic species of ferns on account of the easy dispersal of the latter by means of their spores.

Another supporter of natural selection, Copeland (1), maintains that age has been previously recognised as a factor in distribution, and that " mutation " is merely another word for " variation." Like Ridley, he fails entirely to discriminate between continuous and discontinuous variation. Indeed, mutation or discontinuous variation has taken so great a place in evolutionary facts that adherents of natural selection seem to accept them, unconsciously admitting the facts upon which de Vries founds his theory while nominally in opposition to the theory itself. This is obvious in the following quotation from Copeland, " Regarding myself as a confirmed adherent of the doctrine of natural selection, I do not hold it in the slightest measure *directly* responsible for the origin of any species. Species originate by variation. There is not the slightest doubt that in nearly all cases . . . variation is indiscriminate in direction."

In a series of appreciative reviews de Vries (3–5) emphasises the point that " a general cause must govern this phenomenon, a cause which is, at any rate, independent of morphological and biological qualities," and concludes that the " age and area " theory is the only one sufficiently broad to explain all the facts.

Other reviews in sympathy with the theory have appeared, one by Lotsy (6) and another by Coulter (2) which, if somewhat non-committal, is not in strenuous opposition like those of Ridley and Copeland. Indeed, Coulter and various other eminent American botanists are quite in sympathy with the new doctrine which will, no doubt, take its proper place in the study of geographical distribution.

The present writer in work about to be published on the evolution and geographical distribution of the Compositæ, has found the age and area law very valuable indeed, confirming in the case of every tribe the phylogenetic conclusions reached in the study of the morphology and physiology of the subdivisions of that large and undoubtedly recent family.[1]

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[1] Since the above was written Willis has published a further paper (*Ann. Bot.* vol. xxxi. 1917) in which he shows that the area occupied by a species *in New Zealand* increases with the number of outlying islands included in the total area of the species. A most remarkable confirmation of a prediction to that effect. A further example of the action of the age and area law has been found in the Australian grasses by Breakwell.

THE HYPOPHYSIS CEREBRI: ITS STRUCTURE AND DEVELOPMENT

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THE problems of the origin and function of the pituitary body have for many years proved a stimulating subject for research, with the result that a considerable mass of literature has accumulated, much of which is of interest only to the specialist. Stendell (19) has dealt with the comparative anatomy of the hypophysis in considerable detail, but this work also is of more value to the specialist than to a student of general comparative anatomy. The object of the present paper is to summarise our knowledge of the hypophysis cerebri and thus render it more accessible to the student of biology in all its branches.

It is impossible to give here any adequate account of the work done on the hypophysis; admirable historical notes are given in papers by Herring (7), Tilney (21), and others. The literature list appended to this paper merely gives references to a few out of many important publications on the pituitary body.

In the majority of Vertebrates, the primordium of the hypophysis is a hollow invagination of the ectoderm, but in Teleosts and Amphibia it arises as a solid mass of ectoderm. It is apparent that the hollow invagination is the more primitive type. Its position is indicated at an early stage of development (either before or shortly after closure of the brain) by a thickening of the epithelium of the buccal cavity immediately anterior to the oral plate. This thickened area becomes invaginated and retains from the first close contact with the depressed region of the diencephalic floor which forms the primitive infundibular recess. The lips of the invagination gradually close in so that a hollow pouch-like structure, known after its discoverer (17) as "Rathke's pouch," is established. The opening of this pouch becomes drawn out into a narrow

duct, the hypophysical duct which persists until a relatively late stage, when it becomes obliterated by the development of the cartilaginous basis cranii. Meantime, there arises from the primitive infundibular recess a short diverticulum, generally hollow and thin-walled, constituting the infundibular process. A portion of the wall of Rathke's pouch either maintains from the first or else secondarily acquires intimate relations with the infundibular process. This portion may simply surround the infundibular process closely, or a still more intimate relation may be brought about either by the formation of outgrowths of the infundibular process which grow into the surrounding epithelial tissue, or by the penetration of that epithelial tissue into the substance of the infundibular process. In this way the nervous portion of the pituitary body and the epithelial layer directly surrounding it may become so closely related as to form a single structure, commonly known as the posterior lobe. From the region where Rathke's pouch narrows to join the duct there arise outgrowths which may take the form of a median sprout, or lateral outgrowths, or a distinct proximal lobe. These outgrowths may disappear (Baumgartner (2) lizards and snakes), or may grow up around the rest of the pouch until they come into contact with the brain and form a distinct layer of tissue lying against the tuber cinereum. In some cases these outgrowths also give rise to a layer of cells surrounding the main epithelial portion of the hypophysis. The whole of the remainder of the walls of Rathke's pouch (*i.e.* that which is not converted into the epithelial layer covering the infundibular process, nor used up in the median or lateral outgrowths), becomes converted into a glandular structure. The process of conversion varies considerably in different forms, even within the limits of an order. It consists either in the branching of the originally hollow, thin-walled pouch or in the outgrowth of hollow or solid processes from the surface of the pouch, or simply in the thickening of the walls, which gradually become penetrated by connective tissue and transformed into a compact gland, richly supplied by blood-vessels.

The hypophysis of the adult thus consists of two main parts, distinguished according to their origin as the *pars neuralis* (nervous lobe, " Hirnteil," etc., constituting, with the epithelial layer covering it, the posterior lobe), and the *pars*

buccalis (including the anterior or glandular lobe and the pars intermedia). The pars neuralis is poorly developed in many of the lower Vertebrates, but attains considerable size and importance in the higher Vertebrates, where it forms a hollow or solid swollen structure, attached to the diencephalic floor by a narrow neck, the pituitary stalk. The outer surface is closely invested by epithelial cells derived from Rathke's pouch and constituting the pars infundibularis (pars intermedia " Zwischenlappe," etc.). There has been much discussion as to whether or no the pars neuralis actually contains true nervous elements. Herring (7, 8), who has dealt with the pituitary body from the embryological, histological, and physiological points of view, holds that the pars neuralis consists of neuroglial cells and fibres, and, in addition, cell islets derived from the pars infundibularis (pars intermedia).

The portion of the pars buccalis which is derived from sprouts from the neck of Rathke's pouch forms the pars tuberalis (Tilney, 22). In many forms there has been described " an anterior tongue-like process of the pars intermedia," this being a small mass of epithelial tissue produced anterior to the main body of the anterior lobe, stretching towards the optic chiasma and lying in contact with the tuber cinereum. Tilney (22) defines the tuber cinereum as the portion of the diencephalic floor which is limited anteriorly by the optic chiasma, posteriorly by the corpus mammillare, and laterally by the optic tracts. He describes the pars tuberalis in the cat, dog, rabbit, rat, sheep, and fowl as encircling the pituitary stalk, lying in contact with the tuber cinereum, between it and the main part of the pituitary body. According to him, " the pars tuberalis is moulded to the base of the brain in such a manner that the saccular eminence of the tuber cinereum rests in it as one saucer in another " (Tilney, 22, p. 263). This description also applies to those Marsupials,[1] which I have examined. Tilney regards the pars tuberalis as forming a morphologically independent portion of the pituitary body and its ontogeny gives support to this view. He has himself described its development in the cat and chick, in both of

[1] The marsupial material to which reference is here made, as well as some sections of Cyclostomata, Elasmobranchii, Amphibia, and Reptiles are all in the collection of Prof. J. P. Hill, F.R.S., and I wish to express my gratitude to him for his kindness in allowing me the use of his material.

which it arises as a pair of epithelial processes from the region of junction between the main body and the stalk of the pars buccalis. These grow up dorsally, and, becoming applied to the surface of the tuber cinereum, increase so as to encircle the pituitary stalk completely. Baumgartner (2) states that in Reptiles the pars tuberalis of Tilney and also, in some cases, a thin cortical zone surrounding the "anterior lobe," are derived from lateral buds, similar to those of the cat and chick. In Marsupials (Parker, 16) Rathke's pouch is early subdivided by a constriction into proximal and distal lobes (relative to the duct), and the pars tuberalis is derived from the proximal lobe which grows dorsalwards around the rest of the pars buccalis, branches and gives rise to a collection of tubules encircling the pituitary stalk. There is thus much evidence to show that there exists in many Eutheria, in Metatheria, in some Reptiles and some Birds, a special portion of the pars buccalis which is developed from sprouts arising at the junction of the main body of the pars buccalis with the duct. This portion (the pars tuberalis of Tilney, 22) is probably also represented in some fishes and Amphibia (*vide* infra) but more detailed embryological evidence is required to establish the homology throughout the Vertebrates.

The pars tuberalis and the pars infundibularis together constitute those portions of the pars buccalis which are in actual contact with the nervous tissue and may accordingly be grouped together as the pars juxta-neuralis. In contradistinction to this, the rest of the pars buccalis may be called the pars distalis, and this forms the greater part of the epithelial portion of the pituitary body and is commonly known as the anterior or glandular lobe (Hauptlappen). It consists of glandular tubules or columns of epithelial cells with a very rich vascular supply, and it frequently includes cells of characteristic staining capacity. Further details of its structure may be most conveniently described in dealing with the separate classes of Vertebrates.

Clycostomata.—The hypophysis of Cyclostomata has been described by Haller (6), Stendell (19), Sterzi (20), Gentes (4), and Herring (8), amongst others.

Herring states that the hypophysis of Myxinoids is very similar to that of Petromyzon. Stendell regards that of Myxinoids as partly primitive, partly reduced.

The nervous portion is very slightly developed. There is a wide and shallow depression of the diencephalic floor (fig. 1), the hypophyseal or infundibular recess (H.R.), whose wall is not thickened to any marked extent. Herring (8) states that the wall consists chiefly of ependyma cells placed with their long axis vertical to the internal surface and stains rather more deeply than the rest of the brain wall. This pars neuralis (P.N.) is closely invested by a layer of deeply staining epithelial cells, constituting the pars infundibularis (P.I.), which does not penetrate into the pars neuralis.

The pars distalis (P.D.) lies anterior to the pars infundibu-

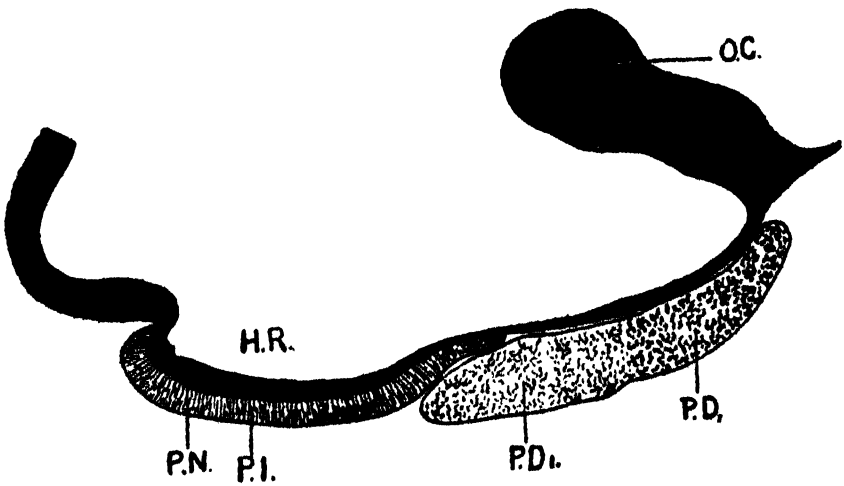

FIG. 1.

laris, from which it is separated by a thin layer of connective tissue. It consists of two portions, the chromophil and chromophobe portions of Sterzi. The anterior of these consists, according to Herring, of solid, irregularly arranged columns of large, granular cells, which take up most stains readily. The posterior portion consists of columns of cells, fairly regularly arranged, and showing little affinity for stains. Both portions are richly vascularised and the line of division between the two is well marked.

Stendell (19) describes the development of the hypophysis in Cyclostomata, quoting von Kupffer (11) and Sterzi (20). The hypophysial and olfactory primordia arise from a common invagination. The caudal portion of this represents the pri-

mordium of both the hypophysis itself and the naso-pharyngeal canal, the dorsal part giving rise to the glandular lobe of the pituitary body.

Pisces.—(1) *Elasmobranchii.*—The pituitary body in Elasmobranchs has been described by Stendell (19), Herring (7 and 8), Baumgartner and others. Herring (8) states that the "elasmobranch pituitary differs from all other pituitaries in not possessing a posterior lobe. The brain wall of the embryo merely evaginates to form a paired saccus vasculosus, but no pars nervosa is formed." On the other hand, Stendell (19)

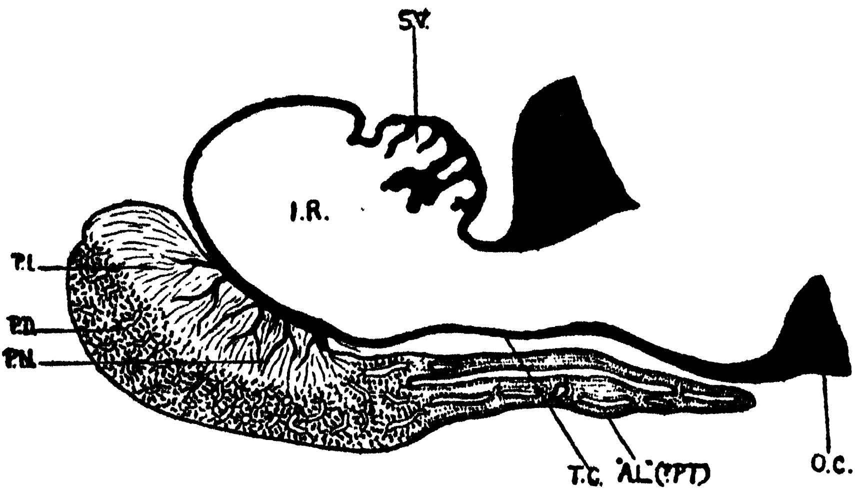

FIG. 2.

describes and figures a well-developed, branched "Hirnteil" in the Notidanidæ.

The third ventricle in Elasmobranchs (fig. 2) is produced backwards and downwards into an infundibular recess (I. R.), the dorsal wall of which is evaginated and richly vascularised and forms the saccus vasculosus. Its ventral wall may remain thin, as in *Scyllium*, or may be produced into branching processes, solid as in *Acanthias*, or hollow as in *Heptanchus*, which constitute the nervous lobe of the pituitary. This is invested by an epithelial pars infundibularis (P.I.).

The pars buccalis as a whole is divisible into an anterior lobe, superior (or dorsal) lobes, and inferior (or ventral) lobes. The anterior lobe in *Scyllium*, *Raja*, and *Acanthias* forms a

narrow, tongue-like process, projecting forwards from the main body of the hypophysis, almost to the optic chiasma. Baumgartner (1) describes the derivation of this lobe from the portion of Rathke's pouch lying anterior to the duct. Tilney (23) states that its development and relations prove it to be a true pars tuberalis. Further observations on the comparative anatomy and embryology of the pars tuberalis are, however, desirable.

The dorsal and ventral lobes appear to correspond respectively to pars infundibularis and pars distalis (P.D.), but there is not a well-marked line of separation between the two portions.

The "anterior lobe" (pars tuberalis ?) appears to consist of columns of epithelial cells and retains a considerable remnant of its original lumen. The ventral lobe (pars distalis) was described by Haller (6) as being a tubular gland, but Herring (8) confirms Gentes' (4) view, viz. that the so-called tubules are blood spaces with the gland cells arranged around them. Herring (8) states that there is no differentiated "pars intermedia" in *Raja batis*. Stendell (19), however, figures a solid mass of lightly staining cells closely investing the nervous portion in *Scyllium canicula* and in *Raja asterias*, and there is no doubt that the portion of the dorsal lobe which lies in contact with the nervous tissue differs histologically from the rest of the gland. Herring (8) states that there is no differentiation of chromophilic and chromophobic cells in the epithelial lobe as a whole, but Baumgartner (1) states that in *Acanthias* the anterior and inferior lobes may be considered the chromophilic ones.

The development of the hypophysis in *Acanthias* has been fully described by Baumgartner (1). A hollow hypophysial invagination is formed, and in a 28 mm. embryo the primordia of all the various lobes are recognisable as distinct outpouchings. From these arise glandular outgrowths which Baumgartner describes as being hollow in the anterior and inferior lobes and solid in the superior lobe.

Teleostomi.—Stendell (19) states that there is no group of Vertebrates in which the hypophysis shows so much variation as in the Teleostomi.

The nervous lobe is well developed and generally consists of branching processes, closely surrounded by epithelial cells;

which constitute the pars infundibularis. In some forms, *e.g.* *Gadus morrhua* (Herring (7 and 8), *Anguilla* (Stendell, 19), Tilney (21)), the processes of the pars neuralis are situated both anterior and posterior to the pars distalis, which forms a solid mass of cells which, according to Herring (8), are chromophilic and form a wedge-shaped lobe between two chromophobic portions. These lightly staining portions are penetrated by the tissue of the pars neuralis and represent the pars infundibularis.

The primordium of the pars buccalis in Teleostomi is solid, and in some cases at least it is bilobed.

Dipnoi.—The descriptions of the hypophysis of Dipnoi are somewhat scanty. Haller (6) criticises Burckhardt's (3) description on the ground that he confuses the saccus vasculosus with the nervous lobe of the pituitary.

Graham Kerr (10) gives a brief description of the development of the hypophysis in Lepidosiren, from which it appears that the hypophysial ingrowth is solid at first and later acquires a lumen. He also states that the hypophysis " becomes here as elsewhere closely united with the infundibulum, its dorsal portion becoming penetrated by outgrowths of the latter."[1]

Amphibia.—The hypophysis of Amphibia has been described by numerous workers, including Herring (8), Haller (6), Tilney (21), and Stendell (19).

In the Urodeles the pars neuralis is only slightly developed. In *Triton* the ventral wall of the infundibular process remains thin, whilst in *Salamandra* it is slightly thickened. In correlation with this, the pars infundibularis is only slightly differentiated from the pars distalis which forms a relatively compact mass of cells.

In *Triton* there are two cell strands which lie against the tuber cinereum anterior to the pars distalis. From their position and from comparison with the origin of similar structures in the Frogs and in Reptiles, it seems probable that these cell strands represent a pars tuberalis.

In the *Anura* (fig. 3) a swollen, solid pars neuralis (P.N.) is present, consisting, according to Herring (8) of neuroglia

[1] Since writing the above, I have had the opportunity of examining an embryo (about 13 mm. long) of Ceratodus in the possession of Mr. D. M. S. Watson. In this form the hypophysis is represented by a flattened pouch, which at this stage at least is hollow.

and ependyma cells. Closely applied to the ventral and postero-ventral faces of this lobe is a layer of lightly staining cells, constituting the pars infundibularis (P.I.). The pars distalis (P.D.) is a solid mass of cells detached from the pars infundibularis from which it is separated by a layer of connective tissue. The cells of the pars distalis are of two types, chromophilic and chromophobic, and numerous blood-vessels are scattered amongst them.

The paired "tongue-like processes" described above in

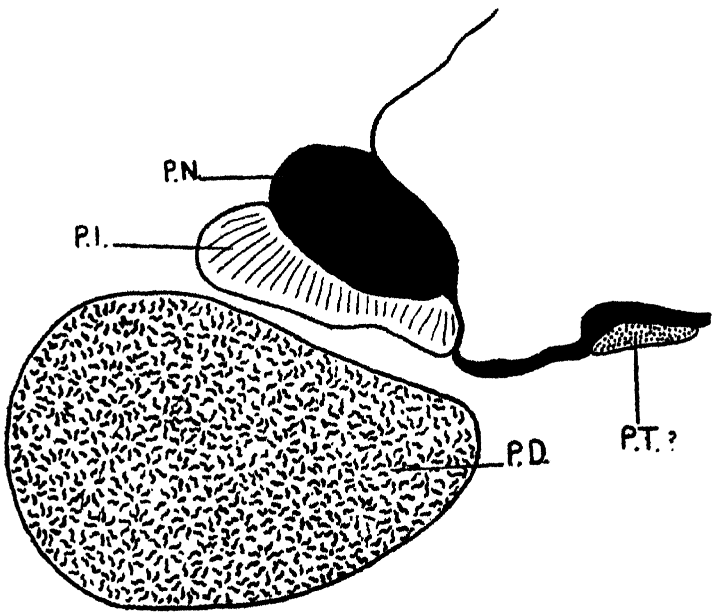

Fig. 3.

Triton are present also in *Rana* as detached masses of cells lying in very close contact with the brain floor, which, however, retain a connection with the main body of the pars buccalis in late tadpoles. These probably represent the pars tuberalis.

The dorsal (saccular) surface of the infundibular process in Amphibia is thin-walled and richly vascularised, but does not form a definite saccus vasculosus.

The primordium of the pars buccalis in Amphibia is solid.

Reptilia.—Baumgartner (2) has given a very full account

of the development of the Reptilian hypophysis, and has also given a description of the adult anatomy as well as a review of the literature dealing with this group.

The pars neuralis is well developed and forms an outgrowth which is hollow in most forms, but is solid in some snakes. This is closely invested on its ventral and postero-ventral faces by the pars infundibularis. There is no cleft separating the pars infundibularis from the pars distalis, but Baumgartner states that in some lizards a remnant of the original lumen of Rathke's pouch persists as a cleft in the substance of the pars intermedia.

The pars distalis has, according to Herring (8), a structure peculiar to Reptiles. It consists of columns of cubical or columnar cells forming distinct acini, the cavities of which are filled with a homogeneous colloid material. Two types of cells are recognisable, the majority being clear and non-granular, others being granular and readily stained.

In early stages of the development of some members of each group of reptiles, the primordia of a pars tuberalis are recognisable as lateral buds arising from Rathke's pouch. According to Baumgartner (2) in turtles these lateral buds persist and form a pars tuberalis lying against the tuber cinereum, dorsal and anterior to the pars distalis, and also a cortical zone around the anterior part of the pars distalis; in alligators they form a tongue-like process (pars tuberalis) and a spiral band around the pars distalis; in *Sphenodon* (Gisi, 5) they form a "pars terminalis" (equivalent to pars tuberalis); in most lizards and snakes they disappear completely. Baumgartner points out that on embryological and histological grounds both the cortical zone in turtles and alligators and the separate cell masses derived from the lateral buds must be included in the pars tuberalis. These latter cell-masses are paired and appear to resemble closely the cell-strands in Amphibia to which reference is made above. The pars tuberalis, according to Baumgartner, consists of irregular columns of cells, separated by a large amount of vascular connective tissue.

The pars buccalis in Reptiles develops from a typical hollow, ectodermal pouch, whose walls become differentiated into pars infundibularis and pars distalis, while, as already stated, lateral buds arise which in some cases persist and form a pars tuberalis.

Aves.—The pituitary of birds in its general features closely resembles that of Reptiles.

The pars neuralis is hollow and is sometimes convoluted (Herring, 8). The pars buccalis is of considerable size, and contains no lumen, but Herring states that the position of the cleft is represented by a small amount of connective tissue lying between the pars distalis and the strands of the pars infundibularis.

The pars infundibularis consists of strands of epithelial cells closely investing the pars neuralis. The pars distalis consists of cell columns showing only slight differentiation in staining capacity.

The pars tuberalis, according to Tilney (22), forms in the common fowl a ring-shaped structure around the base of the pituitary stalk. It arises as in Reptiles from lateral buds which grow up so as to come into contact with the brain and then increase in size to such an extent that the two originally separate lateral structures meet and unite both anterior and posterior to the pituitary stalk and so displace the pars distalis slightly, ventralwards, removing it from contact with the brain floor.

The pars buccalis of birds arises as a thin-walled ectodermal invagination which closes to form a hollow pouch. The development of the pars tuberalis has already been outlined.

Mammalia.—The hypophysis of the Metatheria closely resembles that of the Eutheria, so that it is possible to deal with the group as a whole.

In the general relations of the parts, the hypophysis of Mammals closely resembles that of birds and reptiles. The pars neuralis is large and well-developed, and, in most cases, is solid. In the cat, however, it retains a lumen. Herring (7) states that the pars neuralis of the Mammalian pituitary contains no true nervous elements, but consists entirely of ependymal and neuroglial cells and fibres. The relations of the pars neuralis and pars infundibularis are extremely intimate in Mammals (fig. 4); the cells of the pars infundibularis penetrate into the tissues of the pars neuralis (P.N.) so that the so-called posterior lobe (P.L.) is a complex structure.

The pars buccalis retains the original lumen of Rathke's pouch as a cleft (L), generally cup-shaped, separating the pars infundibularis from the pars distalis. The pars distalis (P.D.)

consists of a solid mass of cell columns amongst which are numerous blood-vessels. In the cat, according to Herring (7), the cells are of three kinds—small, polygonal cells with little protoplasm, containing few granules, larger cells with fine granules in the protoplasm, and cells which stain very deeply (chromophilic). Herring inclines to the view that these various types merely represent different phases of activity of secretory cells.

The pars tuberalis in Mammals closely resembles that of birds, forming, as Tilney (22) expresses it, a saucer-shaped structure, perforated for the passage of the pituitary stalk and lying between the pars distalis and the tuber cinereum

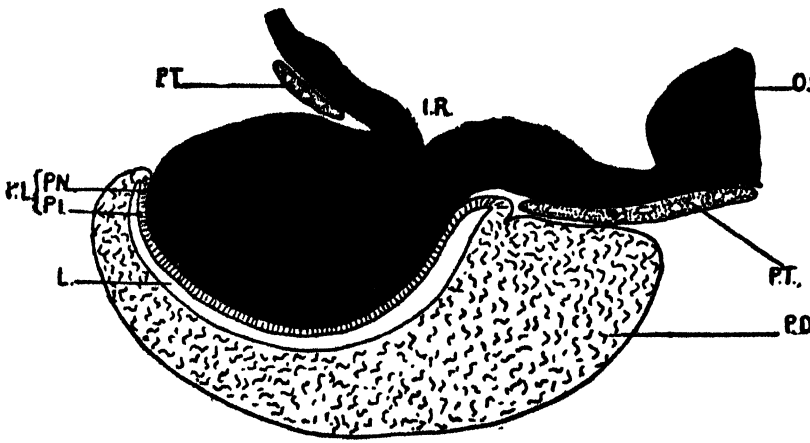

FIG. 4.

(P.T.). It consists of lightly staining cells which frequently form acini.

The development of the hypophysis in Metatheria has been described (Parker, 16), whilst that of Eutheria has been the subject of numerous detailed investigations (Müller (12) Mihalkovics (13), Salzer (18), Herring (7), Tilney (22), and others). Rathke's pouch arises as an ectodermal invagination. A portion of its dorsal wall remains thin and constitutes the pars infundibularis ; the rest increases in thickness and becomes converted into the glandular lobe of the adult, either by outgrowth of solid or hollow processes, or by the ingrowth of connective tissue which breaks up the substance of the walls into lobules. The chromophilic and chromophobic types of cells

become differentiated *in situ*, the chromophilic cells appearing first in the posterior region of the pars distalis (Parker, 16). The origin of the pars tuberalis has already been briefly described above.

It is only possible to refer very briefly to the physiological significance of the pituitary body (see Swale Vincent, 24). Oliver and Schäfer (15) found that saline extracts of the pituitary body, when injected into the blood-stream, produce a rise in blood-pressure. Pituitary extract has also been found to act specifically on the kidneys, producing dilatation of the renal vessels and increase of secretion from the tubules. From the results of Herring (8), Howell (9), and others, it seems probable that this effect is produced by the secretion of the posterior lobe, and that the active principles of that lobe are products of the epithelial cells of the pars infundibularis. Herring (8) finds that the extract of the " anterior lobe " (pars distalis) in some forms stimulates mammary glands to activity, but has no effect on blood-pressure, kidney-volume, or urinary secretion.

There seems to be good evidence for associating a pathological condition of the pituitary with acromegaly, a disease characterised by enlargement of the bones, especially of the hands and feet, so that it seems possible that the pituitary is an internally secreting gland, exercising some control over growth of bone.

Many problems relating to the origin and evolution of the pituitary body have been left untouched in the foregoing, and it is impossible to deal with them here. Embryological evidence shows that the pars buccalis is derived from an ectodermal invagination, and numerous hypotheses have been advanced as to the original significance of this structure. Amongst other theories it has been suggested that the hypophysis represents the mouth of the ancestor of the Vertebrates, the costal gland of an Arthropod ancestor, the remains of the proboscis of a Nemertean ancestor, a functional anterior neuropore, a pair of gill-clefts, and so on. Nusbaum (14) described a connection between the hypophysis and the preoral gut in dog embryos and put forward the theory that the two structures represent remains of a portion of gut extending in front of the present mouth in the ancestor of the Vertebrates. In some Marsupials also the preoral gut is closely associated with the hypophysis, but it is impossible at present to say

whether this is merely due to slight variation from the normal development or indicates a common origin of the preoral gut and hypophysis, as suggested by Nusbaum. It must therefore be admitted that the phylogenetic significance of the hypophysis remains an unsolved problem.

Description of Figures

The accompanying diagrammatic figures are made partly from the figures of Herring, Stendell, and Tilney, and partly from my own observations.

Fig 1. Median longitudinal section through the hypophysis of a Cyclostome.

H.R. Hypophysial recess. O.C. Optic chiasma. P.D. Pars distalis, chromophilic region. P.D.1, Pars distalis, chromophobic region. P.I. Pars infundibularis. P.N. Pars neuralis.

Fig. 2. Median longitudinal section through the hypophysis of an Elasmobranch.

" AL." (? P.T.), Anterior lobe, probably equivalent to pars tuberalis. I.R. Infundibular recess. O.C. Optic chiasma. P.D. Pars distalis. P.I.Pars infundibularis. P.N. Pars neuralis. S.V. Saccus vasculosus. T.C. Tuber cinereum.

Fig. 3. Longitudinal section through the hypophysis of an Amphibian, passing through the lateral pars tuberalis.

P.D. Pars distalis. P.I. Pars infundibularis. P.N. Pars neuralis. P.T. Pars tuberalis.

Fig. 4. Median longitudinal section through the hypophysis of a Mammal.

I.R. Infundibular recess. L. Lumen. O.C. Optic chiasma. P.D. Pars distalis. P.I. Pars infundibularis. P.L. Posterior lobe. P.N. Pars neuralis. P.T. Pars tuberalis.

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PRE-PALÆOLITHIC MAN IN ENGLAND

By J. REID MOIR, F.R.A.I.

It has been my purpose in the three articles already published in Science Progress[1] to draw attention to some of the various forms of flaked flints found in deposits of a greater antiquity than those containing the normal palæolithic implements, and to describe the experiments in flint fracture which were carried out, and which have convinced me that these flaked flints are undoubted works of man. I have endeavoured to establish the fact that in this country we have evidence of a continuous evolution in the making of flint implements, which, commencing with the most primitive "eolithic" edge-trimmed stones, proceeded uninterruptedly and inevitably to the production of the well-known pointed and ovate palæoliths of the river-drift deposits.

I am perfectly well aware that such a view runs counter to the opinions held by the more conservative school of archæologists both at home and abroad, which, while not denying that the earliest palæolithic implements cannot represent the first efforts of man in flint-flaking, has nevertheless refused to believe that the races of pre-palæolithic people are represented, either by their flint implements or actual skeletal remains in this part of the country.[2] It has been the custom with some to look to Asia as the birthplace of mankind, an area, as Keith so truly states in a recent issue of *Man* (vol. xvii. No. 5, May 1917), "of which we know almost nothing, and therefore can believe it capable of anything." I have read with care the arguments put forward by various writers in support of this and similar hypotheses, and I have found them to be unsatisfying and unsatisfactory. I am unacquainted

[1] "Flint Fracture and Flint Implements," Science Progress, No. 41, July 1916, pp. 37–50; "The Oldest Flint Implements," *ibid.* No. 43, January 1917, pp. 431–40; "The Relationship of the most Ancient Flint Implements to the later River-drift Palæoliths," *ibid.* No. 45, July 1917, pp. 83–96.

[2] See for instance:—"Men of the Old Stone Age: Their Environment, Life, and Art," Henry Fairfield Osborn, 1916.—"Man the Primæval Savage," G. Worthington Smith, p. 2.

with a single valid reason for accepting the view that Asia witnessed the earliest stages of man's evolution, and moreover as this vast country is a veritable *terra incognita* to prehistorians, I regard it as useless and unscientific to engage in speculation as to what did or did not happen there in the remote past. But, as I know of no reasons why Asia should be regarded as the probable home of earliest man, neither am I familiar with any cause or causes which would preclude the area which is now England from having had that distinction. Thus from the standpoint of pure theory any one may favour Asia or England as his fancy prompts him.

When, however, we pass from the domain of theory to that of fact the situation assumes a different aspect. Of prehistoric Asia we know next to nothing, and therefore have no facts to rely upon; of prehistoric England on the other hand we know a great deal and have a multitude of facts at our disposal. What are those facts?

First and foremost there are the various pre-palæolithic flint implements which have been found in different parts of the country.

I have seen and examined such implements from the Kent plateau,[1] from ancient deposits near Salisbury, Peppard in Oxfordshire, Aldershot, and Selsey Bill in Sussex.[2] It would thus appear that these early flint implements occur over a very wide area, and further search will no doubt extend it still more. But it is in East Anglia where the greatest facilities exist for recovering evidences of pre-palæolithic man. In this district occur widespread glacial deposits of Boulder Clay and gravel, and beneath these in many places is found the Pliocene Red Crag surmounting a detritus-bed containing the débris of an ancient pre-Crag land surface.

In no other part of England is such a deposit as the Red Crag to be found, and in fact the Pliocene strata are almost solely confined to East Anglia.

It has been my good fortune during the past twelve years to have been able to investigate the Boulder Clay, Middle Glacial Gravel, and the sub-Red Crag detritus-bed in search

[1] "On the Primitive Characters of the Flint Implements of the Chalk Plateau of Kent," Sir J. Prestwich, *Jour. Anthrop. Inst.* vol. xxi. pp. 246-62.

[2] "The Sub-Crag Flints," J. Reid Moir, *Geol. Mag.* V. vol. x. No. 12 December 1913, pp. 553-5.

of flint implements. These deposits are exposed in various pits sunk into the plateau in the neighbourhood of Ipswich, and it is in this plateau that the present rivers have cut their valleys in which they have laid down the beds of gravel, etc., containing the normal palæolithic implements. A glance at fig. 1 will at once show the reader the relation of these plateau beds to the valley deposits and demonstrate that the former are of a much greater antiquity than the latter. In fig. 2 is shown a diagrammatic vertical section of these plateau beds, and we will commence by examining the lowermost and oldest stratum, the sub-Red Crag detritus-bed which rests upon the London Clay. The top of the London Clay was a land surface in pre-Crag times, and this land surface was

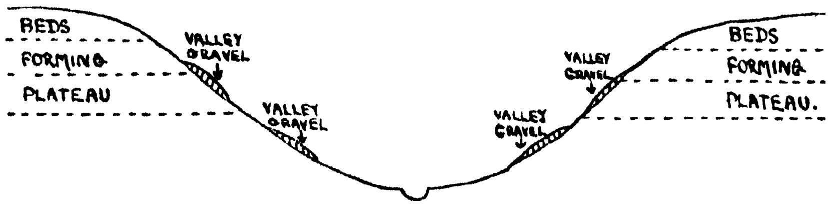

FIG. 1.—Sectional drawing showing the relationship of the plateau beds to the valley gravels laid down by the river when eroding its bed through the plateau.

The beds forming the plateau were at one time continuous across the space now occupied by the river valley.

eventually slowly submerged beneath the waters of the Crag Sea. The objects lying upon this ancient land surface, large and small flints, flint implements, pieces of bone, etc., were no doubt swept during this submergence into hollows or pockets in the London Clay, and finally covered by the sands and shells of the Red Crag Sea. The implements recovered from the detritus-bed are fashioned by bold and skilful flaking, and include the well-known rostro-carinate form, together with scrapers, borers, choppers, etc.[1]

[1] "The Flint Implements of Sub-Crag Man," J. Reid Moir, *Proc. Prehis. Soc. of East Anglia*, vol. i. part 1, pp. 17–43; "On the Discovery of a Novel Type of Flint Implements . . ." Sir Ray Lankester, *Phil. Trans.* Series B, vol. ccii. pp 283–336; "On the Further Discoveries of Flint Implements beneath the Base of the Red Crag of Suffolk," J. Reid Moir, *Proc. Prehis. Soc. of East Anglia*, vol. ii. part 1, pp. 12–31; "Implements of Sub-Crag Man in Norfolk," W. G. Clarke, *ibid.* vol. i. part 2, pp. 160–8.

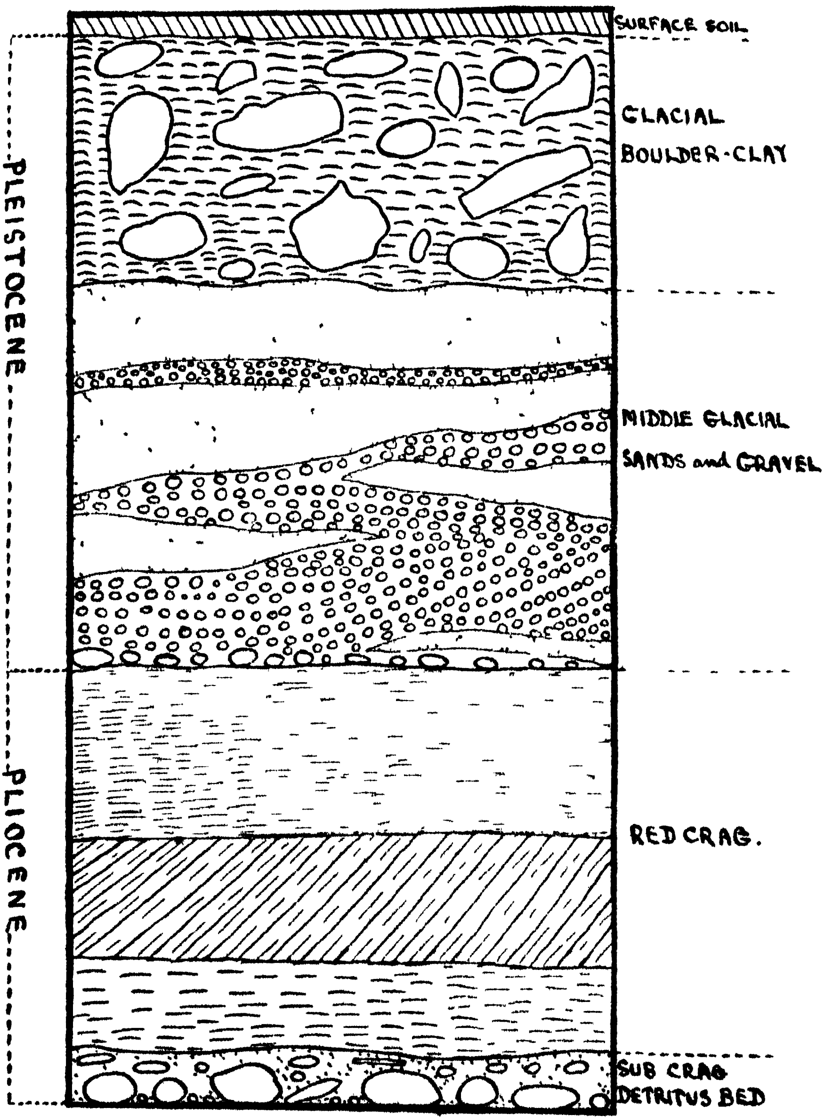

FIG. 2.—Vertical section showing the principal beds forming the plateau near Ipswich. Pre-palæolithic flint implements have been found in the sub-Crag detritus-bed, the Middle Glacial Gravel and the Glacial Boulder Clay.

Though these specimens occur sealed down beneath a definite Pliocene deposit, it is evident that they cannot represent man's first efforts in flint-flaking, and the earliest examples

of his handiwork must in consequence be looked for in still more ancient strata.

The detritus-bed is generally about six to twelve inches in thickness, and the crag surmounting it may be twenty or more feet in depth. But it seems probable that during the succeeding glacial period a large amount of the crag was eroded away, and that originally, therefore, there was a much greater thickness than is now present. The Middle Glacial Gravel which usually overlies the Red Crag is of Pleistocene date, and supposed to be intermediate in age between the earliest glacial deposits of the Norfolk coast sections and the later Glacial Boulder Clay.[1]

This Gravel, which was probably formed by the breaking up and redeposition of an ancient land surface, contains flint implements of different ages, all of which exhibit flaking of a different order to that in vogue in pre-Crag times.[2] Above the Middle Glacial Gravel occurs the well-known Chalky Boulder Clay which is supposed to be the direct result of land ice during the glacial period. In this deposit I have found another series of humanly flaked flints which again differ in their technique from the succeeding Middle Glacial and pre-Crag specimens.[3] The three implementiferous beds I have described must not be supposed to represent all the geological strata which go to form the plateaux of East Anglia, nor must it be imagined that the Red Crag period was succeeded by the epoch in which the Middle Glacial Gravel was laid down. Between these two periods a number of other beds were deposited, but these are not represented in the Ipswich district. The Middle Glacial Gravel and Chalky Boulder Clay do, however, represent a correct sequence in point of time.

This brief account of the pre-palæolithic implements of East Anglia and elsewhere will serve to demonstrate that in England we have very clear evidence of the presence of races of people making implements of a primitive type and who lived prior to the time when the earliest palæolithic implements were fashioned.

[1] *Geology in the Field*, part 1, p. 120 (F. W. Harmer, "The Pleistocene Period in the Eastern Counties").

[2] "Flint Implements of Man from the Middle Glacial Gravel and the Chalky Boulder Clay of Suffolk," J. Reid Moir, *Proc. Prehis. Soc. of East Anglia*, vol. i. part 3, pp. 307-19.

[3] *Ibid.*

That appears to me to be a fact of the first importance far exceeding in scientific value speculations regarding ancient, unknown Asia.

The second fact which, in my opinion, lends support to the view that the flint implements and actual skeletal remains of pre-palæolithic man are represented in this country, is afforded by the now famous discovery at Piltdown in Sussex. I confess I am somewhat loth to add fuel to the fires of controversy which since their discovery have raged round these ancient human remains. But in my judgment these relics have been so extraordinarily misinterpreted, both as regards their age and significance, that I feel it to be necessary to comment upon them.

As is well known, the late Mr. Charles Dawson, of Lewes, found at the base of a thin deposit of gravel at Piltdown portions of a very thick and massive human skull, associated with half of a remarkable human lower jaw.[1] This jawbone and a very large canine tooth found near it show in many respects distinct simian characteristics, while the skull, whatever its exact form and size may be, is clearly human in its characteristics.

Thus we find in this unique fossil a combination of human and simian characters, such as have been looked for by evolutionists ever since Darwin first enunciated his famous theory regarding the ancestry of modern man. But all evolutionists would agree that such a form would occur only at a very early stage of man's development, and this is a fact of great importance. Putting aside for the moment all considerations of the geological age of the Piltdown remains, and the type of flint implements found with them, we may say with confidence that the whole aspect of the bones, their condition of fossilisation and their half-human, half-simian character, point to a very great antiquity. How does the geological evidence affect such a conclusion?

The human bones were found in the lowermost stratum of a thin deposit of gravel resting at about 120 O.D. and approximately 80 feet above the level of the Sussex Ouse. The height at which any particular deposit occurs above sea or river-level is in itself of almost negligible value. Ancient

[1] "On the Discovery of a Palæolithic Skull and Mandible . . ." Charles Dawson and Arthur Smith Woodward, *Q.J.G.S.* March 1913, vol. lxix.

beds may outcrop at a very low level, while much newer strata may occur at a considerable height above sea or river-level. I mention this to demonstrate that the antiquity of the Piltdown gravel cannot be determined by reference merely to the height at which it occurs.

It is necessary, therefore, to examine the actual strata composing the gravel-bed. Mr. Dawson, in his very accurate and careful examination of this deposit, recognised four well-defined strata,[1] and it was in the lowermost but one of these, resting upon " a pale-yellow finely divided clay and sand," that the human remains were found. With these remains were found fragments of the bones and teeth of six different animals, two, and most likely three of which can be referred with certainty to the Pliocene period. The three other mammals, red deer, horse, and beaver, may or may not be of Pliocene date.

Of the beaver Dr. Smith Woodward states[2] that it is " most probably Pleistocene," while of the red deer he remarks that typical specimens " have never hitherto been found below the Pleistocene." The discoverers of these various relics at Piltdown draw attention to, and seem to lay stress on, the fact of the difference in condition of the specimens as regards the amount of rolling by water to which they have been subjected, and apparently attempt to draw conclusions regarding their respective ages from such differences. My experience, however, has led me to place little value on the condition of any constituents of a deposit of gravel, and I have frequently found specimens of contemporaneous date and lying in close proximity to each other, some of which are rolled while others exhibit scarcely any signs of their transport. And if one attempts to envisage the multifarious varieties of treatment to which specimens in a gravel would be subjected during its deposition, such apparent paradoxes are not very difficult to understand.

I cannot but think that had no human bones been found in the Piltdown Gravel there would not have been such an evident attempt to place it in the Pleistocene period, and

[1] " Supplementary Note on the Discovery of a Palæolithic Skull and Mandible . . ." *Q.J.G.S.* April 1914, vol. lxx. p. 83.

[2] " On the Discovery of a Palæolithic Skull and Mandible . . ." *Q.J.G.S.* March 1913, vol. lxix. p. 148.

while it is without doubt wise to err on the side of caution in these matters, it is well to remember that over-caution may lead to as much misconception as over-confidence.

I see no reason why the lower stratum of the gravel at Piltdown should not be a Pliocene deposit overlain by gravelly strata of later date, and I know that such an association of beds of different ages occurs not infrequently in the valleys of East Anglia. *In a Supplementary Note on the discovery of the Piltdown Skull*[1] Messrs. Dawson and Smith Woodward state, "We cannot resist the conclusion that the third or 'dark bed' is, in the main, composed of Pliocene drift, probably reconstructed in the Pleistocene epoch," and if, as I think, such reconstruction is *very* problematical, it is clear that the geological evidence does not conflict seriously with the view based upon the characters of the human bones themselves, that these bones must be of a very considerable antiquity. But it is when we turn to the evidence afforded by the flint implements found with the Piltdown person, that this great antiquity seems definitely established. If I, as a practical flaker of flint, had been shown Dr. Smith Woodward's reconstruction of the Piltdown skull and jaw, and had been asked what sort of flint implements in my opinion such a very primitive semi-human creature would be capable of producing, my answer would have been "the very primitive edge-trimmed flints generally known as eoliths."

I should not regard it as in any way probable that such an ape-like man would be able to make even the earliest of the pointed or ovate palæolithic implements, which I know from experience require much thought and skill to produce. Now if we turn to the excellent illustrations of the flint implements found *in situ* at Piltdown,[2] it will be realised that such a supposition is correct, as not a single example of a pointed or ovate palæolith appears among these illustrations. There are certainly some flints illustrated of which the legend reads, "Palæoliths from Piltdown," but I beg leave to dispute the correctness of this description. Some little time ago Dr. Smith Woodward very kindly gave me an opportunity of examining these specimens, and I at once recognised, as is also clear in the drawings, that the workmanship of the flints is quite dis-

[1] *Q.J.G.S.* April 1914, vol. lxx. p. 85.
[2] *Q.J.G.S.* March 1913, vol. lxix. Plates XVI. and XVII.

tinct from the technique of the makers of the normal palæolithic implements. The ill-defined cones of percussion, and rough heavily truncated flake-areas of the Piltdown specimens, stamp them indelibly as the work of pre-palæolithic man, and further that their makers lived a long way back in the pre-palæolithic epoch.

I have found such specimens as these below the Pliocene Red Crag, and would refer the reader to an illustration in one of my published papers which testifies to the truth of this statement.[1] But when it is realised that these particular flint implements, which as I have shown pre-Crag man was capable of making, are the latest found in the Piltdown Gravel, and that they occurred in a stratum less ancient than that containing the human bones, it will be seen that we are dealing with the remains of a person who in all probability existed at a period the remoteness of which makes the palæolithic epoch seem comparatively modern. For the only implements found in the "human" stratum and in intimate association with the Piltdown individual were the primitive edge-trimmed flints generally described as eoliths.[2] This particular type of implement represents, as I have shown in a former article in SCIENCE PROGRESS,[3] the earliest efforts of man deliberately to shape flints to his needs. I have also shown in the same publication[4] that there is very good reason to believe that these "eolithic" implements were the precursors of the rostro-carinate form found in the sub-Crag detritus-bed. Thus it would appear probable that the human remains from the Piltdown gravel must be referred to a member of the early pre-palæolithic period—and Piltdown is not in Asia.

There seems to me to be no escape from these conclusions. All I ask is that the antiquity of the Piltdown remains may be computed in the same way as the age of all other ancient human bones. In the case of the Neanderthal and other Pleistocene skeletons which have been found, their antiquity has been decided upon by reference to the geological strata in which they lay, and the fauna and flint implements with which they were associated.

[1] *Proc. Prehis. Soc. of East Anglia*, vol. ii. part 1, Plate VIII. fig. B.
[2] *Q.J.G.S.* April 1914, vol. lxx. pp. 84 and 85, also Plate XIV.
[3] No. 43, January 1917, pp. 431–40.
[4] No. 45, July 1917, pp. 83–96.

If this method, the only possible method, is applied in the case of Piltdown, the evidence is quite overwhelmingly in favour of an early pre-palæolithic date for the human bones found there. There appears to be no possibility of regarding them as of palæolithic antiquity.

In my judgment then we have most definite and clear evidence that the pre-palæolithic peoples are abundantly represented in this part of the world. This conclusion is based on the following facts.

1. The discovery in various parts of England of different kinds of flint implements in deposits which are of a greater antiquity than those containing the earliest palæoliths.

2. The discovery at Piltdown in Sussex of the remains of a very primitive type of human being in intimate association with certain definite Pliocene mammalian forms, and the earliest kind of flint implements known to science.

The neolithic and palæolithic stages in this country are fairly well known, but the vast pre-palæolithic periods await examination.

These periods are fully represented in England, and the flint implements, etc., contained in the deposits laid down during these epochs must be collected and investigated. Such an investigation, I submit, will show that the evidences of man's pre-palæolithic history are as abundantly represented here as in any other part of the world, and lead us to be careful in speculating as to unknown Asia's monopoly in this direction.

NOTE.—In the article on "The Relationship of the most ancient Flint Implements to the later River-Drift Palæoliths" in SCIENCE PROGRESS, July 1917, p. 93, fig. 5*a* does not show the crosses mentioned in line 13.

POPULAR SCIENCE

THE STRUCTURE OF MATTER

By Prof. W. C. McC. LEWIS, D.Sc.,
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Part I

It is well known that matter is capable of existing in three physical states, the solid, the liquid, and the gaseous, and that transition from one state to another is possible under certain conditions. These physical states correspond to different degrees of spatial distribution of the exceedingly minute discrete particles which we call molecules. It is not sufficient, however, simply to know, as a result of observation, that such modes of distribution can exist; the aim of molecular physics has long been to understand and explain the molecular mechanism which underlies these modes of molecular distribution, and the conditions which determine transition from one to the other. Of course, nothing like finality has been or can be attained in such investigations. It is of interest, however, to review in a very brief manner some of the more important advances which have been made in recent years in this wide and intensely interesting field.

Evidence for the Real Existence of Molecules

The concept of discrete particles or molecules, possessing mass and velocity, and therefore kinetic energy, has been employed for the greater part of a century as a satisfactory working hypothesis, by means of which we are able to account for the behaviour of gases, as expressed in the well-known gas laws of Boyle and Gay-Lussac, and to a less degree the behaviour of liquids. It is only within very recent years, however, that satisfactory evidence for the real existence of molecules has been brought forward. This very considerable step we owe to the French physical chemist, Perrin (1) and after

him to the American physicist, Millikan (2). The phenomenon investigated by Perrin, a phenomenon which at first sight would seem to have very little to do with the existence of molecules, was the well-known Brownian movement of small particles suspended in a liquid medium. If, for example, we examine by means of a high-power microscope a liquid in which very fine particles are suspended, such as gamboge in water, or the still finer particles of colloidal solutions, which can be rendered visible (indirectly) by means of the ultra-microscope, we find that these particles are in a state of rapid and irregular motion. Early investigations of Ramsay, and still more those of Gouy (3) had served to demonstrate that the most likely cause of this movement was the bombardment of the particles by the molecules of the surrounding medium. Perrin succeeded in putting this conclusion to a quantitative test. He showed, for example, that the suspended particles distributed themselves in such a manner that their concentration at the bottom of a cylinder was considerably greater than that at the top, and that, in fact, the concentration varied quite regularly according to a simple law. Having obtained the necessary data in a fairly exact form, he proceeded to apply certain theoretical considerations which necessarily follow from the assumption that the Brownian movement is due to molecular bombardment and is at the same time governed by the principles of the classical statistical mechanics, *i.e.* the principle of equipartition of energy. In this way, Perrin succeeded in establishing a relation between the concentration gradient of the gamboge particles and the quantity N, a quantity which stands for the actual number of molecules in one grammolecule of a gas, to which Perrin gave the name, the Avogadro Constant. The value thus obtained for N agreed so well with certain approximate determinations which had previously been made that there could be no reasonable doubt but that the Brownian movement was actually due directly to bombardment by molecules. Hence, although we have not yet succeeded in seeing a molecule, we have actually before our eyes an irregular motion analogous to molecular motion itself and directly caused by the movement of molecules. Later on, Millikan improved the technique of such experiments by following the movement of a tiny droplet of oil suspended in air, and has in this way arrived at a very

exact value for the Avagadro Constant. We may take this value as $6 \cdot 1 \times 10^{23}$. If we pause to consider the enormous magnitude of this quantity, a quantity which is known at the same time with a considerable degree of precision, we cannot but be struck by the great advance which has been made in our knowledge of the molecular world as a result of such researches. The above number means that in any gas, which obeys the gas law, the number of molecules present in one cubic centimetre at normal temperature and pressure is very nearly $2 \cdot 8 \times 10^{19}$. The mass of a single molecule—of hydrogen, say—is also easily calculable from the data given and comes out to be $3 \cdot 2 \times 10^{-24}$ gram. That is, one gram of hydrogen contains 300,000,000,000,000,000,000,000 molecules. To give a somewhat more tangible, though less exact, idea of what such numbers represent, we cannot do better than recall the classic illustration of Lord Kelvin, viz. if we imagine a small drop of water magnified to the size of the earth, the molecules would then be visible, their size being somewhere between that of a cricket-ball and that of small shot.

The Gaseous and Liquid States

Whilst we are on the subject of molecular magnitudes, it will be convenient to recall a few other related quantities which have become known to us as a result of investigations of the gaseous state. The first of these is the diameter of a molecule. Knowing the number of molecules per cubic centimetre, it is possible to calculate the diameter of each by making use of viscosity data. The following table contains a few of the values obtained in this way by Sutherland (4):

Gas.	Molecular diameter.
Hydrogen	$2 \cdot 17 \times 10^{-8}$ cm.
Oxygen	2·71 "
Nitrogen	2·95 "
Carbon dioxide	2·90 "
Chlorine	3·74 "

It will be seen that the dimensions of a molecule are extra ordinarily minute, so minute, in fact, that if we imagine a number of hydrogen molecules placed end to end, it would require fifty million of them to form a row one centimetre in

length. It is well to remember that the picture we have in mind at this stage is that of a molecule as a small sphere, which can collide with and rebound from other spheres. That is to say, we are thinking of a molecule as possessing a definite though extremely minute surface. In the next section we shall see that this idea is too crude, for, when we come to think of it, a surface is, after all, a geometrical and not a physical concept.

The next magnitude with which we have to deal is that known as the mean free path of a molecule in the gaseous state, that is, the mean or average distance which a molecule traverses between two consecutive collisions with other molecules. In the following table are given the values of the mean free paths of a few gases at normal temperature and pressure together with the values of the collision frequency, that is, the number of collisions which on the average any single molecule experiences in one second. This latter quantity is extremely large, one hydrogen molecule, for example, colliding with its neighbours some nine thousand million times per second.

Gas.	Mean free path in cms.	Collision frequency.
Hydrogen	0·0000182	$9{\cdot}28 \times 10^9$
Oxygen	0·0000094	4·82 "
Nitrogen	0·0000100	4·28 "

It will be observed that under moderate pressures the mean free path of a molecule is very much greater than the actual diameter of a molecule, in fact, several hundred times as great. Under these conditions, therefore, the molecule is very small compared with the mean free space allotted to it, so that, in a preliminary treatment of the gaseous state, it is quite justifiable to neglect the actual volume of the molecules themselves compared with the total volume occupied by the gas. With a simplification such as this it is an easy matter to arrive at the well-known gas law, $PV = RT$, where P is the pressure exerted by the gas, V its total volume, T the absolute temperature, and R a constant having the same numerical value for one grammolecular weight of any gas. If, however, the pressure be greatly increased, say by compressing the gas, the mean free path will diminish, and as the

molecules themselves are only slightly compressible, a stage will be reached finally at which it is no longer justifiable to neglect the actual volume occupied by the molecules in comparison with the total volume. This is particularly the case with liquids, for it is well known that in liquid systems the average distance of the molecules apart is just about the same order of magnitude as the molecular diameter itself. To allow for this effect, van der Waals, many years ago, introduced a volume correction term into the gas law, denoting the volume of the molecules when in contact by the term b, so that, if V is the total observed volume of the compressed gas or liquid, the true compressible volume or empty space is $(V-b)$. As a first step in the attempt to modify the gas law, $PV=RT$, so that it may apply to compressed gases and to liquids, we may write the expression $P(V-b)=RT$. This, however, is not a sufficient modification. We have still to consider another very important factor which enters into the question, namely, the existence of molecular attractions. As long as the molecules are not very close together this force is negligible. When, however, as a result of compression, the molecules are brought into closer range, mutual attractions must be taken into account, for it is owing to these that the gas possesses less expansive force than it would otherwise have. In other words, a gas becomes too easily compressible and diminishes excessively in volume as the external pressure upon it is increased. The pressure actually exerted by the gas under these conditions is of course P, measured by the manometer. But this value is really smaller, and may be much smaller, than the pressure which the gas would exert if molecular attraction were absent. These attractions act like an additional pressure which we shall denote by the term π, so that the total compressing agencies can be represented by the sum of the terms $(P+\pi)$. The modified gas equation may be written, therefore: $(P+\pi)(V-b)=RT$. This expression is undoubtedly a truer representation of the behaviour of a compressed gas or liquid than is the simple gas law itself. We have not made much progress, however, until we can express π in terms of measurable quantities. Van der Waals suggested that π might be written as a/V^2, where a is regarded as a constant, characteristic of a given substance, whether in the gaseous or liquid state, and V may be taken

to be the specific volume or volume of one gram of the substance. It is impossible to follow van der Waals' train of reasoning in arriving at this conclusion. Its general reasonableness may be shown, however, in the following way. We are already familiar in the phenomenon of gravitation with the fact that any two pieces of matter attract one another, and that this attraction is the greater, the greater the masses of material and the closer they are together. In the case of molecular cohesion we are apparently dealing with a phenomenon of a different kind, one of far greater intensity when the particles are close together, but dying off much more rapidly than gravitational attraction does, as we draw the two molecules apart. It is unlikely that molecular cohesion and ordinary gravitation have anything in common, except that they both represent forces of attraction. In the phenomenon of cohesion it seems reasonable to assume that the cohesion will be the greater the more closely packed are the molecules. That is, the cohesion π across any unit plane is some function of the density of the substance, for the more closely packed are the molecules the greater is the density. π, therefore, increases with the density, but what is the proper function to employ? In expressing forces of attraction, gravitational or electrostatic or magnetic, we find in general that the expressions have something in common as regards their form. What is common to them is the occurrence of a product of two quantities, masses or charges, or pole strengths. In the present case, since density or close packing seems to be the significant thing, we are led by a very rough analogy to think of cohesion as depending upon the product of the densities of the substance on the two sides of the unit plane. Since the substance is the same in every respect on both sides of the plane, we conclude that the cohesion is proportional to the square of the density. But the density is simply $1/V$, where V is the specific volume, so that π is proportional to $1/V^2$. Denoting the proportionality factor by a, we can write $\pi = a/V^2$. It must be clearly borne in mind that the above reasoning is by no means rigid. The density term enters into the expression not in virtue of the mass of each molecule—for that would be equivalent to identifying cohesion with gravitation as regards nature, though not necessarily as regards degree—but in virtue of the fact that the greater the compression, the greater the *number* of molecules

which will exert their influence across the unit plane. Introducing the above expression for π into the modified gas equation we get an expression known as van der Waal's equation, viz. $(P+a/V^2)(V-b)=RT$.

It would be quite beyond the scope of this article to deal with the ramifications of this expression, and the very full experimental tests to which it has been subjected. Suffice it to say that, in many respects, it gives a wonderfully good representation of the general behaviour of gases and liquids. Notably it has served to indicate the conditions under which transition from the liquid to the gaseous state can take place. It affords a theoretical basis for the principle of continuity of state, first enunciated on experimental grounds by Andrews, according to which there is for every substance a certain critical temperature above which it is impossible to condense the gas to the liquid form, howsoever great the degree of compression may be. At the same time the equation breaks down in certain important quantitative aspects. For this reason, other expressions have been proposed to account for the temperature-pressure-volume relations of gases and liquids of which the most noteworthy is that of Dieterici (5). It cannot be said, however, that we have even yet attained to a really satisfactory representation of the behaviour of such systems under various conditions of temperature and pressure.

Before leaving the van der Waals equation it is important for our present purpose to point out that the assumption, that the internal pressure or cohesive force per unit area inside a liquid or compressed gas can be expressed as inversely proportional to the square of the volume, really amounts to a law of force for the attraction between any pair of molecules, which varies as the inverse fourth power of the distance from the centre of the molecule. This point has been very fully examined by Sutherland, more especially in view of the fact that an inverse fourth power law is the one to be expected for an electrical doublet, consisting of a positive and a negative charge, acting upon a similar doublet. The evidence brought forward in this connection is fairly convincing, and leads to the conclusion that molecular attractions are essentially electrical—electrostatic—in nature. This suggests at once an electrical structure for molecules themselves, *i.e.* that they consist of positively and negatively charged particles

of matter. Further, an inverse fourth power law means that the force of attraction dies away very rapidly as we pass out from the molecule. A rough estimate of this may be made in the following way. Knowing the number of molecules in a given volume of a normal liquid, that is, a liquid in which the molecules have not coalesced to form double molecules, etc., and also knowing the average size of each molecule, it is an easy matter to determine the average distance apart of any two contiguous molecules. The distance from centre to centre is about 5×10^{-8} to 1×10^{-7} cm. Now by means of a thermodynamical relation, into which it is unnecessary to go, it is possible to calculate the heat which would be absorbed if we could imagine one cubic centimetre of the liquid extended until it occupied just twice its original volume. The writer (6) has calculated the values of these heat absorptions in the case of a number of normal liquids and has found that they are quantitatively very similar to the internal latent heats of vaporisation per unit volume, that is the amount of heat which is absorbed in transforming one c.c. of liquid into the state of vapour. The fact, that the energy absorbed in the two processes is practically the same, means that by distending the liquid to twice its former size we have already practically overcome the forces of cohesion, for the much greater distension involved in the act of vaporisation requires practically no further addition of energy. If $r = 1 \times 10^{-7}$ cm., is the distance apart of two molecules in the original state, then the distance apart when the volume is doubled is about $1{\cdot}3 \times r$. Hence on increasing the distance of the molecules apart in the ratio of $1{\cdot}3 : 1$, we infer from what has been said that the internal cohesion has now dropped to small dimensions. We conclude, therefore, that the effective range of molecular attractions, that is, the range throughout which the attractive force is still sensible, is of the order 1×10^{-7} cm., a very small quantity indeed.

Although the effective range of molecular attraction is small, it must not be inferred that attraction itself is small. Quite the reverse is the case. Naturally the magnitude of the force varies with the distance from the molecule, but we can get some idea of the quantity by taking the actual case of some liquid substances as they exist under ordinary conditions. The values given below can only be regarded as approximately

correct. They indicate, however, how enormous these forces of cohesion are in liquids. Most of the values quoted are taken from a paper by Tinker (7).

Liquid.	Internal Pressure at O.C.
Ethyl ether	5920 atmospheres
Ethyl chloride	6550 "
Ethyl acetate	9600 "
Benzene	11100 "
Water	*ca.* 25000 "

With such forces as these it will be readily appreciated that the correction term, a/V^2, in the van der Waals equation is really of far more importance than the P term itself in the case of the liquid state.

We have now dealt with some of the more important properties of molecules *qua* molecules. The next point for consideration is the structure of an individual molecule. We shall deal with molecules of gases and liquids and leave the question of solids until later, as the solid state appears to be characterised by certain fundamental properties not shared by liquids or gases.

In general, molecules contain two or more atoms. The exceptions to this statement are the rare gases of the atmosphere and the vapours of metals, which are monatomic. Chemical reactions in the majority of cases are essentially atomic reactions, *i.e.* they involve the transfer of an atom or group of atoms from one molecule to another, or the building up of quite new molecular structures by the assembling of atoms obtained from other molecules. We shall refer to this very briefly later. The question of the structure of the molecule resolves itself, in the first place, into the question of the structure of the atom. This is considered in the next section.

(To be continued)

CORRESPONDENCE

To the Editor of "Science Progress"

THE ABOLITION OF SLUMS

FROM LORD LEVERHULME

Dear Sir Ronald, I am very pleased to note from your letter that you are taking an interest in the pages of Science Progress in the Abolition of Slums and the consequent elimination of the slum menace to the health of the people.

It has always appeared to me that our slum problem is merely a case of bad "packing," if I may use such an expression. There is not, as far as I know, a town of any size in the United Kingdom that has not its slum area ; and it is equally true that there is not a town with a slum area that has not within its boundaries a sufficient area of land to accommodate, and that comfortably and under ideal conditions, three times their present population. I think we may take London as a fair example of this ; and, as you know, London has the most appalling slums of any town in the Kingdom. The total Metropolitan area is some 450,000 acres. Ideal Garden Cities, with ample space for gardens, parks, recreation grounds, good wide roads planted with trees, can be obtained on the basis of ten houses per acre, with ample space for certain portions of the area to be devoted to docks, warehouses, manufactories, and so on. With ten houses to the acre and an average of five individuals per house, you can see that the London Metropolitan area could accommodate 22,500,000 of people under ideal conditions instead of the 7,500,000 of people under conditions which make slums inevitable for a very considerable proportion.

I am not sure myself whether our methods in the past of dealing with slums, viz. to purchase the slum property and clear the site and build thereon blocks of tenement houses where the people live in flats, is the final word or the best method for clearing slums out of our cities. Flats are certainly not suitable for children, who ought to be able to run in and out of doors

without climbing stairs, so that they can play and obtain exercise easily and without crowding up staircases and passages, as must happen with families in apartments opening on to a common staircase and passages, and piled one above the other as in the modern tenement house. Every house wants, and ought to have, a fringe of grass between the windows and the road to allay dust and an area in the rear for gardens where vegetables may be grown and general reasonable open-air exercise obtained by the dwellers in the odd five, or ten minutes, or half-hours, that can be thriftily so employed throughout the day. When allotment gardens are half an hour's walk from the house this is impossible. The allotment garden ought to be right at the rear of the house, and all this is possible on the basis of ten houses per acre.

It has always seemed to me that each Municipality ought itself to acquire, as occasion offers, the fringe of land on its suburbs, and that this land ought to be planned and laid out by the Municipality to meet all the requirements as to manufactories, railways, etc., and for housing the people on Garden City lines. I go further than this, and hold that the Municipality could afford to give this land to those who would build the houses thereon. At present the great difficulty in providing cottages and houses is the land question. Land is very cheap when purchased at a particular moment, as at the death of the late owner when the land is being sold by order of trustees, especially if the area of land to be dealt with is large. I have known within the last twenty years such an estate of about 400 to 500 acres within seven miles from the Marble Arch to realise only £50 per acre. But, when the wave of building reached such an estate, and small plots were applied for for building purposes, the ordinary market value for building-land would apply, and necessarily so. Also, under existing conditions, the owner has no power to lay out his land to the best advantage for the whole district. If he asked to carry a street through a neighbour's land he would be refused; whereas the Municipality, under the Town Planning Act, could not only acquire certain of the land, but could make a plan and scheme for the remaining land in the whole district with a view to meeting every requirement of direct access to tram or train or omnibus. Suppose we imagine that such an estate was acquired in the suburbs of any of our manufacturing towns,

and suppose that the price paid was £100 per acre, such portions of the estate as were required for warehouses or manufactories could be sold at the current market value of land in the district—unless it was desired to induce some manufacturer to come into the district who would otherwise be likely to go elsewhere, with a view to finding employment for the people. Then it is obvious that for each acre of land let for building houses or cottages, supposing it was planned for ten cottages per acre and that the rateable value of each cottage was £20, then each acre would have a rateable value of £200 less whatever were the rebate on compounded cottage rating, which might be, say, 20 per cent., leaving a net rateable value of £160 per acre. The rates most likely would average 8*s*. in the £, so that the Municipality would receive £64 per acre. This £64 would not be net income. There would be the cost of lighting and policing and scavenging the district, though, as far as my knowledge goes, the burden on the rates for these necessary expenses is very light in suburban areas. The frontagers to the roads would pay for the cost of making the roads and for the cost of sewers. It is quite obvious, therefore, that out of the £64 income, interest and sinking fund on the £100 could be readily provided. I know it is objected that to adopt this plan would benefit somewhat the builder of the houses, in that he pays neither ground-rent nor capital value; but I would point out the enormous risk there is in building cottage property, and that this inducement would be merely a minimising of the risk by making the building of cottages a more attractive investment than it is at present, all of which would benefit the whole community.

Under town-planning such as I have suggested, rapid transit to and from the centre of the town to the suburbs (where the workers would live) could be arranged; and it is a well-known fact that workmen's trams, omnibuses and trains, although at reduced fares, are extremely profitable to transport companies.

This would now enable us to deal with the slum areas. Having provided cheap and comfortable houses in the suburbs with cheap and rapid transport to and from the factory to the workers' homes, the slums would become practically tenantless. We must remember that the modern slum is created by overcrowding. The workman is forced to live in the slum in the

early stages of the slum-dwelling because he is unable to find any other place to live in. Such a man would be the first to leave the slum. If he is not taken out of the slum, he and his wife and family would so degenerate that in the process of time they would actually be loath to leave the slum. They would have sunk in the social scale to the level of the slum and would not feel happy in better surroundings.

You will see, therefore, that I would not advocate a frontal attack on slums. I would use the money that it would cost to buy up and diminish slums for the purchase of land in the suburbs, and I would draw away the slum-dweller to better living conditions.

The final stage would be the demolition of the slum area, which by that time would be purchaseable at a proper price, and not the exorbitant price that at present has to be paid. The slums demolished, the area could very often be so laid out as actually to show a profit by the use of the site for better purposes ; but on no account should tenement houses be built in the slum areas. If what had formerly been a slum area was not required for better uses by the town, then it should be laid out on a basis as nearly approaching the basis of ten houses to the acre as possible and used for the housing of the people. These would then become veritable oases in the desert of weary, monotonous streets.

Now, with regard to the prevention of the growth of new slums in the future, this is entirely a matter of town-planning. Let a town be properly planned, then building regulations can be enforced under the Town Planning Act, limiting the number of houses per acre, and slums become impossible ; for, once the streets are laid out on what I may call broad-gauge lines and the property gets built thereon, it can no more be altered in the future to produce a tangle of mean streets and sordid dwellings than you could change Kensington or Regent's Park. It was the original planning of the slum area on the basis often of fifty houses to the acre (in some cases what are called "back-to-back" dwellings, in other cases what are almost as bad, with only barrow-ways at the backs between two monotonous rows, and the permitting of building under these conditions) that produced the slum. I have known slum conditions to appear in an area so planned and built on within twenty years—that area before the builder erected these over-crowded dwellings

being a rural district where the land was sold prior to this development at £50 per acre. It seems to be often an absolute infatuation of estate agents developing land on behalf of their principal to exercise the utmost ingenuity in showing the maximum number of houses that can be over-crowded on a given area of land, and a matter of professional pride to show the larger income that can be obtained in this way. Towns and Municipalities under the Town Planning Act are armed with powers to prevent this. They can town-plan in any district and can refuse to pass or approve of the erection of buildings after they have made their plan, except on the lines of their plan.

And now, with reference to the effect on the health of the people—statistics show that the average death-rate in communities where the houses do not exceed ten per acre generally runs about eight per thousand, as compared with an average death-rate of, I think, about fourteen per thousand for the whole country. The birth-rates in these areas are also considerably above the average for the country, in some cases as high as thirty-three per thousand. I believe the unsatisfactory housing condition of the workers of this country is a constant cause of irritation to them and danger to the whole community, not only because over-crowding brings ill-health, but because over-crowding means a shutting out of all healthy environment and real enjoyment of life. I rejoice at the workers' discontent as we see it to-day in the country. If the workers were content to live in slums or in monotonous rows of mean streets without a single square foot of ground into which they could put a spade or out of which grow a flower, under such conditions that their children can only play in the gutter, then the outlook for this country and the Empire would be gloomy indeed.

It may be that some people are alarmed at the sign of what is called "industrial unrest," but they would have much more cause to be really alarmed if there were industrial rest and acquiescence in present conditions. The whole subject is so easy of adjustment. It is merely a question of better planning, or "packing," if I may use the illustration I employed at the commencement, and of better organisation of the spaces available.

Yours sincerely,

Leverhulme.

NOTES

Prof. Adolf von Baeyer (Frederick A. Mason, B.A., Ph.D.)

CHEMISTS the world over will learn with regret of the death of the famous Professor of Organic Chemistry, Adolf von Baeyer, who has just died in Munich in his eighty-second year.

His full official title was Seiner Exzellenz Geheimrath Professor Doktor Adolf Ritter von Baeyer, but he was known familiarly to at least two generations of chemists in every land simply as "Baeyer," and he may without fear of contradiction be regarded as one of the founders of modern organic chemistry.

His researches and those of his pupils into the inner mysteries of synthetic organic chemistry were so profound, so widespread, and so fundamental that it is safe to say that organic chemistry as we know it—the chemistry of life and vital products, and of synthetic dyes, drugs, high explosives, and the like—would have been impossible if Baeyer had not laid the foundations both by his numerous researches and by the training he gave to the many distinguished chemists who passed through his hands. In the technical world too, both by his teaching and his activities in the laboratory, he assisted in a marked degree to assure the unique position which Germany held before the war in chemical industry.

It is sufficient to note that he was the first chemist to synthesise indigo from coal-tar products, and so to open the way to Germany's successful competition with natural indigo. This achievement alone would have sufficed to assure his fame throughout the world, but indeed that rests on a broader basis than this one success.

Johann Friedrich Wilhelm Adolf Baeyer was born on October 31, 1835, as the son of Staff-Major (later Lieut.-Colonel) J. J. Baeyer and Eugenie his wife, at 242 Friedrich Str. Berlin. From early days he was attracted by chemistry, and even at the age of nine we find him discovering a new compound of soda and copper carbonate, and delivering lectures of a sort to a school-boy friend who, in return for this honour, undertook to clean the bottles!

Von Baeyer studied at Berlin University for a time, and from there he went to Heidelberg in 1856 to work with "Papa" Bunsen, and there made the acquaintance of many well-known chemists, such as Roscoe, L. Meyer, Beilstein, and others: after a short time, however, he transferred his allegiance to Kekulé, who was also teaching in Heidelberg and later in Ghent. In 1858 Baeyer went to Berlin for his final examination, where he had to be content with a "second." He then stayed at Berlin as a privat-docent for some years, coming thus under the influence of A. W. von Hofmann, who had left the Royal College of Chemistry in London—largely on account of the unscientific and unsympathetic attitude of the British public—to accept the professorial chair in Berlin in 1865 (in passing it will be remembered that it was in 1856 that Hofmann's pupil, W. H. Perkin, discovered mauve, the first aniline dye), and shortly after Hofmann appointed Baeyer as Professor Extraordinarius. In 1868 Baeyer married the daughter of Geh. Bindemann.

In 1872 Baeyer was called to Strassburg as Professor of Chemistry and

director of the new chemical laboratories in which many famous chemists studied—such as Emil Fischer, C. Graebe, and C. Liebermann (the discoverers of synthetic madder or alizarine, which effectually destroyed the French madder industry), Victor Meyer, and many another. Baeyer stayed only a short time in Strassburg, being called to Munich in 1875 as successor to Baron von Liebig; here he built the new Chemisches Institut, where two generations of organic chemists of all nationalities have received their training, and under his guidance the chemical school at Munich became world-famous.

It was in Munich chiefly that Baeyer carried through those researches in organic chemistry which have become classical. Of special interest to every one at the present moment were his researches upon the constitution of indigo, begun in 1865 and carried on for more than two decades, which have borne such remarkable scientific and industrial fruit. It would serve no purpose to describe the methods he adopted or the results obtained, except to point out that by brilliantly skilful and painstaking work he was able to establish the constitution of the indigo molecule and, as a result he was able in 1870 to show the synthesis in the laboratory of indigo, and later devised a second synthesis starting from toluene—now in such demand for T.N.T.—which had a certain slight technical success, some tons appearing to have been made by this process. His patents were acquired in 1880 by the Badische Anilin und Soda Fabrik and the Hoechst Farbwerke, who carried out conjoint research for eighteen years, expending nearly £1,000,000 on the problem, until at last two satisfactory processes were developed for the manufacture of indigo from coal-tar products, the one method starting from naphthalene and the other from benzene *via* aniline. Space forbids the discussion of the further development of this remarkable achievement of synthetic chemistry and its enormous economic significance, since it succeeded—aided by the ignorance and unscientific methods of the planters—in almost ruining the once flourishing Indian indigo trade, the price for the pure synthetic product being less than half that for the often very impure natural product. Baeyer and indigo are names for ever associated together in the minds of chemists, and the whole story serves well at the present moment as a superb object lesson to emphasise the results of close co-operation between pure scientific research and technical progress.

To enumerate all Prof. von Baeyer's discoveries would take us too far afield, but there is no gainsaying the fact that he occupied an almost unique and patriarchal position in the chemical world. His one-time students now occupy professorial chairs the world over. We have, to give only a few names, Prof. Noyes of Illinois, U.S.A., Prof. W. H. Perkin of Oxford; and of German professors, Emil Fischer (discoverer of veronal), Willstätter, Goldschmidt, Knorr (discoverer of antipyrin), may be mentioned.

Von Baeyer was a well-built man, slightly bald, but bearded, and of a dignified and venerable appearance, which earned for him the nick-name of "St. Peter" among his students. Right up to his eightieth birthday he delivered his course of lectures on five mornings a week at 8 a.m. in summer and 9 a.m. in winter, and was daily in his laboratory, wearing an old "squash" hat and smoking the inevitable cigar. There is reason to believe he viewed with disfavour and apprehension the rise of the Prussian military party to power, and it is improbable that he approved in any way of the aims or methods of Prussianism.

Industrial Research (From a Correspondent)

The pregnant sentence in the monograph by Mr. A. P. M. Fleming on this subject, which has recently been published by the Department of Scientific and

Industrial Research, that "Industry is the basis of national prosperity, and that every science should be used to facilitate its progress," deserves the careful consideration of all those concerned with the future welfare of our race and empire. The wonderful progress of German industry during the last thirty years and the resulting economic prosperity is a phenomenon which is now fully appreciated by all. It is a commonplace to-day to say that, had Germany continued the process of peaceful penetration into our industries that she had used during the last twenty years, there would have been no need for her to go to war; ten years hence her position as a world-power in industry and commerce would have been supreme. Those who know what was done in the production of scientific apparatus and the improvement of technical appliances cannot but be impressed by her efforts, and they realise that, although scientific and industrial research were by no means the sole reasons for her success, they contributed no small share to that result. In the meantime, America has been learning the lesson thoroughly, and the description given in the pamphlet referred to above should open the eyes of those in this country who still fail to appreciate the importance of applying all the available scientific skill and knowledge as much as possible to the development of industrial processes

Research work is carried out in America by the manufacturers themselves as well as by the Universities and other national institutions. Naturally manufacturers devote most of their energies to industrial problems, while the Universities and the National Bureau of Standards specialise on pure science and fundamental principles. There is, however, no hard and fast line; the Universities are taking up industrial problems, and are providing special facilities for dealing with them either in special departments or in already existing laboratories, while the work of the General Electric Company and the research laboratory it has established at Schenectady, which occupies itself very largely with work of a purely scientific kind, are so well known that it is unnecessary to describe them more fully here. It is the most striking example that exists of the recognition by a manufacturing firm of the influence that research work in pure science must have on manufacture. The work done by manufacturing corporations is divided by Mr. Fleming into five classes:

(1) Research designed to eliminate manufacturing troubles.
(2) Research having some new and special commercial object, such as the development of a new process for manufacture.
(3) Research in pure science made without any direct industrial object, but simply with the idea of enlarging the boundary of human knowledge.
(4) Research by public supply companies with the view of finding new uses for electrical energy.
(5) Research for the purpose of establishing standard methods of testing and standard specifications for raw material.

Although there is no doubt that manufacturers in this country must develop, ultimately, laboratories of the type described by Mr. Fleming, his memorandum will do much to stimulate their interest in this direction. One of the most important developments that may be expected in the future is a closer co-operation between the Universities and Technical Colleges of University standing with industry and manufacture. The tradition of British Universities in the past, based on Oxford and Cambridge, is becoming gradually altered by the more modern creations. It is no longer thought to be essential that the subjects of instruction should be non-utilitarian. There existed, and still exists in some quarters, the idea that a subject is only of educational value, and fit to be taught in a University, when it has no direct practical application in every-day life. The

famous toast that was drunk at one of the older institutions to celebrate a new discovery in science, "May the discoverer flourish, and may his discovery never be of use to any one," is a relic of a bygone but by no means distant past. There are, in fact, many in our modern seats of learning who, while they will not admit fully their point of view, are strongly averse from University study becoming in any degree mixed up with practical problems; they think that the function of a University is to promote pure learning, without admitting that the learning may be pure when it is in intimate admixture with the practice of science. That opinion is in process of decay (a decay which has become more rapid since the war), and which it is hoped may soon be as extinct as the dodo. As long as it prevails, the full use of our best brains in industrial science is impossible. The pamphlet just published by Sir Richard Glazebrook on *Science and Industry: the Place of Cambridge in any Scheme for their Combination*, lays stress on this point. "A distinction is often drawn nowadays between pure science and industrial science. I saw somewhere the other day a protest against the using of the latter term. Science is one, and industrial science, so called, is the application of the discoveries of pure science to the problems of industry."

There is, however, no general consensus of opinion as regards the contention in Sir Richard Glazebrook's pamphlet, that the National Physical Laboratory is the natural, and one might almost say, the only bridge between Science and Industry. A research worker attached to an Institution like the National Physical Laboratory, which carries on an enormous amount of routine testing, is hardly in as good a position for carrying out industrial research as colleges and Universities, where fellows and research students, who can devote the whole of their time to the solution of a problem, can work at leasure and consult with experts in all branches of science that may be related to the difficulty they have to solve. There are, undoubtedly, types of investigation for which the National Physical Laboratory, both by the nature of its equipment and its personnel, is peculiarly fitted, but colleges and Universities which are so fortunate as to be placed near great centres of industry, and in which the staff of the University departments is in close and intimate touch with the works, would seem to be the best places (in the absence of institutions devoted solely to industrial research) that can be found, for carrying out work on problems of local importance. It is the hope of many of those connected with many of our newer Universities that this work will develop and grow to the mutual benefit of the Universities and the manufacturers.

The war has already made a great change in the relations between the Universities and industrial firms. The university professors have not taken up, as a rule, the purely "academic" standpoint, and have recognised that, interesting as "academic" science may be to them and their students, it is the practical application of science that is of immediate importance to the bulk of the people, and which has an increasing value for industry. The manufacturer, in many cases, has come to recognise that the professor, when a practical problem is put before him, may be able to obtain a solution which will enable the manufacturer to increase the efficiency of his factory and diminish the risks involved in the manufacturing processes. This spirit of mutual co-operation is one which should be encouraged and stimulated in every possible way. It will lead to closer relations between the professors and students and those who are subsequently to employ them, and the professor, from his more extended practical knowledge of manufacturing conditions, will be able to recommend men who will be of real value at once to the manufacturer. It is to be hoped, also, that when industrial undertakings realise more fully the importance of t chnical knowledge, they will seek to attract a

larger number of the best men into scientific industrial work. The standing of the technical man in this country to-day is deplorably low, as compared with the position he holds in other countries, notably Germany and America. For this, the public schools and the older Universities are, in all probability, largely to blame. It used to be a tradition in the country families of England that the dullest boy should go into the Church; to-day engineering or some other branch of applied science is often looked on as the best occupation for a boy who seems unlikely to succeed in what are regarded as the more intellectual professions. The science side is the Mecca of the unintelligent at a public school, the classical side is still the bait for the best brains, and, save for a few brilliant exceptions, few of the best of the boys the public schools are turning out in large numbers have entered industrial work. This is greatly to be deplored. Mr. Fleming's pamphlet, however, does not deal with this point directly, though the necessity for its consideration in any scheme for the more effective prosecution of research work connected with industry is evident. There are many problems which can be dealt with successfully by men of average ability, who have had the necessary training in scientific method, but solutions will be obtained more rapidly and more effectively if men of outstanding ability and intelligence are called on to solve them. This is self-evident, and the manufacturers themselves can remedy the difficulty to a certain extent, by placing their technical staff on a more satisfactory footing than that they occupy at present. It is interesting to find that in large undertakings, such as the Eastman Kodak Company, who have established research laboratories of a most elaborate kind, including plant and machinery, to enable a laboratory process to be tried on an industrial scale, the total charge involved amounts to only 0·7 per cent. of the Company's profits, while in the General Electric Company, where work is even more elaborate and highly developed, the percentage charge is even smaller. There are, however, in this country, comparatively few firms who, at present, are in a position to establish laboratories of the kind referred to, and the scheme outlined, whereby associations of manufacturers act in combination to establish research institutions to carry out work which can be utilised by any of the firms concerned, is of great interest. There is a strong tendency in this country, at the present time, to advance in this direction. Associations of industries have already been established: one of the oldest of these is the British Electrical and Allied Manufacturers' Association; but the example has been followed by the formation of the British Engineers' Associations and by many others. The organisation of which Mr. Dudley Docker has been the moving spirit, and by which it is intended to establish a "Parliament of Industry" which will represent all the associations of the individual industries, is a further effort to try and eliminate the individualistic cut-throat competition which has previously existed between different firms engaged on similar manufactures. Such organisations should be in a very strong position as regards research work, and it is gratifying to find that nearly all of them have placed industrial research in the forefront of their programme.

On the other hand, many national institutions have already been founded in America to deal with it. The sub-division of responsibility between the State, including the national State-aided institutions like Universities, and the manufacturers' organisations, is a subject which still requires careful and detailed consideration. We have already advanced very far in our conception of the duty the State owes to industry; it is recognised that the State may, without injustice to the taxpayer, employ its resources to assist the development of manufactures which will ultimately benefit enormously the individual citizen. That all the work

done in national institutions should be published for the benefit of manufacturers in other countries than those in which the work has been done is recognised as impracticable, though it is equally obvious that to carry out such work solely for the benefit of an individual firm to the detriment of other manufacturers in the country engaged on the same work, is also open to question.

One of the most interesting institutions described in this survey is the now well-known Mellon Institute, established by the Mellon Brothers, and affiliated to the University of Pittsburg. This Institute is devoted exclusively to Industrial Research, and is intended to provide manufacturers, who may not be in a position to carry out specific researches in which they are interested in their own works, with facilities for carrying out such experiments in a well-equipped and well-staffed laboratory. Separate laboratories are provided in which research fellows work, these fellows being paid and the whole expense of the research borne by the manufacturers interested. A great deal of useful work has been done in the Institute under these conditions, and it is laid down in the agreement between the manufacturer and the Institute that the results of such work shall not be published until three years after the completion of the research; the publication may be delayed still further "if it can be shown by the manufacturer that such publication would be inimical to his interests." This Institute has been established with the intention of overcoming one of the greatest difficulties involved in the carrying out of Industrial Research in public institutions, and the greatest legal purist who may be inclined to object to the use of national institutions for the furtherance of the interests of individual manufacturers would find it difficult to cavil at this arrangement. The director of the institute provides the necessary persons for the conduct of the work (they are, in nearly every case, graduates with technical degrees who have shown their ability as research workers by obtaining such a degree as the Ph.D.), and there his responsibility ends. The whole cost of the work is borne by the firm concerned, and it is held, not unreasonably, under these conditions, that the benefit derived from the results of the research should be the property of the firm who has initiated and paid for the carrying through of the work. Some such arrangement will be essential in the future, if our national institutions are to take their share in the carrying on of Industrial Research.

The full programme suggested by Mr. Fleming is of great interest. He proposes as alternatives:

(*a*) Research laboratories in industrial works.
(*b*) Research laboratories for a group of works in the same industry.
(*c*) The centralisation of research in the Universities and Colleges.
(*d*) An imperial centralised laboratory for the whole industry.

Although these arrangements are suggested as alternatives, it seems likely that, in the end, nearly all the types of laboratory proposed will be utilised. It seems very much open to question whether much is to be gained by an attempt to centralise all work of this kind for all industries in a single institution. The needs of the various industries are so different, and the equipment required for the carrying out of the work so varied, that little would be saved by such an attempt. There is, of course, an immense amount of work which is of such a nature that it can best be done in such an institution as the National Physical Laboratory, greatly developed and extended. No one will question the necessity for an extension in the work of this laboratory. For a country with such vast industrial interests, and engaged in such varied classes of manufacture, the National Physical Laboratory is hopelessly starved and under-equipped; but the work that

has to be done is so enormous and the scope is so varied, that there is room for all the types of laboratory mentioned. The building up of any great scheme of industrial research is necessarily slow: the men to do the work require many years of careful training and practical experience before they are fully equipped for the tasks they have to perform. It is no work for heaven-sent geniuses, who can be picked out from the vocations they now follow, and suddenly plunged into their new task; they must be highly trained, and must have that instinct for discovery, for long and laborious experimenting, and for scientific reasoning that can only be gained by careful and patient preparation. It is therefore necessary, in view of the present position, that every available resource should be brought to bear, and used to forward a work which is of vital importance, if our industrial position is to be maintained in competition with other countries. The need is urgent, the demand is great, and every one who can help should be pressed into the service with the least possible delay, so that our efforts may not be too late. We have seen too much, in the past, of the fatal results of the policy of "wait and see"; let us, in this connection, think well and act quickly.

A House of Poetry

At a dinner given on November 5 at the Lyceum Club by Mrs. Eyre Macklin and her friends to the Poetry Society, Sir Ronald Ross, the President of the Society, outlined his scheme for the creation of a House of Poetry, which he said should consist of a worthy free library devoted to the world's poetry, with a room for public readings, offices, and staff, situated in the best part of London. It was required for two reasons, the better education of the British public, and to do honour to the great poets. He made no secret of his opinion that the intellectual level of the nation was below par, and referred to the saying of M. Taine, that whereas every French workman knew his Lamartine and De Musset, only a minute section of the British nation knew anything of their Tennyson and Swinburne. Although the Board of Education spent £15,000,000 a year on the education of our children, it spent nothing for the honour and study of the subject of which education largely consists, the poetry in which, from the Bible to the present day, the greatest thoughts of the greatest men have been enshrined. He maintained that, like science, poetry was neglected in this country. While we greatly honoured soldiers, governors, and even party politicians, we did nothing whatever to honour those much greater men, the poets and the scientists. General Smuts had said that the British Empire was held together merely by common consent: one of the principal factors was our common and great literature, and the coherence of the Empire of to-day probably depended more upon Shakespeare than upon any other man. At the close of the dinner the whole of the large company enthusiastically drank the health of the proposed House of Poetry.

Poland and Poetry

The influence and power of literature in general and poetry in particular over the destinies of nations is most noticeable in the history of Poland. It was mainly on this theme that Mr. Ladislas Czapski dilated in his two lectures on "Modern Polish Literature and the National Movement and Social Reform in Poland," delivered at King's College in November. He brought to the notice of his audience the remarkable fact that Poland, though so long ruled by three different powers each seeking to absorb her, is just as united in racial sentiment and ambitions to-day as she was before the partition. The history of Poland for the last hundred years or more has been nothing but a series of crushing defeats

sufficient to reduce the nation to despair, and yet the hope of autonomy has increased rather than decreased in the hearts of the Poles. This result Mr. Czapski ascribes to the vitalising power of Polish poetry. The tyranny exercised over the Poles was so severe that these poems had to be read aloud to small companies of men who met in secret, with one of their number on guard to give the alarm when the police approached. The copies of the poems were destroyed after the recitation, but the words remained engraved on the memories of the audience. The lecturer gave a sketch of the life and chief work of one of their greatest poets—Adam Mickievicz—whose central idea was that the only way possible of raising Poland to her former glory lay in individual regeneration. Long passages of this poem were read by Mr. Czapski, which were inspiring, though only in English translation. It was chiefly this poem, he said, that nerved the people to self-sacrifice and kept alive the fire of patriotism, and, when a subsequent wave of materialism threatened to extinguish it, it was again fanned into flame by Stansilau Wispianski.

Notes and News (D. O. W.)

On the first appearance of these notes a word of explanation may not perhaps be inopportune. They have been written to give expression to the Editor's desire that in future SCIENCE PROGRESS should contain a record of the more important scientific events, and such account of the news of the scientific world as is likely to be of interest to its readers. A quarterly journal can hardly expect to obtain priority in its announcements, and the events recorded must be of necessity, to a certain extent, ancient history. It is hoped, however, that their insertion here may afford a more permanent record for reference purposes than the pages of the daily and weekly press.

The first list of appointments to the newly established Order of the British Empire includes the names of the following scientific men: Knights Commanders (K.B.E.): Mr. Dugald Clerk, Prof. H. S. Jackson, and Mr. R. Threfall. This class carries with it the privilege of knighthood. Commanders (C.B.E.): Prof. H. B. Baker, Mr. L. Bairstow, Prof. W. L. Bragg, Prof. S. J. Chapman, Mr. W. Duddell, Mr F. W. Harbord, Prof. F. W. Keeble and Prof. J. F. Thorpe. Officers (O.B.E.): Prof. J. C. McLennan. Dr. Garrett Anderson and Dr. Mary Scharlieb also receive the C.B.E.

Dr. John Cadman, C.M.G, Professor of Mining at the University of Birmingham, has received the Legion of Honour from President Poincaré in recognition of services rendered by him during the war.

The following awards have been made by the President and Council of the Royal Society: Royal Medals to Dr. John Aitken, F.R.S. (for his researches on cloudy condensation), and Dr. Arthur Smith Woodward, F.R.S. (for his work on vertebrate palæontology). The Copley Medal to H Emile Roux, For.Mem R.S., for his services to bacteriology and as a pioneer in serum therapy. The Davy Medal to M. Albin Haller for his researches in the domain of organic chemistry. The Buchanan Medal to Sir Almroth Wright, F.R.S., for his contributions to preventive medicine. The Hughes Medal to Prof. C. G. Barkla, F.R.S., for his researches in connection with X-ray radiation.

Prof. W. M. Bayliss has been awarded the Baly Medal of the Royal College of Physicians and Surgeons for his physiological work; and Sir Arthur Newsholme the Bisset-Hawkins Medal in recognition of his work in sanitary science.

Among the names of the scientific men who have passed away during the last few months are the following: Prof. J. H. Barnes, Agricultural Chemist to the

Government of India; Prof. C. E. Bertrand, of Lille University, a distinguished plant anatomist and palæobotanist; Dr. Félix de Dantec, the well-known French biologist; Prof. A. J. F. Dastre, the Director of the Laboratory of Animal Physiology at the Sorbonne; W. du Bois Duddell, the famous electrician, whose name appears in the list of C.B.E.'s above; Prof. E. Hull, Professor of Geology at the Royal College of Science, Dublin; Dr. Theodor Kocher, Professor of Surgery at the University of Berne; E. Sarasin, the physicist, well known for his work on light and on electromagnetic waves.

We note also the deaths of Adolf von Baeyer, the chemist who made synthetic indigo; Eduard Buchner, who gained the Nobel Prize for Chemistry in 1907 (died from wounds received while acting as Major in the German Army); and Robert Helmert, well known for his work on geodesy.

Science (October 5) gives some interesting details of the scientific organisation of the United States Army. The aerological observation work, aeronautical instruments, sound-ranging and meteorology all fall within the purview of the Science and Research Division of the U.S. Signal Corps and are directed by the National Research Council of which Major R. A. Millikan is the executive officer. The aerological observation section employs about a hundred physicists and engineers under the direction of Dr. (Major) W. H. Blair of the U S. Weather Bureau. Major E. H Bowie (also of the Weather Bureau) is responsible for the weather forecasting for the expeditionary force. The aeronautical instrument research will be controlled by Prof. (Major) Charles E. Mendenhall of Wisconsin. Sound-ranging will be in the charge of Dr. Trowbridge and Prof. Lyman. Prof. R. W. Wood was first offered the tentative rank of Major in the French Army, and has been engaged in research in France in co-operation with the Paris Academy of Science. He is continuing this work with the rank of Major in the Signal Corps.

It is reported that Roald Amundsen will start on another Arctic expedition in March or April next. A new and very completely equipped ship has been built, and the explorer intends to take an aeroplane with him for reconnoitring in the Arctic regions.

The British Association meeting, which should have been held at Bournemouth last September, was abandoned, partly on account of the difficulty of travelling and also for other sufficiently obvious reasons. The other activities of the Society are, of course, in full swing. In particular, an effort is being made to promote the study of geographical subjects (*i.e.* geodesy, terrestrial magnetism, tides, atmospheric electricity, and seismology) by arranging meetings at which investigators, and others who take an interest in the subject, may read and discuss papers and reports, and in other ways contribute to the common advancement of our knowledge of the earth. Two meetings were to be held in November and December last, and three or more in the first six or eight months of 1918. The members of the Committee are: Sir F. W. Dyson (Chairman), Dr. C. Chree, Col. C. F. Close, Prof. E. B. Elliott, Mr. J. H. Jeans, Prof. A. E. H. Love, Major H. G. Lyons, Prof. A. Schuster, Sir Napier Shaw, Prof. H. H. Turner, Dr. G. W. Walker, and Dr. S. Chapman (Secretary). Communications should be addressed to the Secretary at the Royal Observatory, Greenwich, S.E.10.

During the last few months two new chemical societies of considerable importance have been initiated. The Australian Chemical Institute has its headquarters at Sydney, and is intended to work on the same lines as the Institute of Chemistry of Great Britain and Ireland, one of its specified duties being, however, to endeavour to improve the rate of remuneration of chemists. The British Association of Chemists has also been formed to forward the interests of the chemist,

Its first general meeting was held at the Manchester School of Technology on November 10, about 500 chemists being present. The Provisional Committee obtained the support of the meeting for the main objects, but is to endeavour to persuade the Council of the Institute of Chemists to adopt their programme before proceeding to definite incorporation.

In Sweden a Chemical Industries' Bureau has been formed for the purpose of bringing together Swedish chemical industrial interests.

The Ramsay Memorial Fund, which was inaugurated at a meeting held at University College, London, in October 1916, has made substantial progress towards the raising of a sum of £100,000, which is the object of the appeal. The actual sum contributed up to date in promises or donations is £21,798 18*s.* 6*d.* Steps have already been taken to form Co-operative Committees in Glasgow, in other parts of the British Empire, and in certain foreign countries. For this purpose the following have accepted the position of representative and corresponding member of the Ramsay Memorial Committee in their respective countries: Prof. Charles Baskerville in the United States of America, Prof. D. Orme Masson in Australia, Prof. J. H. K. Inglis in New Zealand, Señor Augusto Villanueva in Chile, Prof. Philippe A. Guye in Switzerland, Prof. H. Kamerlingh Onnes in Holland, Prof. J. N. Bronsted in Denmark. In Glasgow a strong Committee is being formed under the chairmanship of the Lord Provost. The fund will be devoted to the two following objects:

1. The provision of Ramsay Research Fellowships, tenable wherever the necessary equipment may be found; and
2. The establishment of a Ramsay Memorial Laboratory of Engineering Chemistry in connection with University College, London.

Those who desire to do so can earmark their gifts for either of these objects. Donations can be addressed to the Joint Honorary Treasurers, Lord Glenconner and Prof. Norman J. Collie, at University College, London, W.C.1.

Science reports that Dr. T. Brailsford Robertson, Professor of Biochemistry and Pharmacology in the University of California, has donated to the University, for the endowment of medical research, all his rights in the growth-controlling substance "Tethelin," which he has isolated from the anterior lobe of the pituitary body. Tests of this substance in military hospitals in Europe have confirmed its remarkable property of causing wounds to heal promptly which have for months, and even years, refused to yield to treatment.

In the August number of the *New East* Dr. Futaki, of Tokyo, gives an account of his discovery of the virus of typhus fever, which he calls *Spirochæte exanthematotyphi.* The spirochætes were first located in the kidneys of patients who had died from typhus and have since been found in the blood, urine, etc. They resemble the *Spirochæta pallida* in their form, and show somewhat vigorous and characteristic movement under dark field illumination. Their length varies from 6 to 8 microns and they have short cilia at both ends.

Mr. J. E. Wodsedalek describes some remarkable experiments he has made on the starvation of various kinds of larvæ (*Science*, October 12, 1917). The first experiments were made on the larva of the *Trogoderma tarsale* (a small beetle). Newly hatched larvæ lived four months without ever having eaten at all, and this period increased with the size of the larvæ at the commencement of the experiment, full-grown larvæ living as long as five years without food. During this time they shrank to about $\frac{1}{20}$ of their maximum larval mass! If the starved specimen is fed it starts growing again, dwindling when restarved, and so on.

In a letter to *Nature* (October 25, 1917) Prof. Strutt mentions an interesting

experiment he has made concerning the constitution of the terrestrial atmosphere. Photographed from the ground, or even from a mountain-top, the solar spectrum does not extend beyond $\lambda 2,900$, and the absorption of the shorter wave-lengths has been shown to be almost certainly due to ozone. Prof. Strutt has now photographed the spectrum of a mercury vapour-lamp 4 miles distant, and finds that it extends at least as far as $\lambda 2,536$, a line near the maximum intensity of the ozone absorption band, so that in this case ozone cannot be responsible for the shortening of the spectrum. It is therefore necessary to conclude that ozone is much more abundant at higher levels than at the earth's surface, a result which is in agreement with certain chemical determinations of atmospheric ozone.

In the *Proceedings of the U.S. National Museum, Washington* (pp. 553-63, 1917), Messrs. T. L. Watson and R. E. Beard give an account of some experiments they have made on the chemical composition and physical structure of variously coloured varieties of quartz. One interesting case discussed is that of the amethyst. They find that it contains more manganese than any other variety of quartz they examined, and conclude that the colour is mainly due to colloidal particles of manganese oxide of ultra microscopic dimensions. The colour of rose quartz when destroyed by heat cannot be restored by daylight or exposure to radium, and is probably not due to any inorganic substance.

Some interesting details of the Haber process used in Germany for the synthetic production of ammonia are given by Dr. C. L. Parsons in a report to the U.S. Government on the processes of nitrogen fixation (*Jour. Soc. Chem. Ind.* October 31, 1917). This process is worked on a huge scale by the Badische Company, the 1917 output being equivalent to over 500,000 tons of ammonium sulphate, as against 20,000 in 1913. The operations required, however, involve some danger and much technical difficulty, since the nitrogen and hydrogen are combined at a temperature above 500° C. and at pressures from 125-150 atmospheres. Indeed, the process involves so high a degree of training that it is reported that if the present technical staff were lost many months would be required to train another. The process, which is very cheap, has not yet been worked successfully outside Germany in spite of the urgent necessity to find some source of nitrates other than the Chile deposits. The General Chemical Co. has, however, achieved success in causing the combination of hydrogen and nitrogen at lower pressures than those demanded by Haber's process. Their process has been offered to the U.S. Government. It is probable that in the future both processes will find a strong competitor in the cyanide process which, while presenting no special difficulties, has not yet been worked out to the point where factory construction can begin.

The report of the Committee appointed to consider the scheme of examination for Class I of the Civil Service Examination deserves serious consideration. It is proposed that, in future, the examination should be divided into two sections, the first, a compulsory one, having a maximum of 800 marks, and the second, containing the optional subjects, a maximum of 2,000, with an extra 100 for an additional modern language. The first section includes a paper on contemporary, social, economic, and political questions; another on the general principles of science; and a third on translation from a modern language. These, together with two papers on English, each gain 100 marks. There is, however, a *viva voce* examination on matters of general interest which receives no fewer than 300 marks. This is a very large proportion of the whole, and while permitting the examiners to test the candidates' personality, throws upon them rather a heavy responsibility. The second section is noteworthy as giving equal weight to classics, mathematics,

and science, and also for including a wider choice of optional subjects, *e.g.* Engineering, Agriculture, and Anthropology. It is further proposed that practical examinations in science subjects should be abolished, each candidate being required in lieu thereof to furnish proof that he has had laboratory training in an institution of university rank. The adoption of this proposal would obviously be calculated to encourage the candidate to reduce his practical training to the utmost limit and, instead, cram for the theoretical papers. It would probably be better to require the examiners to modify their practical examinations so as to reduce the too prevalent element of chance.

The Report of the Executive Committee of the Commonwealth Advisory Council of Science and Industry states that the annual number of students graduating in the pure and applied sciences in Australian universities is only about 110, of which most are absorbed by the professions, and only a very few receive training in research—a number insufficient to supply "even the present needs." The Committee consider it most important that steps should be taken to increase this number. They add, "no doubt the demand will to some extent create a supply . . . but it is also true that the supply will create a demand." They evidently anticipate that future conditions in Australia will differ very much from what they did in pre-war days in England!

The same report contains some interesting data concerning the prickly pear. This weed pest has now overrun some 20,000,000 acres in Queensland and 2,750,000 in New South Wales; moreover, it is spreading in Queensland at the rate of 1,000,000 acres a year. On agricultural land it can be poisoned by spraying with arsenic acid or by gassing with arsenic trichloride. This process is, however, too expensive for use on grazing land, and attempts are being made to find some insect which feeds exclusively on it or a fungus which causes disease in it. A species of cochineal insect attacks one variety of the pear (*Opuntia monacantha*), and has been introduced with excellent effects at Bowen and Charters Towers; but the other varieties still flourish unchecked.

The first report of the Conjoint Board of Scientific Societies, created mainly to promote the co-operation of those interested in pure and applied science and assist in the application of science to industry, shows that most of the ten Sub-committees which have been appointed have already made considerable progress. The Sub-committee on Agriculture is, among other things, considering the development of electrical applications to agriculture in this country. Electricity is used very largely in Germany, not only for stationary motor purposes, but also for ploughing, cultivating and hoeing. There is also the possibility of using it for intensive cultivation by discharging "high-pressure" electricity over the area. The Watching Sub-committee on Education has come to much the same conclusion as the B.A. Committee dealt with below. The Metric System Sub-committee, which is considering the question of Weights, Measures and Coinage, has not yet sent in its report.

In connection with this last matter a joint meeting of the representatives of the Institute of Bankers, the Association of Chambers of Commerce and the Decimal Association, agreed unanimously on a plan for decimalising the coinage with the present pound sterling as the unit. The scheme was approved by the Council of the Association of Chambers of Commerce on November 7, and they have decided to press the need for this reform through the chambers of commerce in all parts of the United Kingdom.

The second Report of the Committee of the Privy Council for Scientific and Industrial Research contains a large amount of important information. The Com-

mittee has at its disposal a Trust Fund of £1,000,000 to be expended on research during the next five or six years, and an annual grant from Parliament which for the financial year 1916-17 amounted to £40,000. It is proposed to use the Trust for industrial research, which will be undertaken by Trade Research Associations registered under the Companies' Act. Associations for the benefit of the cotton trade, woollen and worsted manufacturers, photographic manufacturers, etc., are in course of formation. The Committee has taken over the National Physical Laboratory, and will, apparently, finance it from this fund. The annual vote will cover (*a*) the cost of those researches which will not be undertaken by the proposed Research Associations; (*b*) the grants to individual research workers, both students and others; and (*c*) the cost of administration. The general principles which will govern the grants to research workers are as follows: (i) No aid should be recommended for persons who are required for military service, or who, though unfit for military service, are required for munition work. (ii) No aid should be recommended for the purpose of relieving educational institutions of any part of the salaries of the members of their staffs. (iii) No aid should be recommended which would in effect strengthen the teaching staff of an institution. (iv) Money should not be given to increase the yearly value of a research scholarship or studentship, though, in special circumstances, a personal grant may be given to a scholar or student otherwise unable to take up the award. (v) Allowances for training students in the methods of research should not be made to persons of alien nationality. The report gives details of the progress of the investigations started in the period 1915-16, and mentions a number of others commenced since the publication of the last report. Among these are experiments intended to remove difficulties experienced by English manufacturers of X-ray bulbs (directed by Dr. Willows); experiments on superheaters (at the Royal Technical College, Glasgow); on cellulose (Manchester School of Technology); and on the acoustics of the pianoforte (Northern Polytechnic, Holloway). The Committee states that "the encouragement of the discoverer and inventor is a matter with which we have great sympathy," and are prepared to assist in the development of definite inventions which meet with their approval as likely to be of real use.

The Fuel Research Board of the new Department of Scientific and Industrial Research has issued its first published report. The chief question under consideration, at present, is this: "To what extent can and ought the present use of raw coal to be replaced by the use of one or other of the various forms of fuel manufactured from coal—coal, briquettes, tar, oil, or gas?" While the process of the high temperature (900-1,200° C.) carbonisation of coal is highly developed, the question is still open as to whether, by low-temperature carbonisation, products can be obtained of a collective value greater than the cost of the original coal and of manufacture. Outlets for all the products of carbonisation have to be found to justify the process economically and that without poaching on the preserves of existing industries. The Admiralty needs the oil that could be obtained, but to produce 1,000,000 tons of oil would require 20,000,000 tons of coal, and leave 15,000,000 tons of coke which would have to be disposed of at a profitable price. The Committee hopes to be able to deal also with the preparation of fuels from oil shales, brown coals, and peat, and is carrying out a survey and classification of coal seams in the various mining districts by physical and chemical tests in the laboratory.

The Report of the Committee appointed by the British Association to consider the question of Science Teaching in Secondary Schools consists roughly of two parts. First, a discussion of matters of general interest, and secondly a fully

detailed set of typical science courses for schools of different types. The Committee strongly deprecates the present-day tendency to introduce science teaching for two hours only each week: it is their opinion that, for the training obtained to have any practical value, an average of one-sixth (for boys) and one-seventh (for girls) of the total time available should be allotted to this purpose. (A very modest proportion compared with the 40 per cent. claimed by Latin and Greek.) It is found that, since the last report was completed in 1908, Mechanics is less frequently included in the science curriculum, while Biology is receiving more attention than before, as it undoubtedly should do; though a student intending to specialise in science would do well to master the elements of mechanics before starting systematic biology. The Committee also reports that there is evidence that the present syllabus of the Class I Civil Service examinations discourages science in the Public Schools. The new scheme dealt with above may possibly improve this if it ever materializes. An appendix gives tabulated statements on the salaries of teachers in aided and maintained secondary schools in England and Wales. It is an old story, but it needs constant repetition. The average salary is £175; the maximum in 54 per cent. of the schools is less than £200, and in one case only reaches £300. The prospects for a boy leaving a secondary school are infinitely worse in the teaching profession than in any other—*e.g.* Civil Service or Banking. He is worse paid throughout his career, harder worked, has no security of tenure, and, as a rule, no pension. These things are a scandal which cry to heaven—and they will probably continue to do so until the teaching profession adopts the only remedy which seems to meet with a successful response to-day. It should be said, however, that the Board of Education has set up a Departmental Committee to consider the *principles* which should govern the salaries of science teachers. This does not sound very promising unless it be accepted that the fundamental principle is the right to the removal of the disabilities mentioned above.

The question of the inclusion of general science in the preparatory and public school curriculum is discussed in a quiet but trenchant fashion by Major V. Seymour Bryant, M A., in his monograph entitled *The Public School System*, published by Longmans for the Committee on the Neglect of Science. Major Bryant points out that over 80 per cent. of the masters on the Head Masters' Conference are classical scholars, and that they dominate 82 per cent. of the boys attending the public schools. Only 10 per cent. have any science qualification at all, and these control only 7·5 per cent. of the boys. Unbiased modern ideas can hardly be expected from such an assemblage, and, in fact, we find that the percentage of time allotted to the teaching of various subjects in preparatory schools by a committee of this conference is as follows:

	Dead Languages.	Modern Languages.	English.	Mathematics.	Scripture and History	Geography.	Science and Nature Study.
Before taking up Greek	29·7	11·9	19·8	17·8	11·9	8·9	0
Afterwards . . .	40·6	11 9	11·9	17·8	11·9	5·9	0

No science and 40 per cent. of the time devoted to memorizing the grammar and vocabulary of the dead languages—a stultifying drudgery indeed! The author considers that specialisation should not begin until the age of 16 or 16½; but that up to that time general science should be an essential part of the curriculum, and he gives brief suggestions for courses suitable for preparatory

schools and the lower forms of public schools. It is stated by some of those who oppose these innovations—*e.g.* Sir Sydney Lee—that "the exaltation of the technological branch of school and college studies in Germany, to the exclusion or neglect of the humane branches, had encouraged the systematized worship of brute force, with its inevitable issue, in a brutal defiance of the usages of humanity." Major Bryant quotes the obvious answer to this charge. The *gymnasien* course in Germany, at which the whole of the nation, except those trained in the elementary schools, are educated are even more classical than our own public school courses. Latin and Greek receive 43 per cent. of the time, science 7 per cent.! If science is to be banned because of its misuse in the hands of the Hun, would it be so utterly impossible to make out a case against those very "humane branches" theology and literature?

* * * * *

Since the above notes were written the detailed scheme for the decimal coinage system already referred to has been received from the Decimal Association, and also a detailed criticism written by Sir Ray Lankester, issued by the Committee on the Neglect of Science. With regard to the former, the proposed unit is the mil, the thousandth part of the £ sterling, whose value as a monetary standard remains unaltered. The bronze coinage would consist of 1, 2, 3, and 4-mil pieces (this last equal to ·96 penny); 5 and 10-mil pieces would be coined in nickel, and the remaining coins would remain as before, except that the half-crown would disappear, a double florin (equal to 200 mil) taking its place if necessary. From a commercial point of view the scheme has much to recommend it. The new bronze coins have nearly the same value as the old, and the present 3-column method of cash entry could be continued, the headings being changed to £.f.m., *e.g.* £1 18 11½ would be replaced by £1. 948 or £1. 9. 48; four figures only being required instead of six. The advocates of the scheme claim that those who are compelled to buy food and other necessaries in small quantities would benefit by the finer grading of the small coins. Thus, while the *cost* of a halfpenny or penny article may have increased only by 20 per cent., the *price* has to be raised to a penny or three halfpence because of the lack of intermediate coins; the farthing being used but seldom. On the other hand there would be undoubtedly a tendency for the penny to be replaced by the 5-mil piece instead of the 4, with the result that the value of the shilling would depreciate still more than it already has done. Of course, compensating adjustments could easily be made; but our unhappy experiences of commercial methods to-day make that possibility very remote.

The concluding paragraph of Sir Ray Lankester's report seems to summarise very pertinently the view of the position put forward by the Committee on the Neglect of Science:

> The object of the Committee on the Neglect of Science was (and still remains) to obtain from the Civil Service Commissioners such a scheme of examination as would *compel* the managers of the great public schools to adopt a more intelligent scheme of education. The Commissioners are the only body in existence which can put pressure on the great schools and universities. Very naturally—and perhaps correctly, but not courageously nor patriotically—Mr. Stanley Leathes's Committee refuses this responsibility. "Permissive legislation" is the limit of their courage. So it has been in former times in regard to measures for securing the public health, elementary education, and even national defence. But eventually legislation in these matters has taken a compulsory form; and so it must, without further delay, in regard to education. It is simply absurd to allow the great schools and the old universities to administer great national funds so as to maintain, decade after decade, century after century, the vested interests of a schoolmaster-class, ignorant of, and therefore hostile to, the most important national interests—the education of our best sons in the knowledge of nature. Mr. Stanley Leathes's Committee, instead of rescuing education from the professional vested interests of the classical schoolmasters, hands back the victim, after many professions of goodwill, to the tender mercies of those who are banded together to starve, torture, and discredit her, and remorselessly to maintain the domination and the pecuniary allurements of the "classical system."

ESSAYS

THE ELECTRICAL BASIS OF COHESION (Herbert Chatley, D.Sc. Lond., Research Engineer to the Whangpoo Conservancy Board, Shanghai).

It cannot but have been observed that there is a considerable hiatus in scientific knowledge of the properties of matter between the behaviour of the atom and the mechanics of visible masses. While microscopic investigation of crystalline structure and of the Brownian movement in liquids has somewhat narrowed this field of ignorance, it is not too much to say that the actual nature of molecular force as distinct from the atomic force which produces chemical change remains to a great extent a mystery, particularly in the case of the solid state, where its influence is preponderant.

Engineers have acquired an immense amount of information as to the "strength of materials," and based thereon is an elaborate mathematical structure termed the "theory of elasticity," but the relation of the cohesive forces, which manifest as "strength" and "elasticity," to electrostatic, magnetic, or gravitational forces is unsolved.

Kelvin (*Popular Lectures*, vol. i., and *Proc. Roy. Soc. Edin.*, April 21, 1862) opined that very great density of the molecules would explain cohesive attraction in accordance with the Newtonian law for gravitation, but it is not difficult to show that this hypothesis is incorrect. Recent work shows that the mass of an iron molecule Fe_2 is about $1 \cdot 86 \times 10^{-22}$ grams, and the distance from centre to centre of a pair of molecules in a solid is about $3 \cdot 0 \times 10^{-8}$ cm. Applying the Newtonian rule to these quantities we find that the gravitational bond is about $2 \cdot 5 \times 10^{-36}$ dynes. Quantities of a similar order are obtained with other molecular pairs, since the molecular interval increases with (but not so fast as) the molecular weight.

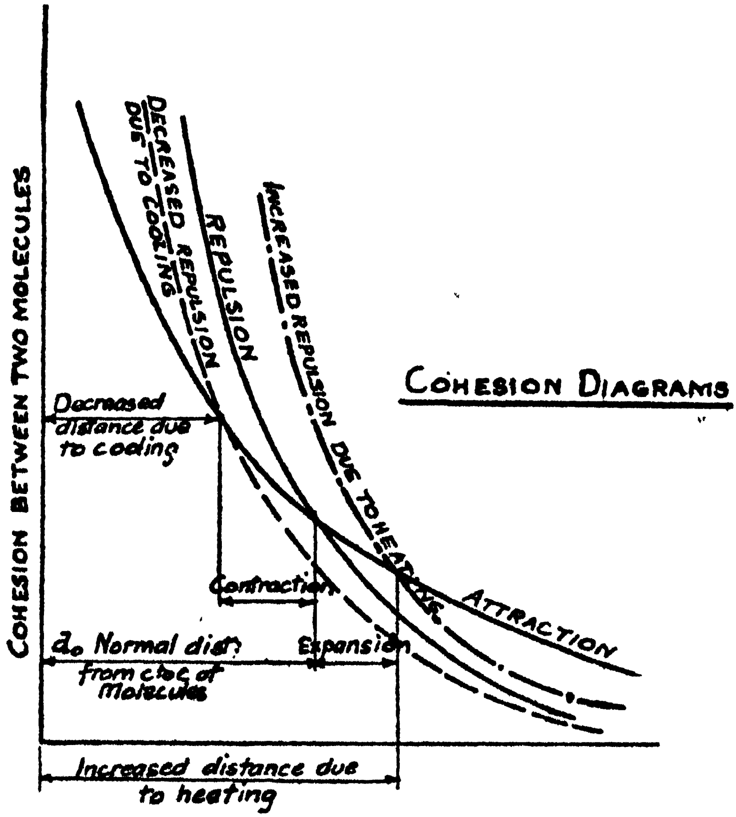
It is of course irrelevant to point out that even if this hypothesis were true no further light would be shed on the nature of gravitation itself, but there would only remain one generality to compare it with, viz. electrostatic force. If Sutherland's hypothesis as to the force of gravitation being a differential effect of positive and negative tractations and pellations is accepted, nothing further would be required to give a complete theory of tractation on an electrical basis.

Nernst and his collaborators, on the basis of crystal architecture, consider that cohesion is identical with chemical affinity. Against this it must of course be objected that cohesion appears to be always active at minute distances even for neutral molecules, is not necessarily vectorised, and seems to be weaker than the feeblest chemical linkage.

The tractation between two dissimilar unit charges at a distance of $3 \cdot 0 \times 10^{-8}$ cm. is $2 \cdot 5 \times 10^{-4}$ dynes.

(It should here be remarked that the absolute ratio of electrostatic tractation to gravitational tractation for equal masses at equal distances is 10^{42}.)

It is at present almost impossible to deduce how far the cohesive attraction between two isolated molecules differs from that between a similar pair forming part of an appreciable volume, but since the effective range within which it can preponderate over gravity or other external force is small, it seems improbable that the gross tractation between a contacting pair in a mass can be much greater than ten times that between an isolated pair. For the purpose of this inquiry no



distinction has been drawn, although it will doubtless be ultimately desirable to do so.

Tensile strengths of 20 kg. per sq. cm., or say 2×10^7 dynes per sq. cm., are common (*e.g.* cement), and since this represents the net value after deducting the pellation (of which more later) and corresponds to a decreased intensity due to separation, it is a conservative figure. Assuming unitary linkage between molecular pairs separated by the imaginary section plane and reckoning only 10^{15} pairs, this corresponds to a tractation of 2×10^{-8} dynes.

Maximum tensile strength is in the neighbourhood of 10,000 kg. per sq. cm. (hard steel), or say 10^{10} dynes. Assuming again 10^{15} molecular pairs (which is probably too few) we get a tractation of 10^{-5} dynes.

Hence it appears that cohesive tractation enormously exceeds gravitational force but does not quite reach electrostatic (chemical) values.

Before proceeding further it must be clearly pointed out that in the solid state the cohesive force which provides tensile strength to the material is only the excess of tractation over pellation due to the enlargement of the molecular field. Similarly compressive resistance is the excess of pellation over tractation when the molecular field is contracted. In the condition of no stress, tractation balances pellation, and at first sight it might seem that the two forces might have any value whatever so long as their differences satisfied the space conditions. Experiments on tensile stress show, however, that it reaches a maximum when the separation of the molecules is from 0 1 to 0 5 times the original interval, and analogy suggests that in this condition of ultimate stress the pellation is almost zero, so that in the position of equilibrium it is improbable that the gross tractation (which is there equal to the pellation) can be very much greater than the maximum tensile stress in the position of rupture. In a paper on the "Cohesion of Solids" contributed to the Physical Society in 1915, this point is discussed by the present writer, and it is shown (*Proceedings*, vol. xxvii. Part V., p. 450) that a stress-strain diagram is the algebraic sum of tractation-strain and pellation-strain diagrams, and that the space rate of decrease of the pellation exceeds that of the tractation. The fact that repulsion is associated in some way with contact (probably of enormously energetic fields of corpuscles) certainly suggests that its value will decrease very rapidly as the propinquity decreases.

In the case of the liquid state appreciable tensile stress only occurs when the liquid is free of exceptionally active molecules (*i.e* gas), and for general purposes it may be assumed that the liquid condition chiefly depends on a very slight excess of pellation over attraction which is balanced at the surface by gas pressure. At the surface the lack of upward tractation (due to the wider spacing of the gas molecules) enables the downward tractation to there exceed the pellation, so producing surface tension effects.

In gases the tractation is only temporarily potent between colliding molecules, and is almost wholly neutralised by the impulsive forces arising from the reaction momentum of collision.

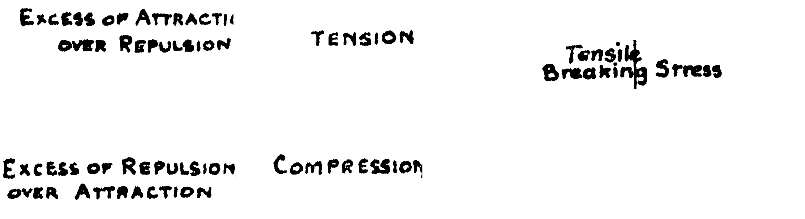
Reverting to the question of the solid state, the very minute range within which tractation so exceeds pellation that the net cohesion *increases* with separation (a condition necessary for the stability of the solid) is shown by the fact that very great pressure is required to make solid substances cohere, and also by the fact that rupture occurs in all true solids with a linear expansion of less than 50 per cent., indicating that the maximum stress occurs at or below that degree of separation.

The case of liquefaction of solids is a little different, since it seems clear that the pellation is chiefly if not wholly due to the kinetic energy of the molecules, which, being increased by temperature, rises at the melting temperature to a value at which it exceeds the tractation. This may be shown graphically by raising the pellation curve vertically so that it ceases to give intercepts on the tractation curve, and gives a stress diagram entirely corresponding to expansion which can only be neutralised by an externally applied pressure.

The space rate of change of stress in solids (which is termed the "modulus of elasticity" so long as it remains sensibly constant) is the difference between the space rates of change of tractation and pellation and becomes zero at the point of maximum tensile stress, and is indefinitely large for pure compression (*i.e.* without shear or lateral bursting tension).

Lateral stresses are due to the change of position of the molecules. Thus in the case of tension the longitudinal pairs are separated, thus diminishing the tractation at intermediate points. The pellation of laterally disposed molecules displaces them towards the field of decreased pressure so that there is a general lateral contraction. In the case of axial compression there is increased pellation between the longitudinal pairs, and the lateral molecules are displaced into new positions of equilibrium, thus giving a general sideward expansion. Poisson's ratio indicates the manner in which the lateral forces are modified.

In the absence of accurate knowledge as to the mechanical constitution of molecules and atoms it is difficult to suggest a law for the pellation. Certainly it depends on the temperature, and it seems probable that the kinetic energy of the molecules is related to the gas constant, subject to certain deduction for the potential involved in the change of tractation. The only step at present possible seems to be to assume a law for the tractation from electrical and gravitational analogies, and then deduce the repulsion from stress-strain effects by difference.



Stress Strain Diagram
(Sum of Cohesion Diagrams)

We have seen that tensile stress lies near in value to electrostatic force, but the fact that the latter explains atomic linkage, whereas cohesion is only slightly discriminatory, suggests that we cannot directly identify them. Abegg and Veles hypothesis as to secondary valency does help to render consistent the formation of homogeneous molecules, but something more is needed to explain the adhesion of molecule to molecule, and the writer would add to it the idea that the electrical fields which correspond to atomic linkage are not wholly neutralised. A "stray field" can be conceived to exist which is capable of tractating other similar fields with an intensity of say one-hundredth that of a nascent atom and shows as gravitation for distances of more than a few molecular diameters.

On this basis the writer has produced the following empirical formula, which satisfies approximately the conditions required for cohesive tractation at smal distances and gravitational force at large distances :

$$t_s = \frac{Gm^2}{d^2 + (4d_0 \cdot d^{-1})}$$

where t_s is the tractation between two isolated molecules,

G „ „ Newtonian constant for gravitation,

m mass of a molecule,

d „ „ molecular interval (centre to centre)

d_0 „ „ „ „ „ at absolute zero.

At absolute zero, d equals d_0, the exponent becomes 6, and $t_2 = Gs^2$, where s is the density of the substance at absolute zero.

When d/d_0 is large, we get the ordinary Newtonian formula.

For a pair of iron molecules, the tractation according to this formula falls from about 10^{-5} dynes when in contact (compare value given above for hard steel) to 10^{-8} when there is 10 per cent. separation.

The ratio of this tractation to simple Newtonian falls from $1{\cdot}7 \times 10^{30}$ for contact conditions down to unity for a separation of a millimetre.

As soon as sufficient investigation shall have shown that this is generally consistent with the properties of matter (if it does so) the magnitude of the pellation can be studied. It appears to require that the pellation shall follow some such rule as

$$t_1 = Kd^{-n}$$

where n is a very high number (about 70 in some cases that have been examined). K is necessarily very small.

Assuming that the pellation is wholly a kinetic energy effect, some difficulties still arise. Low-temperature experiments indicate great increase of cohesive strength with decrease of temperature, and there seems no reason to suppose that even at absolute zero compressive strength has ceased. We may, however, suppose that the tractation increases, as would be indicated by the above formula, and that resistance to compression at absolute zero arises from the kinetic energy of the electron fields as distinguished from that of the atoms and molecules themselves. A problem also occurs in connection with the storage of strain energy in compression. Compressive strain reduces the tractation potential but increases the pellation energy. A temporary increase is shown by a measurable rise of temperature, but this is only a small part of the strain energy if the load is applied slowly. The amplitudes of molecular oscillation having decreased, we must either suppose that the molecular frequency remains higher (even after radiation has restored the temperature balance) or that the electron field becomes more energetic.

If, as has been supposed, cohesion is a residual effect of electrostatic tractation, it would be natural to expect that some more definite relation between cohesion and electrical phenomena should be apparent. The writer suggests that such relations do exist, and while he does not press this point too strongly, seeing that it is frequently difficult to know whether there are or are not free ions or electrons participating, he cannot but feel there is a considerable amount of evidence in favour of such an hypothesis.

In the first place, the production of static charge by friction indicates that the disturbance or rupture of cohesion bonds liberates electrons. Secondly there are several direct relations between electricity and cohesion. According to Silvanus Thompson:

"Metal conductors, when subjected to the prolonged action of currents, undergo slow molecular changes. Wires of copper and brass gradually become brittle under its influence. During the passage of the current through metallic wires their cohesion is temporarily lessened, and there also appears to be a decrease in their coefficient of elasticity."—*Electricity and Magnetism*, 249.

Again it has been observed by Beccaria that an electrified liquid evaporates more quickly than one not electrified, and the capillary electrometer shows how considerably surface tension is modified by difference of potential.

In all these cases one must of course take into account the stresses due to the charges themselves and the strain effects due to the temperature change accompanying the passage of a current, but it seems very doubtful if these are adequate to explain the results.

Not the least powerful argument in favour of an electrical basis for cohesion is the principle of economy. Unless such an hypothesis is admitted, the existence of an absolutely new and unrelated form of energy must be postulated, or at the least it must be supposed that Newton's law ceases to be true for minute distances, thus making cohesive tractation a special case of gravitation.

Still another line of thought is suggested by crystal formation. It is inconceivable that the links which bind molecule to molecule in crystal architecture can essentially differ from those which bind atom to atom in molecular and subcrystalline structure. As has been observed by Grüneisen in the case of crystal formation, cohesive forces are vectorised, and this is a perfectly logical result to follow from the tractation of polarised stray fields, whereas if cohesion is simply a tractation due to unknown causes and a pellation due to internal kinetic energy, a polarised external effect is most difficult to conceive.

ESSAY-REVIEWS

REAL EDUCATIONAL REFORM, by PHILIP E. B. JOURDAIN, M.A.: on **The Organisation of Thought: Educational and Scientific.** By A. N. WHITEHEAD, Sc.D., F.R.S., Fellow of Trinity College, Cambridge, and Professor of Applied Mathematics at the Imperial College of Science and Technology. [Pp. viii. + 228.] (London: Williams & Norgate, 1917. Price 6s. net.)

USUALLY we understand by the word "education" a system of mental training given with the purpose of making the student pass certain prescribed examinations, and with the ulterior purpose of enabling him to earn a livelihood. Thus, considering now what is called "secondary education," we may hear of a boy who is to go to Cambridge being described as of "scholarship standard" or of "Tripos standard." The actual knowledge is relegated to the background, and it is chiefly from some of those who, without being learned, adorn the "learned professions," and have had the advantage, if indeed it is an advantage, of spending some years at Oxford or Cambridge, that we hear that the benefit of a University education lies not so much in the acquirement of learning as in experiencing the "life of a college." I suppose that the last phrase means such things as boating and conviviality. The kind of livelihood that is to be gained by a University education is either the outcome of pursuits quite or nearly quite unconnected with the knowledge of books or life which is supposed to be acquired, as is the case with the livings got from work in the Church or at the Bar; or is the fruit of a thoroughly artificial life for which the knowledge of books and football that has been acquired may indeed be some sort of preparation. This last profession is that of schoolmastering. In that part of this profession which is concerned with the mental training of boys, the boys are taught in such a way as, in very many cases, to produce the greatest quantity of boredom for the pupil with the least expenditure of thought on the part of the teacher. This is managed by the introduction of subjects that have an absorbing life and growth of their own in a fashion that completely hides both the humanly interesting and the logically interesting aspects of these subjects. Then, too, a good schoolmaster always hopes to train up some of his most promising pupils to become adepts at the task of boring future generations of schoolboys in the same way. Some schoolmasters, I know, have fine ideals; but what has been described seems to me to represent faithfully the practice of most of those whom I have come across. The profession of teaching is an artificial way of earning a living: most of what is taught serves no intellectual or practical end, and it is doubtful if the "discipline" which is supposed to be taught by the system serves any good moral end. Most teachers are intellectually inferior men, and the feebleness and lack of real originality shown in their text-books is a sign of this. Most teachers are only saved from intellectual dishonesty by the fact that they are too stupid to realise that they are doing wrong to human minds for the sake of money to eke out their own

unnecessary existences. By hiding, with perfectly incredible stupidity, the interest—technical or intellectual—which prompted people to make discoveries and often to see, even at the time at which they made them, much of the importance which we now know they have, teachers have, as far as I can see, attained only one good result: the pleasure which every thoughtful man and woman must feel on reading Dr. Whitehead's sound and vigorous protest against their misdeeds. His book points the way to a real reform: it is not concerned with improving the means for reaching those ideals of education which have been fixed by convention—such as becoming a Wrangler or passing high into the Civil Service; it opens fundamental questions as to the why and how of education. It has the stimulus of a fresh spring morning, and Dr. Whitehead's wise and enthusiastic sympathy with the young student will rouse in him an ardent wish to understand here and now some at least of the meaning of what he learns

The book consists of eight chapters of addresses and papers published from 1912 to 1917. The only exception is the seventh chapter, and this is now published for the first time. The first five chapters deal with education—and more particularly mathematical education—and the remaining three with certain points arising in the philosophy of science. "But a common line of reflection extends through the whole and the two sections influence each other. . . . The various parts of the book were in fact composed with express reference to each other, so as to form one whole" (p. v). However, since many of the recent advances made by Dr. Whitehead in the philosophy of science have been already referred to from time to time in the accounts of "Recent Advances in Science" given in SCIENCE PROGRESS, we will, in this review, concentrate our attention on the educational reforms outlined and advocated by Dr. Whitehead.

"It is," says the author (p. 1), "useless to discuss abstract questions in the midst of dominant practical preoccupation. We cannot disregard the present crisis in European civilisation"; and yet we must not imagine that the applications of education to warlike purposes, whether of arms or of trade, fill this book to the exclusion of considerations of culture and abstract logic. This is not the case: while technological applications are given due prominence, as they should be given, it is an outstanding merit of the book that the importance of a combination of both culture and expert practical or theoretical knowledge is emphasised. "Culture is activity of thought and receptiveness to beauty and humane feeling. Scraps of information have nothing to do with it. A merely well-informed man is the most useless bore on God's earth. What we should aim at producing is men who possess both culture and expert knowledge in some special direction. Their expert knowledge will give them the ground to start from, and their culture will lead them as deep as philosophy and as high as art" (pp. 3, 4). What has been called the "divine curiosity" of the ancient Greeks and Descartes, for example, is given a high place by the author, but technology is not neglected or despised as it was by Plato. This "divine curiosity" is the pure desire for knowledge, which is sometimes as strong as, and always more lasting than, any other of those human desires for food and companionship, and so on, which are sometimes disparaged in words by the affected, and more rarely disparaged in deeds by martyrs and men of genius.

The connection of facts, whether of life or logic, is the object of knowledge, and, as for the means it uses, the essential part of "scientific method" seems to consist, firstly, in the abstinence from the shelving of problems through satisfaction with what may seem at first sight their irreducibility to logically simpler facts; and, secondly, the unprejudiced attack of these problems. The unscientific

or pre-scientific way of accounting for the facts of nature has been often confused with a religious habit of mind, and thus it is very interesting to see the new interpretation of the word "religion" which is given in this book. "The essence of education is that it be religious . . . A religious education is an education which inculcates duty and reverence. Duty arises from our potential control over the course of events. Where attainable knowledge could have changed the issue, ignorance has the guilt of vice. And the foundation of reverence is this perception, that the present holds within itself the complete sum of existence, backwards and forwards, that whole amplitude of time which is eternity" (p. 28).

The two greatest evils of traditional education—and more particularly mathematical education—of which we must beware in training a child to activity of thought are "ideas that are merely received into the mind without being utilised or tested or thrown into fresh combinations," that is to say, "inert ideas." "In the history of education, the most striking phenomenon is that schools of learning which at one epoch are alive with a ferment of genius, in a succeeding generation exhibit merely pedantry and routine. The reason is that they are overladen with inert ideas. . . . Except at rare intervals of intellectual ferment, education in the past has been radically infected with inert ideas. That is the reason why uneducated clever women who have seen much of the world are in middle life so much the most cultured part of the community. They have been saved from this horrible burden of inert ideas. . . . The result of teaching small parts of a large number of subjects is the passive reception of disconnected ideas not illumined with any spark of vitality. Let the main ideas which are introduced into a child's education be few and important, and let them be thrown into every combination possible. The child should make them his own and should understand their application here and now in the circumstances of his actual life. From the very beginning of his education the child should experience the joy of discovery. The discovery which he has to make is that general ideas give an understanding of that stream of events which pours through his life, which is his life. By 'understanding' I mean more than a mere logical analysis, though that is included. I mean 'understanding' in the sense in which it is used in the French proverb 'To understand all is to forgive all.' Pedants sneer at an education which is useful. But if education is not useful, what is it? Is it a talent, to be hidden away in a napkin? Of course education should be useful, whatever your aim in life" (pp. 4-6). Every sentence in this extract is weighty, and some of the sentences are developed further on pp. 6-11. What Dr. Whitehead calls "the two commandments to be obeyed in any educational scheme" (p. 3) are: Do not teach too many subjects; and, What you teach, teach thoroughly. The traditional remark, "The mind is an instrument; you first sharpen it, and then use it," is "one of the most fatal, erroneous, and dangerous conceptions ever introduced into the theory of education. The mind is never passive; it is a perpetual activity, delicate, receptive, responsive to stimulus. You cannot postpone its life until you have sharpened it. Whatever interest attaches to your subject-matter must be evoked here and now; whatever powers you are strengthening in the pupil must be exercised here and now; whatever possibilities of mental life your teaching should impart must be exhibited here and now" (pp. 12, 13). The way to follow this difficult rule is "to eradicate the fatal disconnection of subjects which kills the vitality of our modern curriculum. There is only one subject-matter for education, and that is Life in all its manifestations. Instead of this single unity we offer children—Algebra, from which nothing follows; Geometry, from which nothing follows; Science, from which nothing follows; History, from which nothing follows; a

Couple of Languages, never mastered; and lastly, most dreary of all, Literature, represented by plays of Shakespeare, with philological notes and analyses of plot and character to be in substance committed to memory. Can such a list be said to represent Life, as it is known in the midst of the living of it?" (pp. 13, 14). As an example of the way in which apparently widely different subjects should be connected in the mind of the student, the application of the graphs, now so generally used in algebra, is mentioned (p. 16) as providing very vivid "curves of history" which are "more informing than the dry catalogues of names and dates which comprise the greater part of that arid school study."

Our technical work should be "transfused with intellectual and moral vision and thereby turned into a joy, triumphing over its weariness and its pain. . . . The immediate need of the nation is a large supply of skilled workmen, of men with inventive genius, and of employers alert in the development of new ideas. There is one—and only one—way to obtain these admirable results. It is by producing workmen, men of science, and employers who enjoy their work" (p. 31). The social and recreative sides of Life must then not be despised: "For heaven's sake don't think that you must be dull in order to be great" (p. 64), and the claims of beauty and therefore art must be taken into account (pp. 66, 67). In a good technical education "geometry and poetry are as essential as turning lathes" (p. 33), and also "the insistence in the Platonic culture on disinterested intellectual appreciation is a psychological error. Action and our implication in the transition of events amid the inevitable bond of cause to effect are fundamental. An education which strives to divorce intellectual or æsthetic life from these fundamental facts carries with it the decadence of civilisation. . . . Disinterested scientific curiosity is a passion for an ordered intellectual vision of the connection of events. But the goal of such superiority is the marriage of action to thought" (p. 37; cf. pp 38, 41). Thus we understand the stress that Dr. Whitehead lays (pp. 10, 77) on the importance of physical applications. He tends—mistakenly, I think—to disparage the "amateur" as compared with the expert (pp. 25, 26), but this is probably because he defines, without saying so, an "amateur" as a human being who is not an expert.

The aim of education is to give a sense for "style," of which "organisation of thought" and "economy of thought" seem to be synonyms (pp. 24, 25, 105, 106). Dr. Whitehead does not point out that the history of science has shown that the essence of science is this economy, but he (pp. 81-2) suggests the history of mathematical thought—not a list of names and dates—as a way in which the student's ideas can be generalised, and adds, "Perhaps it is the very subject which may best obtain the results for which I am pleading."

Mention should be made of the following points. Dr. Whitehead lays stress on the importance of the quantitative aspect of the world (pp. 14, 15; cf. p. 45): "Elegant intellects which despise the theory of quantity are but half developed;" he points out that the essence of mathematics is generalisation and that it gives us the power of grasping general ideas (pp. 46, 93-5, 97, 101, 102); he urges that we must eliminate reconditeness in education (pp. 73, 74) and thus lay stress on the main ideas (p. 75); he discusses the place which the principles of mathematics should have in education (pp. 92-104); and sketches an ideal course in mathematics (pp. 78-90).

There are numerous witty sayings and practical points of view in this admirable book, and it is wonderful that even the passages of eloquence should be so stimulating. Take this: "When one considers in its length and in its breadth the importance of this question of education of a nation's young, the broken lives,

the defeated hopes, the national failures, which result from the frivolous inertia with which it is treated, it is difficult to restrain within oneself a savage rage. In the conditions of modern life the rule is absolute: the race which does not value trained intelligence is doomed. Not all your heroism, not all your social charm, not all your wit, not all your victories on land or at sea, can move back the finger of fate. To-day we maintain ourselves. To-morrow science will have moved forward yet one more step, and there will be no appeal from the judgment which will then be pronounced on the uneducated" (pp. 27, 28).

REVIEWS

MATHEMATICS

The Mathematical Theory of Population applied to the Data of Australian Population Statistics. Appendix A. Vol. I. By G. H. KNIBBS, C.M.G., F.S.S., F.R.A.S., Commonwealth Statistician. [Pp. xvi + 466.] (Melbourne: McCarron, Bird & Co., 1917.)

THIS volume comprises a very detailed consideration of the Australian Census Statistics, the work being, as far as possible, in mathematical form.

The first seven sections deal entirely with the theory of the formulæ, the types of curves which fit a special series of points, the determination of constants to a sufficient degree of accuracy, etc.; the remaining eleven sections give their application to the actual census returns.

An immense amount of detailed work has been done, especially on the subject of the birthrate, which is considered from many points of view, such as the ages and differences of age of the parents, the number of months between marriage and first confinement, the probability of triplets, etc.; graphs are given not only in a plane, but also of three dimensions, and account is taken of such complexities as the probability of misstatement of age and the varying position of Easter.

Considering the enormous mass of statistics and formulæ, it is not always possible to give the reasons for using any particular equation, and considerable knowledge of actuarial theory is assumed.

The author is careful to point out that mathematical formulæ need using with common sense, that they may appear to give a precision far beyond that which the data can furnish, their computation may be too laborious for practical use, and results need testing by *a priori* judgments.

This work deals essentially with the mathematical representations of statistics and not with theories or deductions from them; indeed, it is pointed out how vague any deductions must be owing to the inevitable omission of some untabulated circumstance, and the lack of corresponding statistics for different countries.

On the assumption that the present rate of increase of the world's population would exhaust the sources of energy within at most seven centuries, we need international statistics in order to regulate the expansion of mankind, and the present work aims at supplying some means whereby mathematical reasoning can be used to interpret tables of figures when they are compiled.

Minor criticisms may be passed on the mathematical language, *e.g.* "the proportion is about 133" (p. 7 footnote), meaning the ratio is about 133 : 100; the introduction of symbols without explanation; the wrong use of the sign = (p. 66); and several inaccuracies probably due to printer's errors.

Authorities may differ in their judgments as to whether the particular formulæ used are the best and soundest possible with our present knowledge; but the author has rendered great service in suggesting lines on which exact reasoning may be applied to this branch of study, and in calling attention to the care needed in handling the subject.

W. M. HUDSON.

A Course in Mathematical Analysis. Differential Equations: being Part II. of Volume II. By ÉDOUARD GOURSAT, Professor of Mathematics in the University of Paris. Translated by EARLE RAYMOND HEDRICK, Professor of Mathematics in the University of Missouri, and OTTO DUNKEL, Assistant Professor of Mathematics in Washington University. [Pp. viii + 300.] (Boston, New York, Chicago and London: Ginn & Co., 1917. Price 11*s*. 6*d*. net.)

THIS volume consists of a translation of the second half of the second volume of the *Cours d'Analyse Mathématique* of Goursat, and the former parts of the English translation have already been reviewed in SCIENCE PROGRESS (July 1917, p. 158). The book under review will form a very useful text-book on the subject of differential equations from a fairly modern point of view. In the first chapter some simple types of equations of the first order, and, more briefly, of higher order, are considered, whose integration can be effected by quadratures; and existence-theorems are only considered in the next chapter. "If this order of procedure seems subject to criticism from the point of view of pure logic, we may at least observe that it conforms to the historical development of the subject" (p. 6). In the chapter on existence-theorems, Cauchy's method of the calculus of limits is first described, then the method of successive approximations, and lastly the Cauchy-Lipschitz method; there are then short and useful sections on first integrals and multipliers, and on infinitesimal transformations. The third chapter is on linear differential equations; the fourth chapter is on non-linear equations; and the fifth and last chapter is on partial differential equations of the first order.

PHILIP E. B. JOURDAIN.

The Continuum and other Types of Serial Order. With an Introduction to Cantor's Transfinite Numbers. By EDWARD V. HUNTINGTON, Associate Professor of Mathematics in Harvard University. Second Edition. [Pp. viii + 82.] (Cambridge, Mass., U.S.A.: Harvard University Press, 1917.)

THIS exceedingly useful introduction to a most interesting part of modern mathematics originally appeared in 1905 in the *Annals of Mathematics*, and the present book is much added to and the references are brought up to date. The seven chapters are on classes in general; simply ordered classes or series; discrete series, and especially the type of the natural numbers arranged in order of magnitude; dense series, and especially the type of the rational numbers; continuous series, and especially the type of the real numbers; continuous series of more than one dimension, with a note on multiply-ordered classes; and well-ordered series, with an introduction to Cantor's transfinite numbers. The most notable addition to this edition is the account on pp. 77–9 of Hartog's theorem on the well-ordering of sets, of which some account has been given in a former number of SCIENCE PROGRESS (July 1917, p. 7). It may be remarked that in this edition, as well as in the preceding one, there is no mention that the "multiplicative axiom" is necessary to prove the theorem mentioned at the top of p. 66, nor that the theorem that the square of every Aleph number leaves the number unaltered is necessary for the proof that there is a series of Alephs. The fact also that the multiplicative axiom also enters into the proof that there is such a series is not mentioned. Indeed, much that comes out in the recent work of Whitehead and Russell is left out of account, although the work of Whitehead and Russell is referred to.

PHILIP E. B. JOURDAIN.

PHYSICS

The Electron. Its Isolation and Measurement and the Determination of some of its Properties. By R. A MILLIKAN, Professor of Physics at the University of Chicago. [Pp. xii + 268, with 33 diagrams and photographs, including 5 plates.] (Chicago: University of Chicago Press, 1917. Price $1.50 net.)

IT is probable that this little book by Prof. Millikan will become a classic on its subject, for, being admirably written in semi-popular style, it must appeal strongly to all those who are in any way interested in the progress of our knowledge of ultimate things. At the same time the very complete experimental details, the discussions based on the latest researches, and the mathematical appendices make it also a book for the specialist. The contents may be divided roughly into three parts. First comes a most lucid account of the history of electrical theories from Franklin to Johnstone Stoney and J. J. Thomson: next a full description of the author's oil-drop method of determining the magnitude of the electron (e) with its corollary, the proof of the atomic nature of electricity: finally a discussion of the probable structure of the material atom and of that outstanding problem of physics the nature of radiant energy. There are, in addition, chapters on the Mechanism of Ionisation (with the final conclusion that in every case one electron and one only is removed from the atom); on the Sub-electron, of the existence of which there is at present, in the author's opinion, no evidence; and on Brownian Movements in Gases. In this last chapter Prof. Millikan is able to quote independent measurements of Brownian movements lately obtained by Nordlund (1914) and Westgren (1915), in addition to those obtained by his colleague Dr. Harvey Fletcher, which give a value for Avogadro's number in close agreement with that deduced from his own value of e (namely $6{\cdot}06 \times 10^{23}$) and much lower than the value ($6{\cdot}85 \times 10^{23}$) previously taken by Perrin as most likely to be correct. This agreement is, of course, of fundamental importance as substantiating the accuracy claimed for the oil-drop method. It is remarkable that the latest oil-drop experiments, completed in August 1916 and pushed to the utmost limit of precision, give exactly the same value of e ($4{\cdot}774 \times 10^{-10}$) as those previously published in 1913, a new value for the viscosity of air obtained by Dr. Harrington in Prof. Millikan's laboratory exactly compensating for the small difference (0·07 per cent.) that would otherwise have been obtained!

D. O. W.

Continuity; or, From Electrons to Infinity. By P. S. G. DUBASH, D.Sc., Phil.B., Phy.M., F.S.P., F.B.E.A., F.P.C. [Pp. 60, with 2 diagrams.] (Blackburn: George Toulmin & Sons, 1917. Price 1*s*. 6*d*. net.)

THIS curious little book has been written with the idea of popularising science among the philosophically minded people of the East, the more especially by way of showing that the doctrine of the re-incarnation of the soul is not forbidden by the tenets of science, but follows almost as a natural sequence from the continuity which the author tries to trace in the evolution of man from the electron! The larger part of the thesis, which is concerned with the development of human life, contains nothing very novel, it being the author's purpose to show not that the chain of evolution is always unbroken but that the successive stages, as for example from the vegetable to the animal kingdoms, are so intermingled at their borders that no definite line of demarcation can be drawn. Passing beyond this stage into the ultra-physical region, it is suggested that death is simply the passage of "an entity from its three-dimensional shape into the fourth-dimensional form," which requires the "sixth sense" for its realisation. Finally, the author propounds a

theory of Spiralical or Retrograding progress designed to overcome the difficulty in the theory of re-incarnation that the transmigration of the soul to lower animals seems wasteful of the economy of nature. It is suggested that, originally existing in the non-animal kingdom, the soul progresses through the scale of evolution in a path which can be represented by drawing a solenoid on paper whose successive coils are almost completely circular, lines at right angles to the axis marking the boundaries of animal, human, and spirit worlds. The presentation of the thesis is ingenious, and the writer is unquestionably better able to judge of its suitability for the Eastern mind than the reviewer, to whom the fact that the resultant motion of the earth through space is along a path of the same type hardly appeals as contributory evidence of any value! The author's final conclusion is, at least, very much to the point; he exhorts that the Indians should be given their rightful due, "for remember, the Englishmen of this life may be the Indians of the next incarnation"!

D. O. W.

Dynamics: Part I. By R. C. FAWDRY, M.A., B.Sc. [Pp viii + 178 + x Answers, with 94 figures in the text.] (London. G. Bell & Sons, Ltd., 1916. Price 3*s*.)

THIS introduction to the study of Dynamics is a very refreshing contribution to a rather overburdened market.

The term "practical mathematics"—quite safe, perhaps, when used by Prof. Perry—has become truly nauseating as interpreted by many science and technical teachers. No longer is twice two to be four, but, two bricks, increased two-fold, become four bricks.

Mr. Fawdry has steered a happy mean course between the dry and uninteresting and the ultra-practical by keeping to thoroughly scientific methods in the text and using practical examples and references as illustrations. The interest of the student, thus stimulated, is maintained by historical notes—a truly excellent idea.

Further, as a text-book it is all a text-book should be, inasmuch as it leaves plenty of scope for the teacher, presenting only the essential points for the student.

In very strongly recommending this little volume, we have no doubt that teachers will help to remove those small blemishes that are bound to be found in all new works and editions. Thus, in Example 8, page 31, "How far does it fall in 6 secs. and in 12 secs.?" should read, "in the first 6 sec. and in the first 12 sec."

Incidentally we are surprised that the University Press of Glasgow should print "secs.," "ozs.," "lbs.," etc.

J. WEMYSS ANDERSON.

Optical Theories: based on Lectures delivered before the Calcutta University. By D. N. MALLIK, B.A., Sc.D., F.R.S.E. [Pp. vi + 181.] (Cambridge: at the University Press, 1917. Price 7*s*. 6*d*. net.)

IN 1912 the author of this volume was appointed Reader in Physics in the University of Calcutta, and invited to deliver a course of lectures on Optical Theories. One of the conditions attaching to this appointment was that the lectures were subsequently to be published. It seems a pity that such a condition should be laid down, and the matter of publication not left to the lecturer's discretion. In the present instance there was already available Dr. Whittaker's excellent *History of the Theories of Æther and Electricity*, published in 1910, which has rendered such a volume as the one under review unnecessary. The present volume is, indeed, considerably shorter than Dr. Whittaker's work; but this is a doubtful

advantage in a subject so comprehensive as the development of optical theories from the earliest times to the present day. The book in fact suffers considerably from undue compression, and the greater portion of it will be very difficult reading except for a student well acquainted with the subject. To the average student, Dr. Whittaker's work, with its fuller treatment and comparative avoidance of mathematics, will form both easier and more interesting reading. The author would have been wise to omit much of the mathematics; unless an argument can be given in full, long formulæ, whose derivation is not given, have but little interest.

The preceding remarks are not intended to convey the impression that the book has no merits. The author has been at considerable pains to make clear the relationship between the various theories, and how we have been led up to the present position. The most valuable part of the book is the final chapter, with its ten pages of summary, which forms an admirable and succinct account of the theories discussed in the preceding chapter. There are several misprints and omissions, and the frequent references forward are annoying. In §54 it is assumed that the ray is normal to the wave-surface, which is not necessarily true in crystalline media. In §100, the sign of the right-hand side of the first equation is incorrect. In §132, 4π is omitted in the expression for f. The nature of a displacement current is not well explained, and the distinction between displacement in a polarised medium and in free ether, which is so fundamental in Maxwell's theory, is not mentioned. The whole treatment of the question of energy in the electromagnetic field is confused, and it is difficult to see what assumptions are made in the various steps.

There is a great need for a full, rigid, and consistent treatment of this important subject; an adequate treatment is not to be found in any existing volume. Closely bound up with this looseness of the discussion of energy is the failure to maintain throughout the book the fundamental distinction between the magnetic force and the magnetic induction. In several places the author does not seem sure which he should be dealing with. For the student fully to understand the subject it is essential that such distinctions should be made absolutely clear.

H. S. J.

CHEMISTRY

A Class-book of Organic Chemistry. By J. B. COHEN, Ph.D., B.Sc., F.R.S. [Pp. viii + 344, with illustrations]. London: Macmillan & Co., 1917. Price 4*s*. 6*d*. net.).

IN this little book the author has set aside the usual conventional order of treatment of the subject, and in simple language gives a clear and readable introductory account of organic chemistry. The book is meant for first-year medical students and for senior science students in schools, and is divided into three parts. The outstanding feature is the way in which the subject-matter is throughout provided with practical illustrations and experiments, with the result that we are introduced practically at the outset to the process of fermentation and enzyme action, with a view to supplying material for illustrating the purification of organic compounds by distillation, while grape sugar is chosen for demonstrating methods of combustion, and molecular weight determination. The determination of the constitutional formula of an organic compound is explained in the second chapter by a study of the reactions of ethyl alcohol, which leads successively to the description of ethyl acetate, ethyl ether, acetic aldehyde, and acetic acid. The first part is brought to a close by a description of methyl alcohol and its oxidation

products. Part II, entitled Aliphatic Compounds, commences with an account of the paraffins, and passes on to their halogen derivatives. A few pages are then devoted in successive chapters to a description of the higher members of the series of alcohols, ethers, aldehydes, and esters; next follow the unsaturated hydrocarbons, carbohydrates, hydroxy-, dibasic, and unsaturated acids, nitrogen compounds, ureides and proteins. The two latter groups are treated in a somewhat stepmotherly fashion, for it seems rather a pity that in a book written for medical students the term "purine" should not even be mentioned, while it is likewise to be regretted that no indication whatever is given of the modern conception of the constitution of the proteins. Part III is devoted to aromatic compounds. The systematic way in which the author refrains from introducing any subject without a practical illustration is well exemplified by the fact that the last subject dealt with in the book is the molecular weight determination of bases by means of their platinum salts, which, instead of being introduced early in the book with the other methods, is here described in connection with the alkaloids. The book throughout is attractively written, and, being printed in clear type on good paper, deserves to be largely read by those commencing the study of organic chemistry.

P. H.

ZOOLOGY

A Bibliography of British Ornithology from the Earliest Times to the End of 1912, including Biographical Accounts of the Principal Writers and Bibliographies of their Published Works. By W. H. MULLENS, M.A., LL.M., F.L.S., M.B.O.U., and H. KIRKE SWANN. In Six Parts. (London: Macmillan & Co., 1916 and 1917. Price 6*s*. net each Part.)

THE last part of this useful work has now reached us, and we can but congratulate the authors on the successful completion of their laborious task. It is difficult to review such a work in the ordinary way, for one feels that one's own knowledge on the subject—so vast in its scope—is wholly inadequate to enable one to write a comprehensive criticism.

The general arrangement is good, and the idea of printing the list of Addenda and Corrigenda on one side of the paper only in order that each item may be cut out and inserted in its correct place is sound and useful. That there are omissions which have not appeared even on this list is not a matter of surprise; but as far as our personal knowledge extends, they are very limited in number.

One would expect the biographical sketches to be short, involving a selection of incidents and facts. Better judgment would have enhanced the value of some of these. For instance, in the paragraph on Dr. R. Bowdler Sharpe, no mention is made of the fact that he was President of the International Ornithological Congress in London in 1910. Yet this was probably the greatest event in his ornithological career.

We look forward with pleasure to the production of the projected Geographical Bibliography. It should prove extremely useful to the many students of county ornithology, and will supply a long-felt want.

W. R.

The Biology of Dragon-flies (Odonata or Paraneuroptera). By R. J. TILLYARD, M.A., B.Sc., F.L.S., F.E.S. [Pp. xii + 396, with 4 plates and 188 figures.] (Cambridge: University Press, 1917. Price 15*s*. net.)

FOR some reason or other the dragon-flies have not been studied from the morphological, embryological, and bionomic points of view anything as much as one

would expect from their wide distribution and striking appearance. The majority of the naturalists who have concerned themselves with this group have confined their attentions mainly, if not entirely, to the systematics The present book would be useful if it only called attention to this neglected side of the Odonata It does more than this, however, and provides a very full account of the group from all points of view, and will furnish a much-needed general description of the structure, physiology and life-history of a more or less typical insect that can be used by students. Its utility from this point of view is further enhanced by a large number of figures mostly original, a bibliography, glossary, indices, and a chapter on "Collecting, Rearing, and Biological Methods."

There is, as the author suggests, a distinct "Australian 'flavour'" about the book, since most of the forms studied by him and referred to in detail are from that country. This is not a drawback, however, as it enables the student in this country, for whose benefit a chapter on British species has been included, to work out afresh, and confirm by comparative methods, the details referred to.

Perhaps in some ways the weakest part of the book is that dealing with the embryology, and this is owing to no fault of the author's, but to the fact that our knowledge is incomplete. The standard work still remains that by Brandt, on which the author naturally draws fully; but that was published in 1879, since when not only has our knowledge of technique advanced considerably, but also a good deal more is known of the comparative embryology of insects. We feel sure that the group would repay further investigation. Throughout the author has, of course, relied considerably on the work of others, but in most cases it has been verified by his own researches, and even when this has not been done the facts and theories have been handled in a critical manner.

The book is to be commended, not only because it fills a useful place in the library of the student and general reader, but also for the broad outlook with which the subject is approached, and the lucid way in which it is stated.

C. H. O'D.

The Biology of Waterworks. By R. KIRKPATRICK. Economic Series. No. 7. [Pp. iv + 58.] (London: Printed by order of the Trustees of the British Museum, 1917. Price 1s.)

THIS excellent little volume constitutes one of the well-known Economic Series issued by the Trustees of the British Museum. It explains in a lucid and attractive fashion many of the scientific and practical questions associated with water supplies. The methods of collection, storage, and conveyance are explained and the various sources of supply indicated. The descriptions of the fauna and flora associated with water supplies, and their influence upon those supplies, are given in an extremely attractive manner, the plates being particularly good. Filtration and purification are clearly explained. This one of the series will take its place with the rest as a well-written and informative volume.

Organism and Environment as Illustrated by the Physiology of Breathing. By J. S. HALDANE, M.D., LL.D., F.R.S., Fellow of New College, Oxford. [Pp. xi + 138.] (New Haven: Yale University Press; London: Humphrey Milford, Oxford University Press, 1917. Price $1.25 net.)

THE four chapters of this little volume contain the substance of a similar number of public lectures given by J. S. Haldane at the University of Yale in 1916. They were delivered under the Silliman Foundation, and a fuller and more technical series is appearing in a separate book.

The subject-matter of the first three chapters is succinctly stated in their titles thus: I. The Regulation of Breathing; II. The Readjustments of Regulation in Acclimatisation and Disease; and III. Regulation of the Environment, Internal and External. They are very lucidly written and furnish a much-needed account of the phenomena with which they deal. Additional value is added by the fact that they come from one of the pioneer workers who, in addition to possessing a thorough knowledge of his subject, wields an engaging pen. Any one wishing to get a readable account of the research into the mechanism of breathing could not do better than to read these pages, in which the delicate mechanism of adjustment is clearly described.

In the course of his investigations the author has come to realise a wonderful regulation of the various parts of an animal and an intimate relation between the organism and the environment—a very old mystery encountered by the biologist in almost any field of inquiry. This leads him into a fourth chapter, where we meet such a bewildering welter of Physiology, Philosophy, Metaphysics, and even Theology that it cannot possibly be dealt with in the limits of a short review. In spite of the fact that the author holds both mechanistic and vitalistic interpretations of life to be unsatisfactory, it is the former view that he attacks most strongly, and his own attitude is practically that of a mystic vitalist. He adds little to this old discussion, so far almost profitless, and the arguments he adduces are entirely unconvincing.

C. H. O'D.

PALÆOBOTANY

Fossil Plants: a Text-book for Students of Botany and Geology. By A. C. SEWARD, F.R.S. [Vol. III., Pp. xviii + 656, with 253 text-figures.] (Cambridge: at the University Press, 1917. Price 18s. net.)

THE scope of this work is monumental, and yet none of the fossils are considered at length; often indeed a page, or half a page, is all that is allowed to important species. Nevertheless, it takes 656 well-filled pages to cover the essential facts concerning but four groups of fossil plants—viz. the Pteridospermeæ, Cycadofilices, Cordaitales and Cycadophyta. The science of Palæobotany is a sturdy infant.

Seven years have elapsed since the publication of Vol. II. of Prof. Seward's treatise, a time not too long for the preparation of so exacting a work, but one long enough to have rendered it difficult to complete, because almost every month has seen some addition to knowledge concerning these particular groups of fossils which have been the frequent subject both of original inquiry and explanatory controversy. That such a work should have been completed at all is a cause of congratulation, and university students and research workers must be grateful for a valuable reference-book. It is, however, hardly a book to be read through from cover to cover, save perhaps by an eager youth cramming for an examination, though the immense mass of data packed within its pages can surely never be retained by any one brain.

The four groups dealt with afford a rich harvest of interest. On the one hand, they cover representatives of families entirely extinct, though linked remotely with the primitive living Gymnosperms, revealing an immense variety of detail among those early seed plants, many of which had so much the external appearance of still extant ferns. On the other hand, the chief mesozoic representatives of the Cycadophyta, conversely, are peculiarly interesting as having had, with the external appearance of still extant cycads, so much more complex fructifications than any living member of the group.

Prof. Seward has followed the scheme of the earlier volumes, appealing both to botanists and geologists. Botanists are perhaps more considered in this volume than geologists, because, of the plants included, there is such a wealth of material revealing their inner tissue structure (in what is commonly known to palæobotanists as "petrified material"), that elaborate morphological and systematic consideration of them is temptingly suggested. The geologists, however, might have been a little more catered for. It would have taken but little extra space to tabulate the geological distribution of the leading genera considered, a point which is not only of value to a student anxious to retain clear impressions of the subject, but is often unexpectedly clarifying to morphologically conceived views of the evolutionary sequence.

Detailed criticism of such a work as this is impossible in a short review: throughout a judicial attitude is maintained by the author, and facts are gleaned from all sources and stated as impartially as may be. One is glad to see that insular prejudice has broken down, and Potonié's rightful use of the generic name *Lyginopteris*, even though it is after his death, is at last allowed to replace the long invalidated *Lyginodendron* for the leading type of the Pteridosperms.

With so much excellence there is naturally something left to be desired, particularly in the diagnoses of species. It would have added to the author's labour, but it would have greatly enhanced the clarity and value of the book, to have given short, concise, and accurate diagnoses of all the species described. While most of the illustrations are good, a number are less good than figures already published in memoirs—*e.g.* Fig. 404 E is a particularly poor illustration of an important feature admirably illustrated by others, and is doubly disappointing as it comes so close to the excellent Fig. 403. Fig. 537 is not alone in being an entirely inadequate representation of its subject.

These, however, are spots on the sun: and for the immense labour involved in this most useful compilation palæobotanists of all countries must be long indebted to the author.

ANTHROPOLOGY

Modern Man and his Forerunners. A Short Study of the Human Species Living and Extinct. By H. G. F. SPURRELL, M.A., M.B., F.Z.S. [Pp. xii + 192, with 5 plates and 1 map.] (London: G. Bell & Sons, Ltd., 1917. Price 7*s.* 6*d.* net.)

THIS work deals with Modern Man rather than with his Forerunners, descriptions of the latter only being given in order to present to the reader the biological background against which it is necessary to place living man if he is to be studied in a scientific manner. The book is divided into seven chapters, of which the first two give brief descriptions of the general problems which confront the anthropologist and of the place of man in the Animal Kingdom. Part of Chapter II. is very original and suggestive, and the author's account of the habits of some of the lower Primates is interesting and instructive. Chapter III. consists of a light sketch of the extinct species and races of men and their culture, and from this it is evident that Mr. Spurrell accepts most of the conclusions of the extreme school of prehistorians, though he does not enter into details and does not pretend to come to close quarters with the great controversial topics. The later chapters of the book are much the most interesting, and it is clear that the author has devoted himself mainly to social anthropology and sociology, rather than to physical anthropology. Chapter IV. deals with "the growth of human power and numbers

during the Neolithic Age," Chapter V. with the "Origins of Civilisation," Chapter VI. with "the growth and spread of Civilisation," and Chapter VII. with "Man at the Present Day."

The author defines the word "civilisation" in a much more precise manner than is customary in theses on such subjects, and he protests against the use of such expressions as "the Cro-Magnon civilisation." He points out that primitive hunting tribes live like the lower animals, in that they only maintain an equilibrium with the natural food resources of their environment; whereas at a certain point human societies go beyond this and increase the natural productivity of the territory wherein they dwell by taming animals, sowing crops, draining swamps, and so forth. It is this advance which he defines as the beginning of civilisation, and the definition appears to be a good and necessary one. Mr. Spurrell makes some illuminating comments on the institution of slavery, which he regards as an almost uniquely efficient engine of material progress. He points out that sheer indolence has been one of the most serious obstructions to progress. There are many things which a man might mildly desire to have, but with which he will dispense in preference to putting himself to the trouble of working to obtain them. Where, however, conquerors can, with little exertion on their own part, compel other people, a subject population, to labour for them, the conditions are altogether different, and all manner of luxurious desires will be satisfied. Moreover, the slaves themselves, living in hourly dread of the lash or of death, will work as they would never work in any other circumstances. The reader will find that on these and kindred questions the author has much which is suggestive to say.

The chief weakness of the book is the over-confident manner in which the author habitually makes his assertions. For instance, Chapter V. begins with a bold announcement that man of the modern type has inhabited the earth for hundreds of thousands of years, and the uninitiated reader would almost certainly infer from p. 5 that the Mendelian theory of heredity and evolution is established beyond dispute. The author naturally builds up his story exclusively on the basis of the theories in which he himself believes, but he should have been more careful (as he is careful in the case of the Galley Hill Skeleton) to let the reader know that many of these theories are notorious matters of controversy in the scientific world.

A. G. THACKER.

MEDICINE

An Introduction to the History of Medicine: with Medical Chronology, Suggestions for Study and Bibliographic Data. By FIELDING H. GARRISON, A.B., M.D. Second Edition, Revised and Enlarged. [Pp. 905, with Illustrations.] (Philadelphia and London: W. B. Saunders Company, 1917.)

MEDICAL men will be pleased to see that Dr. Garrison's excellent Introduction to the History of Medicine has reached a second edition. Very modestly Dr. Garrison claims for his book that he has never regarded it as anything "but a primer or guide-book to a territory of vast dimensions." It is more than that, for it contains biographical matter which is invaluable for reference to every one who writes on the past or present of medical science. Moreover, it is extremely interesting in itself, and the numerous well-printed portraits add greatly to the readers' pleasure and information. It is curious how the same medical type of face recurs in most of these portraits; and just as the priest and the lawyer have

the same physiognomy everywhere, so often has the medical man. The first chapters dealing with more or less ancient medicine are, I think, the most interesting. The two last chapters deal respectively with the beginnings of organised advancement of science in the nineteenth century, and the beginnings of organised preventive medicine in the twentieth century, and must have been extremely difficult to write. It is a question whether the author had not better have adopted another arrangement, because, at present, there is very much overlapping, owing to the fact that the same man often works in very many different fields, while short biographies have to be inserted here and there. Personally, I think, that I would have taken each major *fasciculus* of medicine by itself, at least in modern times, and have traced out its history as independently of other branches as possible, and would then have put all the modern medical biographies together by themselves in a final section. Even this would have required most exceptional knowledge, as no one man can possibly be sufficiently well acquainted with all branches of medicine not to wander away from absolute accuracy at times I certainly think it would have been better if this plan had been adopted regarding the small branch of medicine with which I am acquainted, namely, parts of tropical medicine. As it is, there are several points which I should like to see amended in future editions. I really cannot, for instance, see how F. Schaudinn did anything of any importance whatever in connection with malaria, except to make bad mistakes. It was not he, but Metchnikoff and Simond who first demonstrated true conjugation in sporozoa. His hypothesis regarding alternation of generations in parasites of owls was not only wrong, but was never based on anything approaching proof, while his theory of parthenogenesis of the malaria parasites had, if possible, less than no proof. No proof, also, has been given of the intramuscular conjugation of the malaria parasites, claimed for another author in this book; and Grassi and Bignami certainly did not show that these parasites develop only in the Anophelines, first, because no one has shown it yet, and, secondly, because they did practically no work on the subject except to make the claim, while my work and previous researches on this point were much more extensive, and cost me years of disappointment owing to the fact that I worked with the wrong kind of mosquito. As I said in SCIENCE PROGRESS (April 1917, p. 669), the claims of these people as regards priority are not to be trusted for a moment. But these are small matters, and will doubtless be corrected later on; though I hope not on my authority alone. And we owe to Dr. Garrison the establishment of an excellent foundation upon which a more and more perfect structure will ultimately be built.

R. ROSS.

The Growth of Medicine. From the Earliest Times to about 1800. By ALBERT H. BUCK, B.A., M.D. [Pp. viii + 582, with 28 illustrations.] (New Haven: Yale University Press; London: Oxford University Press. Price 21*s*. net.)

IT is a matter of regret that, owing to the crowded condition of the medical curriculum, students and practitioners of medicine too often know little or nothing of the profoundly interesting history of their own profession. A work which deals with this attractive subject is therefore to be welcomed. It is a satisfactory sign of growing interest in medical history that some American and British Universities are establishing voluntary or compulsory courses dealing with it for the benefit of their students, while, for the practitioner, such opportunities as the Historical Section of the Royal Society of Medicine in London affords are already attracting much attention and are collecting a large amount of interesting material.

Prof. Buck's work shows evidence of much study and research in certain

departments of the field, and will be found of considerable value to the inquirer, more particularly as regards the mediæval and modern departments of the subject.

Egypt and Greece are of more importance than any other countries in regard to the early history of medicine, and here Dr. Buck is somewhat defective. For example, he makes no reference to I-em-hotep, the Egyptian god of medicine, nor has he a word about his temples where the work of healing was carried on, as, for instance, Memphis. At Philæ such a temple is still in existence.

Nearly all his information on Greece and Egypt has been gathered from German or Austrian sources. Meyer-Steineg, Neuburger, and Puschmann are his main authorities. They are of value, but one rather misses acknowledgment of the work done by modern Greeks, such as Cavvadias and Stais, of various French writers, such as Defrass and Girard, also of certain Italian, American, and English investigators.

The most interesting Asklepieia hitherto examined are those at Epidaurus and Cos. The former under Meyer-Steineg's guidance is here treated in a very cavalier manner, while the same author's illustrations of Cos can only be said by those who know Cos intimately to show more imagination than accuracy. Still, with these defects, there is much of value in the account given of Greek medicine, and the interest and importance of the narrative increase as the writer passes on to Roman and Christian times, and to the saving of ancient medical manuscripts by the monks during the decadence of learning. His account of Galen and the great Roman physicians is of special interest. He shows the great debt we owe to the Arabians, who were the first nation in the west and middle east to possess paper, and cheapen manuscripts, also the influence of the Moslem University of El Azhar, and the diligence of the learned Arabians in making translations of the Greek classics. (Alas to what an abyss have El Azar and Moslem learning descended since those days!) Our author is interesting in regard to Salerno and Montpellier.

He notes the facts that in 1140 Roger, king of Naples, prohibited the practice of medicine by unqualified persons; and also the decree of the Emperor Frederick in 1240 that medical study should occupy at least five years. The account of the rise of the great medical schools in Europe, the establishment, after great difficulties, of teaching in practical anatomy in Italy, and the fact that when Charles V endeavoured to prohibit all dissection the Theological Faculty of Salamanca maintained its necessity, are all interesting items of history.

In his criticism of Paracelsus (perhaps too favourable) he speaks of the advocacy of mercury for syphilis by that charlatan.

After reference to Harvey and Linacre, he speaks of the Dutch physicians Boerhaave and others, and of the establishment of systematic bedside teaching in the hospitals at Leyden. This is probably the reason why so many British students of medicine repaired to Leyden in the seventeenth and eighteenth centuries.

The author has collected a large amount of interesting information relating to medicine in all ages, so that we can strongly recommend the book to the practitioner who in his leisure moments (at present, alas! very few) desires to acquaint himself with Roman, mediæval, and early modern medicine. Perhaps in a later edition the author may be able to add certain important details which modern research has disclosed in regard to the rise of our great calling in Egypt and in Greece and as to the difficulties and the superstitions which for so long barred its progress.

RICHARD CATON.

Health and the State. By William A. Brend, M.D. [Pp. 51.] (London Constable & Co., 1917. Price 10*s.* 6*d.* net.)

Dr. Brend has produced an exceedingly interesting volume, relating, in the main, to the beneficial influences which it is within the power of the State to exercise in the protection of the health of the people. The volume opens with a short reference to the history of public health administration, and traces the growth and expansion of sanitary operations, many of which had their origin in individual and philanthropic effort. The influence of disease in fixing or modifying geographical boundaries is considered: "The present distribution of peoples on the globe has been largely determined by the prevalence of different diseases in different parts." The writer indicates the immensely beneficial results of the application of scientific discoveries in the prevention of disease, by making the application of preventive measures more accurate and more precise, especially in regard to the insect vermin pest; the manner and extent to which disease is carried by insects is dealt with, and it is obvious that further action, based upon this knowledge, is still called for, as Sir Ronald Ross remarks, "It is not the fault of science that we do not fully utilise the gifts which she gives us."

Dr. Brend gives its proper place to the importance of education in regard to the advancement of public health, an education not limited to the popular teaching of hygiene in schools, but so extended as to embrace all those responsible for public health administration, whether officials, medical officers of health, councillors, members of Parliament, cabinet ministers and those who advise them, and he lays special emphasis upon the importance of an adequate Ministry of Health. Upon this point it may be thought that the public are becoming fairly enlightened, but those who are interested in maintaining the health of the community (and who is not?) must, from the publicity given to the question, begin to realise the difficulties which beset the advancement of health conditions when each of half a dozen Government Departments are taking a share in the administration; it must be clear that, if there is a division in administration at the centre, there cannot be other than division and consequent overlapping at the periphery. Dr. Brend devotes considerable attention to the problems of urban life, notably in connection with housing; in this connection it must not be overlooked that more recent legislation has simplified the problem of dealing with insanitary areas, by making procedure easier and less costly. The Housing and Town Planning Acts, for example, have been the means of facilitating improvements in congested parts of cities, and the provision of open spaces, whilst the conversion of long-disused burying-grounds into public gardens has been a further step in the right direction. Housing experiences in Liverpool have proved conclusively that the clearing away of large insanitary areas, and rebuilding on sanitary principles on those sites, has been of great benefit, the special feature in the case of Liverpool being that the former occupants of the insanitary dwellings are, with rare exceptions, the only tenants accepted in the new ones. Habits improve under the new environment, and the improved habits and improved environment together have resulted in greatly lowering the sickness and mortality amongst these people.

In his references with regard to "sleeping out," Dr. Brend, perhaps, has not appreciated that the objections to promiscuous "sleeping out" were that the persons who indulged in the practice were undesirable, and were out during the night for other purposes than merely sleeping out. A wide range of subjects affecting the public health is dealt with in the volume, and it certainly deserves attentive perusal.

The Causes of Tuberculosis, together with some Account of the Prevalence and Distribution of the Disease. By LOUIS COBBETT, M.D., F.R.C.S., University Lecturer in Pathology, Cambridge. A volume in the "Cambridge Public Health Series" issued under the Editorship of G. S. GRAHAM-SMITH, M.D., and J. E. PURVIS, M.A. [Pp. xvi + 707.] (Cambridge: The University Press, 1917. Price 21s. net.)

WAR has seriously hindered the progress of the Tuberculosis Movement in this country. Of the truth of this there is abundant evidence. That serious restriction has occurred in our anti-tuberculosis work and endeavours to meet the needs of tuberculous sufferers is made clear by Sir Arthur Newsholme, Chief Medical Officer of the Local Government Board, who, in his recently issued supplement to the Annual Report of the Local Government Board for 1916-17 (Cd. 8767), declares: "One of the most urgent of after war problems will be to secure the early resumption and extension of this work [tuberculosis work], particularly in regard to the provision of greatly increased hospital accommodation for advanced cases of consumption." It is certain that there will be a considerable development in the prevalence of tuberculosis: that this is already the case is shown by the returns from many centres. The deaths from pulmonary tuberculosis in both 1915 and 1916 were 12 per cent. in excess of those in 1913. War conditions and probably after-war conditions must serve to encourage the establishment and spread of tuberculosis among most classes of the community.

In order to meet the many needs of the present and the almost certain increase in the requirements of the near future, there is every reason why the most serious consideration should be given at once to the Tuberculosis Problem. Hitherto many of our endeavours have been experimental, extravagant, and unscientific. We have been content to be directed by political and social expediency, controlled by sentimental considerations, satisfied with merely palliative and ameliorative measures, and ready to wage warfare with an inadequate service and an imperfect equipment. Our enterprises have lacked co-ordination, and our workers have not learnt the importance of co-operation. The existing attitude of mind and the prevailing insufficiency of practice must be changed if serious calamity to large sections of the community is to be avoided. We need to seek guidance in a clearer conception of scientifically established fundamentals. Since Koch's epoch making discovery of the tubercle bacillus investigations have been carried on in all parts of the world, and many of these have thrown a flood of light on the ætiology of tuberculosis. The records of the most important of these researches and the conclusions which are to be drawn from them have been gathered together and presented with great skill by Dr. Louis Cobbett in his recently issued monograph on *The Causes of Tuberculosis.* This monumental book is one the value of which cannot well be exaggerated. The author has collected his material with painstaking precision and a rare judicial discrimination, and has set forth facts, theories, arguments, and controversial considerations with philosophic calm, logical force, and much literary skill. The book is well arranged, effectively illustrated, abundantly provided with references, and there are serviceable indexes to subjects and authors. Needless to add, the University Press of Cambridge has issued the volume in a worthy form. No serious student of tuberculosis can afford to be neglectful of this notable volume, the most valuable contribution to the subject which has appeared in this country for many a long day, and one which will always rank as a first-class work of reference.

Dr. Cobbett's book is addressed mainly to those who are concerned in the stamping out of tuberculosis, and does not seek to deal with the clinical aspects of

the disease. He approaches the subject from the standpoint of the experimental pathologist. The principal object of the author has been to bring together into a convenient form suitable for ready reference an account of the most important researches relating to the ætiology of tuberculosis, and particularly of the investigations carried out by the late Royal Commission of Tuberculosis, the Reports issued by the Local Government Board of England and Wales, and the records of the Department of Health of the City of New York and the Imperial Board of Health in Berlin. The work opens with a statement summarising the heavy tribute exacted by tuberculosis, and this is followed by a consideration of factors which have been influential in the past in securing a decline in the prevalence of the disease. The major portion of the volume is devoted to an able exposition of available knowledge respecting the tubercle bacillus and its various varieties or "Types." Particulars are provided regarding distribution, cultural characteristics, comparative virulence, and special attention is directed to the sharpness of characterisation of these types and their stability under conditions which might be supposed to conduce to modifications. These considerations, instead of being viewed as merely of academic interest, are really of immense importance in the conduct of a scientifically directed campaign for the arrest and eradication of tuberculosis.

While Dr. Cobbett's book will doubtless appeal mainly to members of the medical profession, it is so lucidly written in crisp, direct, unambiguous words, and as far as possible in non-technical form, that it can be read with profit by all thoughtful men and women. Difficulties, ignorances, controversies are all fully faced and dealt with fairly and in detail; but the author does not hesitate to express doubts or formulate opinions. He deals, in particular, with the part played by personal contagion, the relative importances of individual predisposition, opportunities for infection, influence of quantity of dosage of effective tubercle bacilli, channels and portals of entry, and the like. The question of tuberculosis in animals receives lengthy consideration, and the difference in response to invasion by tubercle bacilli presented by different species is pointed out, special attention being directed to the types of bacilli found in instances of naturally acquired tuberculosis in each animal species and in the relative susceptibility of that species to infection with each of the three types as shown by artificial experiment. It is interesting to note that it is not always the type which is most virulent for a given species which is responsible for the majority of instances in it of naturally acquired disease. The descriptions of the lesions and types of tubercle bacilli found in human tuberculosis are very full and of far reaching practical importance. It is interesting to find the following opinion expressed in regard to the relative importance of human and bovine infection in so-called "surgical" forms of tuberculosis: "Bone and joint tuberculosis appears in most countries to be due largely to tubercle bacilli of human type. The contribution made by infection from a bovine source would appear from the evidence available, which is admittedly somewhat meagre, to be on an average in various countries, excluding Scotland, small. In Edinburgh and neighbourhood a very large share has been claimed for bovine infection, amounting roughly to 60 per cent. of the cases in children. This claim needs confirmation. It seems to us unlikely that there should be so great a difference between Edinburgh and other great cities. Possibly some 'pocket' of bovine infection has been tapped. Nevertheless, a substantial case has been made out both in the case of tuberculosis of bones and joints, and in that of cervical glands, for the view that the relative proportion of infections with tubercle bacilli of bovine and human type differs considerably in

different places." The concluding chapter summarises the evidence respecting the comparative distribution of the three types of tubercle bacilli in various animal species and the part, if any, played by each in the various kinds of tuberculosis in man.

Dr. Cobbett's book is a notable contribution to the study of essential factors in the prevention of tuberculosis—the disease which he justly describes as "the commonest, most fatal, and perhaps the saddest, of those which oppress mankind." And amidst all contending claims it is shown that one conclusion is certain: "The principal desideratum is that the public should learn that tuberculosis is contracted from consumptive patients, and without panic and without inflicting unnecessary hardship should in the light of this knowledge take steps to protect themselves and their children." This is the great lesson we need to realise in these sacrificial days of war and in face of the testing which is coming in the near future under after-war conditions.

This notice of a remarkable book is too inadequate, but it will doubtless be sufficient to indicate that all students of tuberculosis and every worker for the prevention and arrest of this dire plague should study the work in its entirety. Such knowledge as is here so effectively garnered and conveniently presented affords the surest grounds for optimism, and will go far to justify the anticipation expressed in the author's closing chapter, although the opinion presented appears to have been formulated before the outbreak of war and its consequent spread of tuberculosis:

"The importance of tuberculosis is not to be measured only by the fact that it causes in England and Wales alone the death of over fifty thousand persons each year, large as this number is. A very considerable proportion of those deaths occur in the prime of life, or only a little earlier, and in addition to these deaths tuberculosis produces a great number of cripples. This mortality, large though it is now, is smaller than it was half a century ago. Since that time the number of deaths caused each year by tuberculosis has diminished steadily and substantially, and the ratio of deaths has fallen by more than 50 per cent. This death-rate is now declining rapidly and at an ever-increasing velocity. It is not too much to say that if this decline should continue to progress more or less along the line it has followed in the recent past, tuberculosis will have become a rare disease before the end of the century. The eyes of some, indeed, discern already on the far-away horizon signs of its complete eradication. The vision may prove to be a delusion, but at any rate even the most matter-of-fact of us may look forward with confidence to a time, at no very distant date, when society will be relieved of this incubus, which at present causes the death of one person in every ten in this land and cripples and disfigures many another."

T. N. Kelynack, M.D.,
Editor of *The British Journal of Tuberculosis.*

Shell-shock and its Lessons. By Prof. G. Elliott Smith, F.R.S., F.R.C.P., M.D., and T. H. Pear, B.Sc. [Pp. xi + 135.] (Manchester: at the University Press, and London: Longmans, Green & Co., 1917. Price 2*s*. 6*d*. net.)

The public in this country is so acutely sympathetic towards the urgent need for relieving our soldiers and sailors who have been invalided from the Forces, that the National Health Insurance Commissioners have issued special instructions to Medical and Panel Committees throughout the land for their continued treatment. It is fully recognised that curing a man's disabilities is the first step for him to become a useful citizen afterwards, and possibly, of all the disabling causes, the

psycho-neuroses stand first and are among the most persistent as well as the most difficult to deal with and to cure. Any text-book or pamphlet, therefore, which can serve as a helpful guide must deserve to be counted as so much economical gain in the reconstruction after the war.

The Abbé Girard once said that the number of synonyms in a language was the best indication of the richness of that language, and in this particular we have no need to bewail the copiousness of our own, for the terms shell-shock, war-shock, psychasthenia, neurasthenia, anxiety neurosis and hysteria have all been applied to almost identical symptoms, and in the general scheme of cases described in the present volume, we think it would have been more relevant and more precise and therefore more discriminating if the term "war-shock" had been selected, for it would then include the authors' groups—viz. "all those mental effects of war experience which are sufficient to incapacitate a man from the performance of his military duties," those causes, in fact, which the joint-authors purpose to treat; reserving "shell-shock" for actual physical and material damage to the nervous system from concussion, gas, or other local injury; and it is a fact of experience that the symptoms of so-called "shell-shock" are rarer in the trenches than behind the lines, at the base or at home - an additional reason why the term "war-shock" would seem to be the more preferable.

The authors insist upon an emotional shock or some frequently repeated emotional stimulus as the chief cause, with which we do not agree; but they rightly assert that the forced (*i.e.* voluntary) suppression of the outward signs of an emotion tends in persons with certain temperaments, dispositions and character, to dissociative dissolutions; because the emotions are denied the normal outlet channels and they emerge through the "mental conflict" as a discharge along some other (probably more strange, bizarre or unusual) channel. The authors point out there are at least three stages in emotional "shell-shock," the initial period, rarely discoverable to the outside observer, being the stage of auto-suggestion or of incubation; then the middle period, during which the feelings, emotions and ideas that are in the mind exist in a state of flux; and the final period, during which all the morbid states are being assimilated and associated, eventually seizing upon the mind as systematised or rationalised notions, ideas which McDougall has described as being "intellectualised." The incubatory stage is well known to those who watch the development and the result of fear; the encouragement of the riding-master or the huntsman, for instance, to induce the tyro to remount after a "spill" is the psychological application of treatment during this stage; but it is during the middle period of flux that the authors recommend individual treatment in shell-shock, and this by hetero-suggestion, or hypnosis or isolation, the endeavour being to re-educate the sufferer through sympathy, and the strong and firm appeal which is made by the personality of the doctor, although naturally, the whole environment—*i.e.* all who are in charge of the case—should similarly be the encouraging factor. The authors lay too much stress, the reviewer thinks, upon the value of dreams, even if they can be correctly interpreted; but the phenomena connected with dreams, their classification, the connection between the psychical life of the dream and that of the waking hours, together with the variable codes of interpretation, render dreams far from being a constant diagnostic help or value in treatment. The third chapter is devoted to the dream; to psychological analysis; and to re-education. A clear account is given of the crystallisation of a delusion or belief, which is precisely similar to that occurring in the integrations of healthy mental life. Much importance is attached to the unconscious factor of the mind, and considerable attention is devoted to the formulation and the answering of

possible objections to psychological analysis, which is a path running alongside, if not quite parallel to, psycho-analysis, but the advocates of the latter are split up into many groups Jung, formerly its greatest advocate, is no longer of the fold, though within the school—and the authors earnestly endeavour to persuade those readers who are disposed to be critical, that a rich harvest shall reward them if they delve deep enough into the unconscious mind. For ten or more years criticism has proceeded vigorously upon explorers into this territory, and it is not yet finally decided whether it is not a rubbish-heap, in which by diligent search may be discovered anything the explorer is seeking for. Many troubled minds are glad to avail themselves of imagined and unreal occurrences to explain their abnormal mental states; trivialities are magnified, suspicions are entertained and personal unworthiness expressed, which all pass off with improved physical health and during convalescence; but until then no appeal to common sense can banish these ideas from the mind.

The main theme of the book is the psychic side and the psychic treatment of shell-shock, but about one-half of the little volume is devoted to a hostile criticism of the present method of treating the insane in this country, of which, however, the authors do not speak from personal experience. It is futile, therefore, to pursue this portion as a lesson to be derived from the treatment of shell shock, and we shall dismiss this part as not logically following from the text. The section dealing with shell-shock is of definite psychological interest, is ably written, and will furnish food for reflection to many lay readers.

ROBERT ARMSTRONG-JONES.

MISCELLANEOUS

The Passing of the Great Race; or, the Racial Basis of European History. **By** MADISON GRANT. [Pp. xxi + 245.] (London: G. Bell & Sons, 1917. Price 8*s*. 6*d*. net.)

THIS is the first time that Mr. Madison Grant has written the history of Europe in terms of race. The object of the book is, as far as we can see, to exhort his fellow Americans who are descended from those of the colonial period to preserve their race-purity and position at the head of affairs against the hordes of Polish Jews, Southern Italians, Syrians and Croats who are invading America and are threatening to lower the American race physically, socially, morally, and intellectually. The whole book seeks to prove that heredity is deeper-rooted and more potent than environment. In the opening chapter Mr. Grant discusses race and democracy. He points out that language and nationality do *not* mean race. Thus there is no such race as Celtic, though there is a group of Celtic languages, and no such race as Germans, though there is both a German nation and a German language. In a country where there are diverse racial elements, democracy always selects the lowest type, or the average man, for office. In fact, it tends to standardise type. Mr. Grant goes on to describe the physical basis of race and the habitat of each. There are three races in Europe: Firstly, the Nordic or Baltic subspecies; long skulled, fair, with light-coloured hair and eyes. It inhabits the lands round the Baltic and North Seas. Secondly comes the Mediterranean or Iberian race; long skulled, swarthy, short, with very dark hair and eyes. It inhabits the Mediterranean basin, and extends along the Atlantic coast until it reaches the Nordic race. Lastly comes the Alpine subspecies inhabiting the massif of Central Europe and extending east to Central Asia. They are round skulled, of medium height and sturdy build, dark hair, and dark

eyes which, however, are often grey, especially in Western Europe. Mr. Grant maintains that if a race be forced to live outside its natural habitat, the race will die out, either from too hot or too cold a climate, as the case may be, or from intermarriage with an acclimatised race. He also gives many reasons why race is more potent than either language or nationality, and in support of this he cites several arguments. He then goes on to describe the aptitudes of the races for colonising. He maintains that members of the Nordic race cannot live south of the latitude of Virginia, and that the American of the colonial period is getting ousted from his land by hordes of low-type foreign immigrants.

In the second part of the book Mr. Grant begins by describing the remains found of early man and also a description of the Neanderthal race and that splendid race known as the Cro-Magnon. The description of the races of Europe up to Roman times is then described. The history of the three main races of Europe is next told, together with their place of origin and a description of the nations they have formed. There is a special chapter on the Teutonic branch of the Nordic race and another on the expansion of the Nordics. Two chapters follow this, one dealing with the place of origin of the Nordic races, and the other with those portions of the Nordic race which were and are found outside Europe. He believes that the Ainus of Northern Japan are an extreme easterly extension of the Proto-Nordics at an early period. Mr. Grant believes that the Mediterranean race is superior to both the Nordic and Alpine in intellectual attainments; it is certainly superior to the latter race. Its physique, however, is much inferior. The Alpine populations are, for the most part, peasants, while the Nordics are a race of soldiers, sailors, adventurers, explorers, rulers, organisers, and aristocrats. Chivalry, knighthood, and feudalism are traceable, for the most part, to the north. Mr. Grant then concludes with a study of the Aryan languages, their origin, and the Aryan language in Asia. At the end of the book are four interesting maps, the first on the expansion of the Alpines, the second on the expansion of the Pre Teutonic Nordics, the third on the expansion of the Teutonic Nordics and Slavic Alpines, and the last on the present distribution of races in Europe.

C. C ROSS.

Thrice through the Dark Continent: a Record of Journeyings across Africa during the Years 1913-16. By J. DU PLESSIS, B.A., B.D., Professor in the Theological Seminary of the Dutch Reformed Church, Stellenbosch, South Africa. [Pp. viii + 350, with 60 illustrations and maps.] (London: Longmans, Green & Co., 1917. Price 14s. net.)

MR. DUPLESSIS is an agent of a great South African Missionary Society which is strongly represented in Nyasaland. He had already travelled much in Tropical Africa, partly in connection with mission work, but generally with an observant eye for all phases of his environment.

The book herewith reviewed records journeys really remarkable for their extent and the relative ease with which they were carried out between 1913 and the close of 1916. In those three years Mr. Duplessis visited Ashanti and much of the Gold Coast, the coast of Dahomé, Southern Nigeria, and the Cameroons; travelled far up into Eastern Nigeria, ascended the Benue River, and traversed the region between the Benue and the Logone-Shari; crossed the watershed of the Congo, passed through Uganda, criss-crossed the Congo Forest, saw something of British and German East Africa, and much of the main Congo and its great tributaries. He traversed Northern Rhodesia and revisited Nyasaland.

Through his eyes we are made to realise the wonderful advance which Central Africa has made during the first sixteen years of the twentieth century. The author writes from quite a detached position, scarcely (as one might expect, seeing that he was born a British subject) showing any special preference for British rule; yet one lays down his book with a very optimistic feeling. If he has much that is good to say of what France has done to justify her rule in Dahomé and the Shari basin, if he praises emphatically the Belgian administration of Congoland, his summing up of the benefits which have accrued to Nyasaland, Ashanti, Nigeria, Uganda, and East Africa from British intervention are really heartening to any one who may have doubts about the ethics of the white man's interference with the Negro and the Negroid.

Incidentally, Mr. Duplessis supplies some interesting zoological, botanical, and ethnological information worthy of note, and he is to be congratulated on writing in a style which is accurate without pedantry, which is concise, and entirely free from gush. Perhaps the only carping remark I might make is that his frequent quotations from ancient and modern poets are not always apposite, and are usually an irritating excrescence like needless paper frills round an excellent mutton chop.

J.

The Public School System in relation to the coming Conflict for National Supremacy. By V. SEYMOUR BRYANT, M.A. [Pp. xvii + 78.] (London: Longmans, Green & Co., 1917. Price 1s. 6*d*. net.)

IN Finland they have a pleasant custom of taking you at the lunch hour to a sideboard crammed with all sorts of cold meats and delicacies which the uninitiated are more than likely to mistake for the real repast, to which these numerous dishes form but a preface. To a caviller, at least, Mr. V. S. Bryant's book on the public school system suggests a certain number of resemblances to the Finnish mode of serving the midday meal. First there is a *hors d'œuvre* in the shape of a foreword by Lord Rayleigh, followed by another fresh from the author's pen, while the preserved element (let us add it has no taste of the tin!) is represented by a third preface on the essentials of a liberal education by Huxley, and finally the first chapter bears the title of Introduction.

Apart from this plethora of presentations and introductions, we can honestly say we have rarely come across a book on education of so modest a compass with so many good things in it, which are so pithily and convincingly put. Some statements may seem too sweeping, but the note of good temper and sanity and the desire to be fair are everywhere in evidence. The author is a public school master, who has also had experience in industrial and commercial life. In a short review one can only catalogue some of the many excellences of the book, while adding here and there the briefest of comments. But if it is the moderate man in English politics who really decides what shape reforms should ultimately take, then Mr. Bryant's book, which seeks to provide a *via media* between the ultras on the classical and scientific sides, should wield a good deal of influence.

Mr. Bryant's case against the classical monopoly in our public schools seems irresistible, thanks to the hard facts on which it is based. We learn from his pages that, out of 114 schools, 92 are under classical head masters, and these contain 28,300 pupils out of a total of 33,400. Incidentally he informs us that Oxford at least is in even a worse plight: of the 21 heads of colleges not one has had a scientific education. *En passant*, the author disposes of the futile allegation that reformers who desire to give Science its proper place in the school studies

are aiming at Germanising English education; any one with even a superficial knowledge of the Gymnasiums and Real-gymnasiums knows that this is a lie. These classical experts do not even know what Classics are being taught in schools similar to their own abroad, and so naturally they know still less of how much or how little Science is taught. As a matter of fact, the curse of German education and German bureaucracy is not the predominance of Science, but of Law. In German administration, by the avowal of the Germans themselves, it is the *Juristen* who are top dog. Excellent, again, is the author's diagnosis of the problem of the preparatory school, run for profit and deriving from its connection with the schools above most of the disadvantages and few of the advantages that such a collaboration should bring. It is noteworthy that the average English mother (for it is the mother in general who chooses the school) is guided in her selection by social considerations—a striking contrast, by the way, to the French mother, whose chief consideration is to find a school where her boy is really taught. Equally valuable are Mr. Bryant's views on the reform of the public schools' curriculum, though he rather runs the "set" system to death. For reasons impossible to give here, we are firmly convinced that the real solution is a working compromise between the form and the set. Lack of space alone prevents more than an allusion to his masterly analysis of what the ignorance of our rulers of the potentialities of science has cost the nation. The average Englishman who reads it should be startled for ever out of his beliefs in the sufficiency of classical studies for everybody.

CLOUDESLEY BRERETON.

BOOKS RECEIVED

(*Publishers are requested to notify prices*)

Elementary Mathematical Analysis. By John Wesley Young, Professor of Mathematics, Dartmouth College, and Frank Millet Morgan, Assistant Professor of Mathematics, Dartmouth College. New York: The Macmillan Company, 1917. (Pp. xii + 548.) Price 11*s*. net.

A First Course in Higher Algebra. By Helen A. Merrill, Ph.D., Professor of Mathematics in Wellesley College, and Clara E. Smith, Ph.D., Associate Professor of Mathematics in Wellesley College. New York: The Macmillan Company, 1917. (Pp. xiv + 247.)

Frequency Curves and Correlation. Addendum with Diagram and Errata. By W. Palin Elderton, F.I.A. London. Printed for the Institute of Actuaries by Charles and Edwin Layton, Farringdon Street, E.C., 1917. (Pp. 22.) Price 1*s*. 6*d*. net.

Celestial Objects for Common Telescopes. By Rev. T. W. Webb, M.A., F.R.A.S. Sixth Edition, Thoroughly Revised by Rev. T. E. Espin, M.A., F.R.A.S. In two Volumes. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1917. (Pp., Vol. I. xx + 249; Vol. II. vii + 319, with illustrations.) Price 7*s*. 6*d*. net each volume.

The Distances, Absolute Magnitudes, and Spectra of Seven Hundred and Thirty-four Stars. Arranged for use with Ordinary Star Maps. By Thomas Edward Heath, F.R.A.S. Sold for the Benefit of the Tenby War Emergency Depot. By the Hon. Secretary, Miss Crealock, South Cliff Street, Tenby, and by all Map and Book Sellers. (Pp. 52.) Price 2*s*. 6*d*. net.

Artificial Dye-Stuffs. Their Nature, Manufacture and Uses. By Albert J. Ramsey and H. Claude Weston. London: George Routledge & Sons; New York: E. P. Dutton & Co., 1917. (Pp. ix + 211, with 24 illustrations) Price 3*s*. 6*d*. net.

The Training and Work of the Chemical Engineer. A General Discussion held by the Faraday Society on Tuesday, March 6, 1917; Sir Robert Hadfield, F.R.S., President, in the Chair. Reprinted from the *Transactions of the Faraday Society*, Vol. XIII., September 1917. (Pp. 60.) Price 3*s*. 6*d*.

Our Analytical Chemistry and its Future. By William Francis Hillebrand, Ph.D., Chief Chemist of the Bureau of Standards, Washington, D.C. New York. Columbia University Press, 1917. (Pp. 36.) Price 1*s*. 6*d*. net.

A German-English Dictionary for Chemists. By Austin M. Patterson, Ph.D., formerly Editor of *Chemical Abstracts*. New York: John Wiley & Sons: London: Chapman & Hall, 1917. (Pp. xvi + 316.) Price 9*s*. 6*d*. net.

A Text-Book of Inorganic Chemistry. By A. F. Holleman, Ph.D., LL.D., P.R.A. Amst., Professor Ordinarius in the University of Amsterdam, Emeritus Professor Ordinarius in the University of Gröningen, Netherlands. Issued in English in Co-operation with Hermon Charles Cooper. Fifth English Edition, Completely Revised. New York: John Wiley & Sons; London: Chapman & Hall, 1916. (Pp. viii + 528.) Price 10*s*. 6*d*. net.

The Chemical Constitution of the Proteins. By R. H. A. Plimmer, D.Sc., University Reader in Physiological Chemistry, University College, London. In Three Parts. Part I. Analysis. Third Edition. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta and Madras, 1917. (Pp. xii + 174, with diagrams.) Price 6*s*. net.

Volcanic Studies in Many Lands. Being Reproductions of Photographs taken by the Author. By Tempest Anderson, M.D., B.Sc., Hon. D.Sc. Leeds, F.G.S., F.R.G.S., Fellow of University College, London; Hon. Secretary, Yorkshire Philosophical Society. The Text by T. G. Bonney, Sc.D., LL.D., F.R.S., etc., Emeritus Professor of Geology in University College, London, and Fellow of St. John's College, Cambridge. Second Series. London: John Murray, Albemarle Street, W., 1917. (Pp. xv + 88.) Price 15*s*. net.

The Ohio State University Bulletin. The Grasses of Ohio. By John H. Schaffner, Ohio Biological Survey. Bulletin 9, Vol. II. No. 5, May 1917. Ohio: Published by the University at Columbus. Vol. XXI., No. 28. (Pp. 256–329.)

Histology of Medicinal Plants. By William Mansfield, A.M., Phar.D., Professor of Histology and Pharmacognosy, College of Pharmacy of the City of New York, Columbia University. New York: John Wiley & Sons; London: Chapman & Hall, 1916. (Pp. xi + 305, with 127 plates.) Price 13*s*. 6*d*. net.

British Grasses and their Employment in Agriculture. By S. F. Armstrong, F.L.S., Univ. Dipl. Agric. (Cantab.), of the School of Agriculture, Cambridge University, and Director of the United Kingdom Seed Control Station, Cambridge. Cambridge: at the University Press, 1917. (Pp. vii + 199.) Price 6*s*. net.

Morphology of Gymnosperms. By John M. Coulter, Ph.D., Professor of Botany in the University of Chicago, and Charles J. Chamberlain, Ph.D., Professor, of Morphology and Cytology in the University of Chicago. Revised Edition. Chicago, Illinois: The University of Chicago Press. (Pp. xi + 466, with 462 figures in the text.) Price $5 net.

The Indian Forest Records. Statistics compiled in the Office of the Silviculturist, Forest Research Institute, Dehra Dun, during 1915-16 Vol. VI. Part II. Calcutta; Superintendent Government Printing, India, 1917. (Pp. iv + 65.) Price 2*s*. net.

The Anatomy of Woody Plants. By Edward Charles Jeffrey. Chicago, Illinois: The University of Chicago Press. (Pp. x + 478.) Price $4 net.

How to Collect and Dry Flowering Plants and Ferns. By Harold Stuart Thompson, F.L.S. London: George Routledge & Sons, Broadway House, 68–74, Carter Lane, E.C., 1917. (Pp. 56.) Price 7*d.* net.

Moth-Borers Affecting Sugar-Cane in Mauritius. By D. D'Emmerez de Charmoy, Department of Agriculture, Mauritius. Scientific Series. Bulletin No. 5. English Edition. Mauritius: The Government Press, Port Louis, 1917. (Pp. 27.)

Records of the Indian Museum. Vol. XIII. Part IV. August 1917. Calcutta: Published by the Director, Zoological Survey of India. Printed at the Baptist Mission Press, 1917. (Pp. 176–241.) Price 2 rupees.

Memoirs of the Indian Museum. Fauna of the Chilka Lake. Vol. V. No 6, August 1917. Calcutta: Published by the Director, Zoological Survey of India. Printed at the Baptist Mission Press, 1917. (Pp. 483-508,) Price 1 rupee 8 annas.

Rustic Sounds and other Studies in Literature and Natural History. By Sir Francis Darwin. London: John Murray, Albemarle Street, W., 1917. (Pp. 231, with illustrations.) Price 6*s.* net.

The Natural History of the Farm. A Guide to the Practical Study of the Source of our Living in Wild Nature. By James G. Needham, Professor of Limnology, General Biology and Nature Study in Cornell University. Ithaca, N.Y.: The Comstock Publishing Company, 1916. (Pp. 348, with 140 figures in the text.) Price 50 cents.

The Life of Inland Waters. An Elementary Text-book of Fresh-water Biology for American Students. By James G. Needham, Professor of Limnology in Cornell University, and J. T. Lloyd, Instructor in Limnology in Cornell University. Ithaca, N.Y.: The Comstock Publishing Company, 1916. (Pp. 438.) Price $3.

Modern Whaling and Bear-Hunting. A Record of Present-day Whaling with Up-to-date Appliances in many parts of the World, and of Bear and Seal Hunting in the Arctic Regions. By W. G. Burn Murdoch, F.R.S.G.S. London: Seeley, Service & Co., 38, Great Russell Street, 1917. (Pp. 320, with 110 illustrations, chiefly from drawings and photographs by the Author.) Price 21*s.* net.

Shells as Evidence of the Migrations of Early Culture. Ethnological Series, No. II. By J. Wilfrid Jackson, F.G.S., Assistant Keeper, Manchester Museum; Hon. Librarian of the Conchological Society of Great Britain and Ireland. Manchester: at the University Press, 12, Lime Grove, Oxford Road; London: Longmans, Green & Co., 1917. (Pp. xxviii + 216, with 13 illustrations.) Price 6*s.* net.

Primitive Man. By G. Elliot Smith, F.R.S. From the *Proceedings of the British Academy*, Vol. VII. London: Published for the British Academy by Humphrey Milford, Oxford University Press, Amen Corner, E.C. Price 3*s.* 6*d.* net.

Radiography and Radio-Therapeutics. By Robert Knox, M.D. (Edin.), M.R.C.S. (Eng.), L.R.C.P. (Lond.), Consulting Radiologist, Great Northern Central Hospital, London; Hon. Radiographer, King's College Hospital, London; Director, Electrical and Radio-Therapeutic Department, Cancer Hospital, London; Captain R.A.M.C. (T.), Fourth London General Hospital (in charge of X-ray Department). Part I. Radiography. London: A. & C. Black, Ltd., 1917. (Pp. xxv + 382 + xx, with 78 plates and 337 illustrations in the text.) Price 30*s.* net.

Report of the Medical Officer of Health for the Year 1916. By Meredith Young, M.D., D.P.H., of Lincoln's Inn, Barrister-at-Law, County Palatine of Chester. Presented to the Public Health and Housing Committee of the County Council, October 19, 1917. Chester: Phillipson & Golder, Eastgate Row and Frodsham Street. (Pp. viii + 132.)

Report of the Chief School Medical Officer for the Year 1916. By Meredith Young, M.D., D.P.H., D.Sc., of Lincoln's Inn, Barrister-at-Law, Lecturer in School Hygiene, Victoria University of Manchester. Cheshire County Council Education Department. (Pp 19.)

The Conduction of the Nervous Impulse. By Keith Lucas, Sc.D., F.R.S., Fellow and Lecturer of Trinity College, Cambridge. Revised by E. D. Adrain, M.B., M.R.C.P., Fellow of Trinity College, Cambridge. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta and Madras, 1917. (Pp. xi + 102, with diagrams.) Price 5*s*. net.

The King's Fishing. Done into Verse. By Charles Mercier, M.C.C. With Notes Critical and Explanatory. London: The Mental Culture Enterprise, 329, High Holborn, W.C., 1917. (Pp. 50.) Price 1*s*. 3*d*. net.

Jacob and the Mandrakes. By J. C. Frazer. The *British Academy Proceedings*, Vol VIII. London: Published for the British Academy by Humphrey Milford, Oxford University Press, Amen Corner, E.C. (Pp. 23.) Price 2*s*. 6*d*. net.

Les Études de la Guerre. Publiées sous la direction de René Puaux. Cahier I. Septembre 1917. Paris: Librairie Payot et Cie, 106, Boulevard St. Germain. (Pp. 76.) Price 1 fr. 50 c.

Industrial Alcohol. By Robert N. Tweedy. Dublin: The Co-operative Reference Library, The Plunkett House, 1917 (Pp. 88.) Price 1*s*. net.

An Ethical System. Based on the Laws of Nature By M. Deshumbert. Translated from the French by Lionel Giles, M.A., D.Litt., with a Preface by C. W. Saleeby, M.D., F.R.S. Edin. Chicago and London: The Open Court Publishing Company, 1917. (Pp. ix + 231.). Price 2*s*. 6*d*. net.

The Year-Book of the Universities of the Empire 1916-1917. Published for the Universities' Bureau of the British Empire. London: Herbert Jenkins, Ltd., 1, Arundel Place, Haymarket. (Pp. xiii + 492.) Price 7*s*. 6*d*. net.

Telepathy Genuine and Fraudulent. By W. W. Gaggally, Member of the Council of the Society for Psychical Research, with a Preface by Sir Oliver Lodge, F.R.S. London: Methuen & Co., 36, Essex Street, W.C. (Pp. 94.) Price 2*s*. 6*d*. net.

Locke's Theory of Knowledge and its Historical Relations. By James Gibson, M.A., Professor of Logic and Philosophy in the University College of North Wales; late Fellow of St. John's College, Cambridge. Cambridge: at the University Press, 1917. (Pp. xiv + 338.) Price 10*s*. 6*d*. net.

Printed by Hazell, Watson & Viney, Ld., London and Aylesbury, England.

RECENT ADVANCES IN SCIENCE

MATHEMATICS. By PHILIP E. B. JOURDAIN, M.A., Cambridge.

Various News and Educational Questions.—Giuseppe Veronese of Padua died on July 17, 1917, at the age of sixty-three. His best known work is concerned with the foundations of geometry, and, on the still somewhat obscure question as to the existence or not of infinitesimals and his "infinite numbers," he had controversies with Georg Cantor, Peano, and others.

Until the war is over, the Circolo Matematico of Palermo has suspended the publication of its *Rendiconti*.

The De Morgan medal for 1917 was awarded by the London Mathematical Society to W. H. Young for his eminent work in mathematics; the Bordin prize of the Paris Academy of Sciences for 1917 was awarded to Gaston Julia for his memoir on the arithmetical theory of non-quadratic forms; and the Alfred Ackermann-Teubner prize for progress in mathematics was awarded in 1916 by the University of Leipzig to Ernst Zermelo of Zürich for his work (1904, 1907) on the theory of aggregates.

Some account of the Mittag-Leffler Institute is given elsewhere in this number of SCIENCE PROGRESS.

The French Mathematical Society has decided to accept articles written in English and Italian as well as those in French (*Amer. Math. Monthly*, 1917, **24**, 444).

In the *Monist* for October 1917 (**27**, 618–39) is a large collection of notes on recent work in the philosophy of science, in which work on the philosophy and history of mathematics has a large place.

The number of the *American Mathematical Monthly* for October 1917 contains much of interest to teachers of mathematics. In the first place, there is a full account of the second summer meeting of the Mathematical Association of America and the meeting of the Council, containing abstracts of papers read (**24**, 353–62); and, in the second place, there are articles

on "Algebra Courses for College Juniors and Seniors" (ed. U. G. Mitchell, *ibid.* 362–8), "A List of Mathematical Books for Schools and Colleges" (Library Committee, W. B. Ford, Chairman, *ibid.* 368–76), and "Discussion relating to Required Mathematics for Women" (Emilie N. Martin, *ibid.* 394–8) while Arnold Emch (*ibid.* 379–82) shows the importance of perspective as an introduction to projective geometry.

In an earlier number, J. A. Nyberg (*ibid.* 309–12) shows how the notion of function can be developed in the first year of college teaching, both briefly and "so thoroughly that the student will think of it as something else besides axes, co-ordinates, and curves, and so comprehensively that the work will be useful regardless of whether the course is trigonometry, algebra, or analytics." Into the foreground is brought the idea of relationship between variables, and only after the notion of function and correspondence is grasped would Nyberg explain the language of co-ordinates. Nyberg further (*ibid.* 406–9) begins the study of the line, and closes with some illustrations of how the new point of view eliminates certain difficulties.

History.—Gino Loria (*Rend. del Seminario Mat. della Facoltà di Sci. della R. Univ. di Roma*, 1917, **4**) gives an excellent and detailed sketch of mathematics in Japan as shown by recent literature, together with several notes containing mathematical developments in our usual notation.

D. E. Smith (*Bull. Amer. Math. Soc.* 1917, **24**, 82–96) gives an account of the cryptographic work of John Wallis, partly because his biographies say little about it, "partly because of the interest naturally excited by the present war, but chiefly because of the new light that certain letters, hitherto unpublished, throw upon the life and character of a mathematician of merit."

J. M. Child (*Monist*, 1917, **27**, 411–54) gives the second and last part of his annotated translation of the manuscripts of Leibniz on his discovery of the differential calculus, as made known by the publications of Gerhardt in 1846, 1848, and 1855. Child also (*ibid.* 524–59) gives an annotated translation of Gerhardt's paper of 1891 on "Leibniz in London."

S. A. Joffe (*Trans. Actuarial Soc. of America*, 1917, **18**, 72–98) gives a systematic review of the important contributions to the theory of formulæ of interpolation, especially

central differences, the developments being traced in their chronological order beginning with Newton.

A useful set of notes on recent publications by A. Favaro, J. Ginsburg, W. H. Bussey, B. Petronievics, F. Cajori, E. Millosevich, L. Viriglio, D. E. Smith, O. Zanotti-Bianco, G. Milhaud, H. Suter, C. Tweedie, G. Bigourdan, G. Vacca, and L. A. Perriere de Silva are given by G. Loria (*Boll. di bibl. e st. delle sci. mat.* 1917, **19**, 88–94).

Some further letters that passed between Cremona and Schläfli from 1872 to 1887 are given by J. H. Graf (*ibid.* 65–73; cf. SCIENCE PROGRESS, 1918, **12**, 363).

Logic and Principles of Mathematics.—C. E. Van Horn (*Proc. Camb. Phil. Soc.* 1917, **19**, 22–31) makes use of the new function of propositions introduced into logic by Sheffer in 1913, and by which the other functions (negation, disjunction, implication, and conjunction) may be defined, to reduce the number of primitive propositions in symbolic logic. However, some steps in his work are unsatisfactory, as was pointed out by J. G. P. Nicod at the end of his paper next mentioned. Nicod (*ibid.* 32–41) is concerned independently with exactly the same problem as Van Horn, but the subject is dealt with much more completely and in a way that seems free from objection. The eight primitive proportions of the theory of implication given in Whitehead and Russell's *Principia Mathematica* are reduced, by the help of Sheffer's new primitive idea, to three—two non-formal and one formal. The formal proposition is very complicated in statement.

J. B. Shaw (*Monist*, 1916, **26**, 397–414), in an attack upon mathematical logic, gives yet another illustration of what seems to be the common feature of all such attacks, viz. the inability to distinguish between the collection of truths discovered in logic or mathematics—and which of course is not created by our "intuition"—and the process by which we discover these truths—in which of course "intuition" is used. Some of the errors and irrelevances in Shaw's article are pointed out by Philip E. B. Jourdain (*ibid.* 1917, **27**, 460–67).

L. S. Hill (*Amer. Math. Monthly*, 1917, **24**, 345–8) gives an example of a continuous series widely different from those discussed by Huntington in his book on the *Continuum* (reviewed in SCIENCE PROGRESS, 1918, **12**, 516).

T. Levi-Civita's five papers of 1917 from the *Rendiconti* of

Rome and Palermo and the *Atti* of Padua on the new mathematical physics of Einstein and Minkowski were noticed and commented upon by G. B. Mathews in *Nature* (1917, **100**, 155). R. I. Pocock (*Journ. Indian Math. Soc.* 1917, **9**, 202–3) gives a short and useful account of astronomy and the theory of relativity, with references to work by de Sitter, Silberstein, Eddington, and Wilson. C. E. Weatherburn (*Proc. Camb. Phil. Soc.* 1917, **19**, 72–85) works out much of the theory of hydrodynamics from the point of view of the theory of relativity.

A paper of importance in connection with the foundations of the theory of errors is that by L. Becker (*Proc. Roy. Soc. Edinburgh*, 1917, **37**, 210–14). The author shows that, in a certain case, we do not find to be borne out by observation the usual assumption that the arithmetical mean of similar meteorological observations is to be regarded as their representative value.

Arithmetic and Theory of Numbers.—F. Robbins (*Trans. Roy. Soc. Edinburgh*, 1917, **52**, 167–74) gives some tables of factorials and allied products together with their logarithms. The logarithms are given with eighteen decimal places, and " when any one of the . . . tables has been given in the past to an extent useful at the present time, it will be found by the computer that the necessary volume is only with difficulty accessible and hardly ever to be purchased."

H. C. Pocklington (*Proc. Camb. Phil. Soc.* 1917, **19**, 57–9) develops the direct method of solving quadratic and cubic binomial congruences with prime moduli.

H. Todd (*ibid.* 111–16) gives a new and elementary proof of a particular case of the famous theorem of Dirichlet on the unities of an algebraic corpus. A short introduction is given by H. T. J. Norton explaining the relation of Todd's argument to the theory of algebraic numbers.

L. J. Mordell (*ibid.* 117–24) finds that the expansions of S. Ramanujan (*Trans. Camb. Phil. Soc.* 1916, **22**) are simple consequences of the properties of modular functions.

H. T. J. Norton (*Proc. Lond. Math. Soc.* 1917, **16**, 294–300) obtains a result in Diophantine approximation which completes a theorem in G. H. Hardy and J. E. Littlewood's paper on the subject published recently in *Acta Mathematica*.

G. H. Hardy and J. E. Littlewood (*Acta. Math.* 1917, **41**, 119–96) give a full account of contributions, principally of 1914

and 1915, to the theory of Riemann's Zeta function and the theory of the distribution of prime numbers. "Our answers to these questions are naturally tentative and fragmentary. The importance and difficulty of the problems dealt with should be a sufficient apology for the incompleteness and miscellaneous character of the results."

F. Hallberg (*Journ. Indian Math. Soc.* 1917, **9**, 174–86) writes on infinite series and arithmetical functions, a subject which is connected with the Zeta function.

Algebra.—W. H. Metzler (*Proc. Roy. Soc. Edinburgh*, 1917, **37**, 324–6) extends so as to involve k instead of two determinants a theorem given by Muir in 1888 on the equality of the sums of two sets of determinants formed in a certain way out of two given determinants.

Olive C. Hazlett (*Trans. Amer. Math. Soc.* 1917, **18**, 167–76) considers linear associative division algebras over a general algebraic field F, which may be described as sets of numbers satisfying all the conditions for a field except that multiplication is not necessarily commutative.

O. E. Glenn (*ibid.* 443–62) generalises the theory of associated forms founded by Hermite in 1856 and developed by Clebsch (1872), Sylvester, and many others.

W. A. Manning (*ibid.* 463–79) gives a much wider generalisation of Bochert's theorem (1892, 1897) connecting degree and class of a multiply transitive group. It may be noticed that, since in the theory of groups of substitutions the two problems of the order and of the class of groups of given degree are in large measure interchangeable, Bochert's inequalities, which limit the degree of a multiply transitive group in terms of its class, are second in importance only to Sylow's theorem.

Analysis.—In 1903 G. H. Hardy proved a theorem about the convergence of certain multiple series; and he now (*Proc. Camb. Phil. Soc.* 1917, **19**, 86–95) states and proves the leading theorems of a more general class of such series in a form more systematic and general than has been given to them before. In the first place, some known theorems concerning simply infinite series are recapitulated with certain changes of form, and then the corresponding theorems for double series are obtained in a form as closely analogous as possible. The further simple generalisation to multiple series in general is left to the reader.

The first number of the fourth volume of *The Rice Institute Pamphlet* (January 1917) contains two lectures by Emile Borel on "Aggregates of Zero Measure" (pp. 1–21) and "Monogenic Uniform Non-Analytic Functions" (pp. 22–52), and two lectures by Vito Volterra on "The Generalisation of Analytic Functions" (pp. 53–101) and "On the Theory of Waves and Green's Method" (pp. 102–17); all delivered at the inauguration of the Rice Institute at Houston, Texas. The aggregates mentioned in the first lecture of Borel play a very important part in the theory of functions of a real and of a complex variable, and a way of comparing such aggregates among themselves is given. Of the results of the work described in his second lecture, Borel says (p. 51): "The results we are establishing suppress the absolutely sharp demarcation established by Weierstrass's theory between real analytic functions and real non-analytic functions"; and then insists on the importance of this from the point of view of the relations between mathematics and physics. Volterra's generalisation is that which has already been the subject of several investigations of his, but here it is his purpose to consider the general case in some detail, beginning with the first foundations. Thus he considers his "functions of hyperspaces," which represent extensions of the functions of curves that he has already treated several times, and extends to these functions the fundamental concepts of continuity and differentiation. Proceeding in this way he finally develops the operations of differentiation and integration and the extension of Cauchy's theorem in complete generality.

In connection with Van Vleck's interesting remark (cf. SCIENCE PROGRESS, 1917, **12**, 366) that a Lebesgue integral is expressible as one of Stieltjes by a fairly simple transformation, the lecture by G. A. Bliss (*Bull. Amer. Math. Soc.* 1917, **24**, 1–47) on Lebesgue integrals and the chief results of the works of other mathematicians on analogous subjects will be found very useful. Bliss points out, with Volterra, that a rapid development is taking place in our notions of infinite processes, examples of which are the definite integral limit, the solution of integral equations, and the transition from functions of a finite number of variables to functions of lines, and gives a full account of the work on integration of Borel, Lebesgue, Radon (1913), de la Vallée Poussin, and others.

W. H. Young (*Proc. Lond. Math. Soc.* 1917, **16**, 273–93) proves the general theorem of integration by parts in a form which takes account of recent progress in the theory of integration, and deduces from it the second theorem of the mean in an extended form and for any number of variables.

G. Pólya (*Acta Math.* 1917, **41**, 99–118) investigates those power-series whose circles of convergence are natural limits. The subject has been hitherto made obscure by the too frequent use of the phrase that a power-series cannot " in general " be continued beyond its circle of convergence : of course both continuable and non-continuable power-series have the same cardinal number. Pólya considers a functional space of infinitely many dimensions whose points are the power-series converging in the unit circle, and proves the theorem : the aggregate of non-continuable power-series has only inner points and is everywhere dense ; while the aggregate of continuable power-series is nowhere dense and perfect.

G. H. Hardy (*Proc. Camb. Phil. Soc.* 1917, **19**, 60–33) generalises a theorem published by Pólya in 1915, on transcendental whole functions which assume integral values for all positive integral values of the variable, by a slight modification of Pólya's own argument, without the addition of any essentially new idea to those which he employed.

A. Wiman (*Acta Math.* 1916, **41**, 1–28) investigates the connection between the maximum $M(r)$ of the modulus of an analytic function and the maximum of the real part, for a given argument of the function, and very greatly generalises a result of Borel (1897) for whole functions. Wiman shows that light is thrown by it both on the famous theorem of Picard and on the properties of the solutions of differential equations in the neighbourhood of points of indefiniteness.

K. B. Madhava (*Journ. Indian Math. Soc.* 1917, **9**, 186–201) gives an account, with many references, of asymptotic expansions of integral functions.

G. N. Watson (*Proc. Camb. Phil. Soc.* 1917, **19**, 42–8) proves rigidly the formulæ of approximations for (1) a Bessel function of equal and large order and argument, and (2) its first derivative, which are due to Cauchy (1854), Graf and Gubler (1898), Debye (1909), and others, without the use of contour integration, on the one hand, and without appealing to physical arguments such as Kelvin's (1887) " principle of stationary phase," on

the other. Watson (*ibid.* 49–55) discusses the limits of applicability of Kelvin's principle, giving a formal analytical proof of a theorem slightly more general than Kelvin's theorem on an asymptotic formula for a certain integral. Watson (*ibid.* 96–110) obtains aproximate formulæ which exhibit the behaviour of a Bessel function when its order is large, right through the transition stages between the domains of validity of the three known formulæ for the dominant terms of the asymptotic expansions of the function for various ratios of the argument to the order of the function. His formulæ are more exact forms of some approximations obtained by Nicholson in 1910.

F. J. W. Whipple (*Proc. Lond. Math. Soc.* 1917, **16**, 301–14) obtains a certain relation between Legendre's P and Q functions with parameters $\cosh a$ and $\coth a$, which explains the symmetry of various parts of the theory of Legendre's functions. Its geometrical interpretation is found in the applications of inversion to potential problems connected with toroidal co-ordinates.

M. Bôcher (*Trans. Amer. Math. Soc.* 1917, **18**, 519–21) gives a note supplementary to his paper of 1901 on certain pairs of transcendental functions whose roots separate each other, and which contains a further general theorem discovered since the former paper was published.

F. H. Safford (*Bull. Amer. Math. Soc.* 1917, **24**, 74–6) obtains a new method of reducing the general elliptic element to the Weierstrassian form, which is allied to the formula published by G. G. A. Biermann in 1865 as derived from Weierstrass's lectures.

Dunham Jackson (*ibid.* 77–82) proves, by a method different from that of Fréchet in his paper (1906) on the functional calculus, that a suitable change of parameters in the representation of an arbitrary continuous curve, $x=f(t)$, $y=\phi(t)$, will eliminate the intervals where f and ϕ are constant together.

E. W. Chittenden (*Trans. Amer. Math. Soc.* 1917, **18**, 161–6) shows the correctness of the equivalence conjectured by Fréchet (1910) of his "*écart*" and "*voisinage.*"

F. L. Hitchcock (*Proc. Roy. Soc. Edinburgh,* 1917, **37**, 250–55) examines and classifies the various cases in which we can obtain a solution of Tait's functional equation $\phi^2=\varpi$, where ϖ is a given linear vector function and ϕ is to be found.

This paper is related to a paper that Hitchcock published in 1915 (*ibid.* **35**, 170–80; SCIENCE PROGRESS, 1918, **12**, 369).

C. E. Wilder (*Trans. Amer. Math. Soc.* 1917, **18**, 415–42) carries out the suggestion of Bôcher (1913) that Birkhoff's (1908) results on the boundary value and expansion problems for the ordinary linear differential equation of the nth order with boundary conditions at two points should be generalised to the equation with auxiliary conditions at more than two points, and gives the proof of the convergence of the expansion, which may be studied quite independently of the other results. The formal development of the boundary problem and a more detailed discussion of the form of series will be presented by Wilder in other papers.

J. Chazy (*Acta Math.* 1916, **41**, 29–69) extends to the complex domain and to differential equations of any order the results of Bendixson and Picard on the integrals of the equation $x^n\, dy/dx=F(x, y)$ for small real values of x and y, where n is an integer greater than 1 and $F(x, y)$ is holomorphic and zero when both x and y are zero; and gives a further generalisation and application to a case of the problem of n bodies. Then Chazy gives part of his important memoir of 1912, to which a prize was awarded by the Paris Academy of Sciences, on certain differential equations of the third and higher orders, and shows that the integrals have transcendental singularities, and are not one-valued. The results obtained in the first part are here used.

W. L. Hart (*Trans. Amer. Math. Soc.* 1917, **18**, 125–60) develops certain theorems concerning a type of real valued functions of infinitely many real variables; he then considers the problem of infinite systems of corresponding ordinary differential equations; and finally discusses the fundamental problem of implicit function-theory in this field. The results of all three sections of the paper include as special cases the corresponding theorems on functions of a finite number of variables.

G. D. Birkhoff (*ibid.* 199–300) gives an exposition of his advances in the treatment of dynamical systems with two degrees of freedom, which constitute the simplest type of non-integrable problems. The researches of Hill, Poincaré, Hadamard, Levi-Civita, and others have thrown great light upon the subject of such systems.

M. J. M. Hill (*Proc. Lond. Math. Soc.* 1917, **16**, 219–72) gives a classification of the integrals of linear partial differential equations of the first order according to the values of the Jacobian.

R. G. D. Richardson (*Trans. Amer. Math. Soc.* 1917, **18**, 489–518) develops a new method by which the well-known results about boundary problems for the ordinary and elliptic partial differential equations can be deduced, and then attempts an application of this method to an investigation of the facts concerning the new boundary problem for the hyperbolic equation.

G. A. Pfeiffer (*ibid.* 185–98) shows the existence of divergent solutions of the functional equation which is fundamental in the problem of the conformal mapping of a curvilinear angle, and shows that these solutions have a significance inherent to the mapping problem referred to.

F. Riesz (*Acta Math.* 1916, **41**, 71–98) considers the inverse problem for a certain class of linear transformations of continuous functions, and also an application to Fredholm's integral equation. The object of the paper is not so much to obtain new results about linear functional equations as to test an exceedingly elementary method. By far the most important conception used is one introduced by Fréchet, and the limitation to continuous functions is not essential in the paper.

Geometry.—J. R. Kline (*Trans. Amer. Math. Soc.* 1917, **18**, 177–84) shows that the converse of the theorem of R. L. Moore (*ibid.* 1916, **17**, 132–64) concerning the division of a plane by an open curve holds in spaces satisfying a certain set of axioms formulated by Moore, and this in certain spaces which are neither metrical, descriptive, nor separable. The converse theorem of the analogous theorem of Jordan (1893) for simple closed curves was first formulated by Schoenflies in 1902, who made use of metrical properties in his proof. A different proof was given by Lennes in 1911, and this proof was commented upon by Moore.

A. J. Kempner (*Amer. Math. Monthly*, 1917, **24**, 317–21) gives, after A. Reymond (1916), a simple graphical method of determining whether a given integer is a prime number or not. This simple relation between projective geometry and the theory of numbers is, says Kempner, really only an analogy without deeper import.

R. A. Johnson (*ibid.* 313–17) applies the method of directed angles which he used earlier in 1917 to the theory of inversion, especially to the use of this theory in studying properties of the triangle. None of the theorems which he obtains is new, but in most cases the form of presentation is new and has certain other advantages.

O. J. Peterson (*ibid.* 376–9) proves, without the use of Plücker's formulæ, the fundamental theorem of Clebsch (1864) that every rational curve of the nth order has $\frac{1}{2}(n-1)(n-2)$ double points. In all his paper, Peterson assumes that all singular points which occur are either nodes, conjugate points, or cusps. He then shows that the upper limit of the number of cusps is $\frac{3}{2}(n-2)$.

G. M. Green (*Trans. Amer. Math. Soc.* 1917, **18**, 480–88) gives two geometric characterisations of isothermal nets on a closed surface; and P. F. Smith (*ibid.* 522–40) gives a theorem for space analogous to Cesàro's theorem in his *Geometria intrinseca* of 1896 for plane isogonal systems.

ASTRONOMY. By H. Spencer Jones, M.A., B.Sc., Royal Observatory, Greenwich.

The Galactic Distribution of the Stars.—Until recently, our knowledge of the galactic condensation of stars of different magnitudes was one of extreme uncertainty. Whilst all investigators agreed that stars of all magnitudes were condensed towards the galaxy, widely differing values were obtained by various investigators of the amount of this condensation and of its rate of variation with magnitude. Three papers recently published may be said to have removed this uncertainty, and to have settled beyond dispute the order of magnitude of these fundamental data of stellar distribution; these papers are by van Rhijn (*Groningen Publications*, No. 27, 1917), Seares (*Astroph. Journ.* **46**, 117, 1917), and Chapman (*M.N., R.A.S.* **78**, 66, 1917). The two principal previous determinations were by Kapteyn (*Groningen Pubs.* No. 18, 1908) and by Chapman and Melotte (*Mems. R.A.S.* **60**, Pt. 4, 1914), the latter being based mainly upon counts of photographic magnitudes on the Franklin-Adams chart plates, a series of plates on a uniform scale covering the whole sky. A number of regions on each plate were counted. Kapteyn's results were based on counts of visual magnitudes. Measuring the galactic

condensation by the ratio of the number of stars in a belt of 20° on either side of the galactic plane to the number in a spherical cap of 50° radius around the galactic plane, Kapteyn obtained condensations of 1·8 at 5^m, 2·6 at 10^m, and 10·4 at 16^m. Chapman and Melotte obtained 2·1 at 5^m, and 3·1 at 16^m, a very different rate of progression; it did not seem possible to reconcile the two results by taking into consideration that Kapteyn's scale was not an absolute one and that one result was based on visual and the other on photographic magnitudes.

Dr. van Rhijn's results are based upon counts of 65 Northern Durchmusterung plates of the Selected Areas of Kapteyn, taken at Harvard Observatory. Prof. Seares has used the counts made by Prof. Turner of the number of stars of different magnitudes in various zones of the Astragraphic Catalogue; these counts refer to definite but unknown limits of magnitude, the limits being transferred to a numerical scale by comparison with Kapteyn's tables. He has also made counts on 88 plates taken at Mt. Wilson of the Selected Areas, of which only a preliminary account has so far been published (*Proc. Nat. Acad. Sci.* **8**, 217, 1917). These results are in close accord with Kapteyn's, particularly when the magnitude scale used by Kapteyn is reduced to an absolute scale. Thus at 16^m, van Rhijn obtains a condensation of 5·5, Kapteyn (revised) of 6·3; at $17^m.6$, Seares obtains 10·2, Kapteyn 11·0. At the same time, van Rhijn has pointed out an error in the method of reduction used by Chapman and Melotte, the harmonic mean of the star densities actually being obtained by the method adopted, instead of the arithmetic mean; Dr. Chapman has admitted this error, and in the paper referred to above revises his results accordingly; the corrected values agree closely with those obtained in the other investigations. We have therefore now three determinations, all based upon photographic magnitudes, which agree closely, so that the galactic condensation and progression with magnitude may be said to be well determined.

By extrapolation from the counts, the total number of stars in our stellar universe can be estimated, assuming that there is no scattering of light in interstellar space. This number is found to be alout 3,000 millions, of which one-half are brighter and one-half fainter than $25^m.5$.

Distances of Spiral Nebulæ.—The recent discovery of a "nova" or new star in the nebula N.G.C. 6946 started a search at the Mt. Wilson and Lick Observatories to ascertain whether other novæ in spiral nebulæ could not be found by comparing old negatives of the nebulæ with others of more recent date. The result has been that several other instances of novæ occurring in spiral nebulæ have been found, whilst previous to the discovery of the nova on N.G.C. 6946, only two instances were known. There seems to be little doubt that these novæ are actually associated with the spirals in whose photographs they have been detected, and that they are not seen simply projected upon the nebulæ, for the novæ unconnected with spirals only occur close to the galaxy, whereas spiral nebulæ are most frequent away from the galaxy. This fact has been used by H. Shapley (*Pub. Ast. Soc. Pac.* **39**, 213, 1917) and H. D. Curtis (*Lick Obs. Bull.* No. 300) to derive estimates as to the distances and dimensions of the nebulæ. A comparison of the mean apparent magnitudes of the novæ associated with nebulæ and those associated with our galactic system shows that the former are about 10^m fainter; assuming that both classes of novæ are similar objects, and therefore equally bright on the average, the spiral nebulæ in which novæ have been observed (probably the nearer spirals) are about 100 times as distant as the galactic novæ. The latter are probably several times as distant as B-type stars of the same magnitude, whence it follows that the distance of the spirals is of the order of not less than 100,000 light-years. This great distance is confirmed by other reasoning: the galactic novæ probably increase in brightness about 16^m, so that the novæ in spirals would be about 30^m before their outburst. The stars of 15^m in our galaxy are about 20,000 light-years away, and to appear of 30^m would have to be removed to a distance of about 20 million light-years. Assuming the two classes of stars to be of the same type, this will be the distance of the novæ. Moreover, since even the most powerful telescopes do not resolve the spirals into discreet stars, all the stars in them must be fainter than 21^m, so that, as a lower limit to their distance, we again deduce several million light-years.

This evidence seems conclusive that such nebulæ are other universes, quite separate from our own stellar universe. A

nebula at a distance of 20 million light-years, and with an apparent diameter of 10′, would actually have a diameter of 60,000 light-years, which is of the order of magnitude of our own system.

Distances of Stellar Clusters.—As in the case of spiral nebulæ, our knowledge of the distances of star clusters has to be obtained indirectly; far more confidence can, however, be placed in the indirect estimates than in any direct measurements, where such distant objects are concerned. H. Shapley (*Proc. Nat. Acad. Sci.* **3**, 479, 1917) has made some interesting estimates. He shows that the extremes of range of variation of the Cepheid type of variable stars in various clusters are about the same, and further that in the same cluster the magnitudes of all Cepheid type variables closely agree. It is believed that the intrinsic magnitudes of all Cepheid stars are about the same wherever the stars occur, and a means is thus provided of estimating the distance of the clusters in which they occur. Shapley thus finds parallaxes for several clusters which average about $0''\cdot0001$, corresponding to a distance of about 30,000 light-years. He further finds that the difference between the mean magnitude of the 25 brightest stars in a cluster and that of the Cepheid variables in it is about $1^m.35$ for all clusters examined; this result can be used to estimate the mean magnitude of the Cepheids in clusters in which none have been observed and hence to obtain an estimate of the distances of such clusters by the preceding.

The Threshold of Vision.—Three determinations of the minimum light perceptible to the human eye have been made recently, by H. E. Ives, *Astroph. Journ.* **44**, 124, 1910; H. N. Russell, *ibid.* **45**, 60, 1917; and Prentice Reeves, *ibid.* **46**, 167, 1917, respectively. The first two of these determinations were based upon the magnitude of the faintest visible stellar object and on the pupillary area of the eye when completely adapted to darkness. The most direct and accurate determination of the latter quantity is by the method used recently by W. H. Steavenson, in which flashlight photographs are taken of the eye when accommodated to darkness. He thus obtained a value for the diameter of the pupil of about 8·5 mm.—considerably larger than previous estimates. This result was not available to Ives, who used too small a value for the diameter; he further assumed that the faintest visible

star is of the sixth magnitude. This is certainly over-estimating the limit of brightness of the faintest star visible on the background of the sky, and on a perfectly dark background still fainter objects will be visible. Russell estimated that on a perfectly dark background the limiting magnitude would be $8^m.5$. He further used Steavenson's determination of the diameter of the pupil. The determination by Reeves of the *minimum visibile* is more direct and therefore probably also more accurate; he used an artificial star behind a neutral dyed gelatine wedge, the position of which could be varied, and the proportion of light transmitted calculated. His value corresponds to a star of magnitude $7^m.4$. He redetermined the diameter of the pupil by Steavenson's method, and obtained a slightly smaller value, which was liable to variation for different observers and according to the state of fatigue of the eye and the general bodily health of the observer. The final results of the three determinations are:

	Area of pupil. Sq. cm.	Minimum perceptible energy entering eye. Ergs per sec.
Ives	0·28	$38·0 \times 10^{-10}$
Russell . . .	0·57	$7·7 \times 10^{-10}$
Reeves . . .	0·51	$18·9 \times 10^{-10}$

It is not stated by Reeves whether direct or averted vision was used in his determination. For barely perceptible objects, vision is mainly by the rods of the retina and the retina is most sensitive about midway between its edge and the fovea centralis; an object which is visible by averted vision may therefore not be visible by direct vision. The smaller value obtained by Russell seems to point to his estimate of $8^m.5$ being too faint.

A further note on the same subject by Henri Buisson has recently appeared in the *Astroph. Journ.* **46**, 296, 1917. He determined the brightness of the faintest visible object by employing a screen rendered faintly luminous by radium paint, the colour of which corresponds nearly to the maximum of sensibility of the eye in the spectrum. The screen was standardised by comparison with a film diffusing by transmission. The results give for the limit of visibility a star of magnitude 8^m. Using the values for the area of the pupil adopted by Russell and Reeves respectively, the values obtained on this basis for the minimum perceptible energy

entering the eye are 12.5×10^{-10} and 11.2×10^{-10} ergs per sec., approximately a mean of the results obtained by Russell and Reeves. Part of the difference between the various results is no doubt due to the eyes of different observers not being of the same sensitiveness.

It is interesting to note that if the limit of visibility is a star of magnitude 8^m, the maximum distance at which a standard candle is visible in perfectly transparent air is about 27 km.

Comparative Climatology.—A valuable discussion of the relative merits of different regions as regards their suitability for white settlement has been made by Dr. Griffith Taylor (Commonwealth Meteorological Bureau, Melbourne, ***Bulletin*** No. 14). The basis of such a discussion must obviously be a knowledge of the various meteorological elements at the various centres, for it is primarily upon these that the health and comfort of the human race depend. It is therefore necessary to combine these elements in such a way as to obtain a rapid and accurate criterion of the suitability of any centre. Dr. Taylor finds that the most suitable elements to use are the wet-bulb temperatures and the relative humidities. The monthly means of these quantities are plotted against rectangular co-ordinates and a polygon formed by joining up the twelve points. This figure is called a "climograph" and presents a large amount of information at a glance. The mean of the climographs for twelve important centres of Anglo-Saxon settlement gives a standard figure, with which any other climograph may be compared. Its limits are: summer, (wet bulb) 62° F., humidity 68·5 per cent.; winter, (wet bulb) 37° F., humidity 81 per cent. With this figure, typical climographs for a large number of stations in many different regions are compared. Dr. Taylor hopes that this investigation may serve as "a scientific basis to the climatic aspect of Empire-building," and has discussed in detail in a separate memoir its application to Tropical Australia. For this memoir he has been awarded the Gold Medal of the Royal Geographical Society of Queensland.

The following is a selection from amongst the most important papers recently published:

Theory of Errors.—SCHLESINGER, F., On the Errors in the

Sum of a Number of Tabular Quantities, *Ast. Journ.* **30**, No. 21, 1917.

The Solar System.—LAU, H. E., Saturn und sein Ring, *Ast. Nach.* No. 4906.

WILKENS, A., Methoden zur Ermittelung der speziellen und der absoluten Koordinatenstörungen der Planeten der Jupitergruppe durch Jupiter, *Ast. Nach.* No. 4906.

CHEVALIER, S., Diamètre et Forme du Disque Lunaire, *Bull. Ast.* **34**, 161, 1917.

Spectroscopy.—FOWLER, A., AND STRUTT, HON. R. J., Absorption Bands of Atmospheric Ozone in the Spectra of Sun and Stars, *Proc. R.S.* (A) **93**, 577, 1917.

ST. JOHN, C. E., AND BABCOCK, H. D., Elimination of the Pole Effect from the Source for Secondary Standards of Wave-Length, *Astroph. Journ.* **46**, 138, 1917.

LUNDMARK, K., AND LINDBLAD, B., Photographisch effektive Wellenlängen für einige Spiralnebel und Sternhaufen, *Ast. Nach.* No. 4907, 1907.

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PHYSICS. By JAMES RICE, M.A., University, Liverpool.

Spectroscopy.—In the *Phil. Mag.* for October, Mr. Hemsalech describes experiments on the origin of the line spectrum emitted by iron vapour in the explosion region of the air-coal gas flame. The chemical actions which underlie the emission of the cone spectrum of iron have no connection with those underlying the emission of the carbon bands. Nitrogen is one of the determining factors in the emission of the cone spectrum and a hypothesis is advanced based on the assumption that a nitride of iron is formed in the explosion region. It is shown that the presence of oxygen in the medium surrounding the air-coal gas flame is essential in bringing about the formation of iron nitride. No definite knowledge has been gained whether the spectrum is actually due to the formation of the nitride or to its subsequent decomposition; but it is possible that the spectrum is the result of the disruptive break-up of the molecules of the iron chloride, oxide, etc., fed into the flame, a break-up which could be brought about or at least facilitated by the existence of a strong chemical affinity between the nitrogen and the metal atom.

In the same number Prof. Richardson and Lieut. Bazzoni give an account of investigations undertaken for the purpose of detecting and measuring the frequency of the shortest vibrations emitted from various gas atoms under electron impacts. Previous investigations by Schumann and Lyman have detected ultra-violet radiation as far down as 900 Ångstrom units, and recently Lyman (*Astrophysical Journal*, March 1916) has published records of work carried on in helium giving definite lines down to about 600 units. The difficulties of working in this region of the spectrum is that not only all radiation in this neighbourhood is absorbed by all solid media (preventing the use of even quartz and fluorite prisms in the spectrograph),

but also the radiation corresponding to the ionisation potential of a gas may be expected to be highly absorbable by the gas. Thus to detect such radiation it is therefore desirable to avoid the long path through the gas which is demanded by the vacuum grating spectrograph. Further, in dealing with helium it is necessary to have an arrangement in which the gas can be maintained at a very high degree of purity, and on this account the authors of this paper criticise Lyman's most recent work inasmuch as under the conditions of experiment it must have been extremely difficult, if not impossible, to keep the helium free from impurities, especially hydrogen. In the method used by Richardson and Bazzoni, the radiation excited by the ionisation of the gas is allowed to fall on a metallic target and the velocity of the photoelectric electrons emitted is measured. In determining the frequency of the impinging radiation the truth is assumed of the well-known equation

$$\tfrac{1}{2}mv^2 = h\nu - w,$$

where m and v are the mass and maximum velocity of emission of the photoelectrons, h is Planck's constant, and w a known constant for the metal target. Hence ν, the radiation frequency, is calculated. The following results were obtained. By means of large electrons currents under potentials up to 800 volts, helium ionised by electronic impact emitted a radiation the upper limit of which extended to a line in the neighbourhood of 420 Ångstrom units ; hydrogen, a radiation terminating at a wave length close to 900. Mercury vapour gives a spectrum extending to about 1,000 units. These limits seem to be identical with values of the frequencies calculated by Bohr on the basis of his theory of the atoms of hydrogen and helium. Incidentally, the gap between the Schumann region of the spectrum and the softest X-rays has been still further narrowed by this attainment of a line near 420.

In the August number of the *Physical Review*, Messrs. Davis and Goucher describe experiments also bearing on the excitation of radiation by electron impact in hydrogen and mercury vapour, and state that their results, while not inconsistent with the Bohr theory of the atom, are of greater complexity than would be inferred from it.

In the September number of the *Physical Review*, Mr. H.

Nyquist publishes the results of researches on the Stark effect in helium and neon.

In the October number of the *Proc. Roy. Soc.*, Profs. Fowler and Strutt communicate the results of a research showing that the group of six absorption lines discovered by Sir William Huggins in 1890 in the photographic spectrum of Sirius lying near 3,200–3,300 Å. are really of telluric origin and due to the ozone of our atmosphere. The same lines appear in the spectrum of some other stars, and strongly in the spectrum of the sun at low altitude.

X-Rays.—In the *Phil. Mag.* for October there is a paper by Prof. Barkla and Miss White on the absorption of X-rays of short wave length. A theory bearing on the energetics of such absorption has already been given by Prof. Barkla in the Bakerian Lecture for 1916 (see SCIENCE PROGRESS, April 1917). But observations on the intensity of corpuscular radiation from air and such substances as aluminium, water, paraffin, wax, and paper show an irregularity from predicted results only explainable on the assumption that the light elements oxygen, carbon, aluminium emit under suitable stimulus a hitherto unobserved charcterististic radiation—a *J* radiation, harder and of shorter wave length than *K* radiation. In the experiments the radiation incident on the various substances tested was in most cases a primary radiation from an X-ray tube, from which the more easily absorbed constituents had been previously eliminated; in a few cases certain characteristic radiations of the *K* series were used as primary radiations and gave results agreeing perfectly with those obtained with primary radiations direct from an X-ray tube. On plotting the mass absorption coefficients in aluminium, water, paper, and paraffin-wax against those in copper, the curves show the characteristic "kink" so well known in connection with the curves which some years ago brought to light the existence of the *K* and *L* radiations. These "kinks" indicate by their position the existence of *J* radiations from carbon, oxygen, and aluminium whose wave lengths are approximately $\cdot 42 \times 10^{-8}$ cm., $\cdot 39 \times 10^{-8}$ cm., $\cdot 37 \times 10^{-8}$ cm. respectively. Some points of theoretical interest are also dealt with in this paper.

In the November number of the *Physical Review*, C. S. Brainin describes experiments undertaken with the purpose

of studying the total emission of X-rays from certain metals and of determining the range of validity of various theoretical conclusions which state that the energy of emission of the X-rays should be proportional to the atomic weight of the radiating target and also to the square of the voltage between the tube electrodes. The results only partly favour these conclusions. Discrepancies appear generally at the critical voltages for the characteristic radiations of the metal employed as target.

Of the theoretical papers published during the past few months mention should be made of an investigation by Yohhio Ishida (*Phys. Rev.* October) on the kinetic theory of a gas consisting of monatomic molecules with no spherical symmetry during its passage from a variable state to a condition of statistical equilibrium. A paper by H. W. Nicholls on the theory of electrical systems in which such elements as resistance, inductance, and capacity vary according to some general law will be found to be of interest by students of mathematical physics (*Phys. Rev.* August). In the *Proc. Roy. Soc.* December, and *Phil. Mag.* December, H. S. Allen discusses various formulæ connecting characteristic frequency and atomic number.

PHYSICAL CHEMISTRY. By Prof. W. C. McC. Lewis, M.A., D.Sc., University, Liverpool.

The Chemical Theory of Capillarity.—Since the time of Laplace surface tension and capillary effects generally have been treated from the purely physical standpoint. That is, forces of attraction are assumed between volume elements or molecules, the forces being exerted in a uniform manner independent of the direction. The molecule is regarded as a unit from the centre of which the force emanates. The problem resolves into a calculation of the magnitude of such forces expressed as a function of the distance from their origin. The problem of the structure of the interfacial layer separating two homogeneous phases, such as liquid and vapour, has been investigated from this point of view, and a certain amount of progress has been made. The phenomenon of capillarity, however, is by no means well understood. Treated in this manner, the subject becomes one of very great complexity, the theoretical results in many cases

being by no means amenable to experimental test. Less advance has been made indeed in capillarity than in almost any other branch of physics or chemistry. Considerable interest attaches therefore to some quite recent attempts to deal with capillary effects from a new point of view. It is too soon to say whether the new mode of treatment will carry us far enough. It has already accomplished enough, however, to warrant a serious consideration.

The new view, which may be called the " chemical " theory of capillarity, to distinguish it from the purely physical view referred to above, rests on the assumption that capillary effects are essentially due to valency, residual valency. That is, the forces of attraction instead of being uniform in all directions are located in certain regions. These forces extend out from a number of definite positions on a single molecule which is thereby attached in a certain way to neighbouring molecules. This mode of attachment involves a more definite conception of the spatial structure of a molecule than is required by the older physical view, and herein indeed lies one of its principal advantages. The new concept of capillarity has been advanced independently by two American investigators, Langmuir (*Met. Chem. Eng.* **15**, 468, 1916; *Journ. Amer. Chem. Soc.* **39**, 1848, 1917) and Harkins (*Journ. Amer. Chem. Soc.* **39**, 354, 541, 1917). We shall give a very brief summary of some of the results arrived at, following in the main the work of Langmuir.

According to the new view of capillarity the structure of the surface layer of atoms is regarded as the principal factor in determining the magnitude of the surface tension of liquids. The molecules arrange themselves in the surface layer in such a way that their active portions, *i.e.* the portions possessing much residual affinity, are drawn *inwards*, leaving the less active portions of the molecules to form the surface layer. The force is regarded as electromagnetic in nature, and the arrangement is such as to make the stray electromagnetic fields a minimum.

The surface energy of a liquid is thus not a property of the molecule as a whole, but depends only upon the least active portions of the molecules, and the manner in which these are able to arrange themselves in the surface layer.

In liquid hydrocarbons of the aliphatic series the molecules

arrange themselves so that the methyl groups at the ends of the hydrocarbon chains form the surface layer. The surface layer is thus the same no matter how long the hydrocarbon chain may be. As a matter of fact the surface energy of the hydrocarbons from hexane to liquid paraffin have nearly the same surface energy, viz. 46 to 48 ergs per cm^2, although the molecular weights are so different. In the case of the alcohols, it is found that the surface energies are almost identical with those of the hydrocarbons. This would be expected if the surface layer consisted of the alkyl groups, the hydroxyl groups being all directed inward. Oxygen in virtue of its tetravalency confers great activity upon any group into which it enters.

In benzene, the molecules arrange themselves so that the benzene rings lie flat on the surface, the surface energy of benzene being about 65 ergs per cm^2. If an active group, such as OH, replaces one of the H atoms, as in phenol, this group is drawn inwards, with the result that the benzene ring is tilted, thereby raising the surface energy to 75 ergs per cm^2. Thus *any* active group strong enough to tilt the ring raises the surface energy to about 75 ergs per cm^2.

Evidence for this view of capillarity is also obtained by examining the behaviour of oil films upon water. The experiments of Devaux have shown fairly clearly that films which spread out on water do so until the film is just one molecule thick. It is natural to assume that the force which causes the spreading of an oil on water is due to an attraction between the water molecules and a certain part of the oil molecules. It cannot be the entire oil molecule which is involved, as this would lead to solubility and not merely to spreading. There must be some portion of the oil molecule which has an affinity for the water molecules. Let us take the case of oleic acid. There is no doubt but that the carboxyl group has a strong affinity for water, as is shown by the solubility of organic acids in water, and the lack of solubility of the corresponding hydrocarbons. Hence when oleic acid is placed upon water it is probable that the carboxyl groups dissolve, *i.e.* combine with the water. The long hydrocarbon chains have too much attraction for one another, and too little for water to be drawn into solution. By spreading out so as to form a film just one molecule thick *all* the carboxyl groups can combine with water

without causing the hydrocarbon chains to separate from one another.

Since the spreading of an oil is thus due to the existence of an active group, it follows that oils without active groups will not spread on water. This has been found to be the case with hydrocarbon oils, aliphatic and aromatic.

On the above view we conclude that in the case of oleic acid films on water the carboxyl groups are immersed and the hydrocarbon chains stand vertically upward. Acetic acid is readily soluble in water because of the great attraction of the carboxyl group for water which overcomes the attraction of the alkyl groups for one another, these being fewer in number than in the case of oleic acid.

The available data indicate also that a double bond increases the solubility of a compound in water. The double bond is therefore attracted by the water molecules. The carboxyl groups exert, however, a greater attraction upon water, and when the water surface is limited the double bonds are displaced by the carboxyl groups. Following out this line of reasoning, Langmuir has made a number of determinations of the spreading of various substances upon water, from which he has calculated the average cross section of the molecules in a plane parallel to the water surface, and likewise the length of the molecule perpendicular to the surface. A few of the results are as follows:

Substance.	Cross section.	$\sqrt{\text{Cross section.}}$	Length in cms.
Palmitic acid	21×10^{-16}	4.6×10^{-8}	24×10^{-8}
Stearic acid	22	4.7	25
Cerotic acid	25	5.0	31.0
Oleic	46	6.8	11.2
Triolein	126	11.2	13.0
Tristearin	66	8.1	25.0

An examination of the results shows that the cross sections of the molecules vary over quite a wide range, from 21 to 126×10^{-16} cm^2. The three saturated acids, palmitic, stearic, and cerotic, all occupy nearly the same areas, notwithstanding the fact that the number of the carbon atoms increases from 16 to 26. It is also evident that the molecules are much elongated, the length of the palmitic molecule being about 5.2 times its average diameter. The results indicate that the molecules arrange themselves on the surface with their long

dimension vertical as is required by the theory. The molecule of tristearin has the *same* length perpendicular to the surface as the stearic acid molecule, but three times the cross section. Thus each of the three active groups is drawn down to the surface of the water while the hydrocarbon chains are packed in side by side, and are erect upon the surface.

ORGANIC CHEMISTRY. By P. Haas, D.Sc., Ph.D., St. Mary's Hospital Medical School, London.

The anomalous behaviour of the first member of a homologous series is particularly well illustrated by the case of formaldehyde, which in many of its reactions differs entirely from its homologues, and a few recent examples of the study of such reactions deserve notice. Thus Werner (*J. Chem. Soc.* 1917, **111**, 844) has recently been re-investigating the question of methylation by means of formaldehyde with special reference to the mechanism of the interaction of formaldehyde and ammonium chloride. This reaction leading to the formation of methylamine and dimethylamine was previously studied by Brochet and Cambier (*Bull. Soc. Chem.* 1895, [III], **13**, 392) and others who assumed that complex condensation compounds were formed in the first instance, and that these subsequently decomposed into the simple amines. Werner, however, finds that when commercial formalin, containing methyl alcohol, is heated with ammonium chloride a volatile liquid is produced containing methyl formate and methylal, while carbon dioxide is evolved at the same time. The methylal of course is formed by the interaction between the aldehyde and its alcoholic impurity, but the production of methyl formate and carbon dioxide point to the oxidation of some of the aldehyde. The following equations are put forward as a probable explanation of the reactions; the first product methylene amine (I)

$$HCHO + NH_3 . HCl = \underset{(I)}{CH_2 : NH . HCl} + H_2O$$

being an unsaturated compound is readily oxidised at the expense of the water present, which simultaneously reduces another molecule of formaldehyde as follows:

$$CH_2 : NH . HCl + H_2|O + HCHO = CH_3NH_2 . HCl + HCOOH$$

The formation of dimethylamine is accounted for by two similar reactions as follows :

$$HCHO + CH_3NH_2 . HCl = CH_2 : NCH_3 . HCl + H_2O$$

and

$$CH_2 : NCH_3 . HCl + H_2|O + HCHO = (CH_3)_2 : N . HCl + HCOOH$$

When dimethylamine hydrochloride is heated with formaldehyde a stable saturated compound is formed as follows :

$$HCHO + 2NH . (CH_3)_2HCl = CH_2 \begin{cases} N(CH_3)_2 . HCl \\ N(CH_3)_2 . HCl \end{cases} + H_2O$$

This compound having no tendency to become reduced does not attack the water molecule and consequently no formic acid or methyl formate is produced. Experimental details are given for obtaining yields of methylamine and dimethylamine hydrochloride equal to 79·6 and 95·3 per cent. respectively of the weight of ammonium chloride employed.

In a paper entitled " Formaldehyde as a Prototype of Diastase," Gertrud Woker (*Berichte*, 1916, **49**, 2311) recalls the fact that formaldehyde can act as a peroxidase or as a catalase and assumes that these enzymes are probably aldehydic in character, and form additive compounds with hydrogen peroxide as follows :

$$HCHO + H_2O_2 = OH . CH_2O . OH$$

Since diastase can also act as a peroxidase the question arises whether the hydrolytic action of diastase is also due to an aldehyde grouping and further whether formaldehyde may not also be able to hydrolyse starch like diastase by forming a hydrate which would offer the elements of water in an active form. The author has shown that formaldehyde, when present in relatively large quantities, can indeed hydrolyse both starch and glycogen, but that it acts much more readily as a peroxidase than as a hydrolytic enzyme. More recently (*Berichte*, 1917, **50**, 679 and 1188) the same author has again drawn attention to the analogy between formaldehyde and diastase by the following observations : (1) The microscopic appearance of starch granules acted on by diastase and by formaldehyde is the same, staining with iodine showing in both cases unchanged

iodine together with dextrin, (2) formaldehyde and dextrin both liquefy starch, (3) a mixture of starch and formaldehyde has a greater reducing power towards Fehling or Pavy solutions than the same quantity of formaldehyde alone; (4) if a mixture of starch and formaldehyde is dialysed the dialysate contains in addition to formaldehyde a fermentable sugar, possibly a mixture of maltose with isomaltose. Woker and Maggi (*Berichte*, 1917, **50**, 1189) also point out that formaldehyde can apparently act as a hydrogenase since a solution of lead acetate containing formaldehyde and sulphur darkens rapidly at 100° or more slowly if exposed to light at the ordinary temperature; this phenomenon is attributed to a hydrogenation of the sulphur by the formaldehyde. The fact that lead acetate is more soluble in formalin than in water is explained by assuming that formaldehyde combines in some way with lead, after which polymerisation to formose results, and a similar reaction with the magnesium in chlorophyll is supposed to precede the polymerisation of formaldehyde to formose in green plants.

The action of formaldehyde on lactose, maltose, and sucrose has been re-investigated by Heiduschka and Zirkel (*Arch. Pharm.* 1917, **254**, 456); the products obtained when this substance acts upon bioses in aqueous solution may contain up to 39 per cent. of formaldehyde; they are not definite chemical compounds but are most probably solid solutions of formaldehyde in the sugars.

The existence of accessory factors in plant growth similar to vitamines in animal growth is discussed by Rosenheim (*Biochem. J.* 1917, **11**, 7), who has shown that aqueous extracts of Bottomley's bacterised peat have very marked growth-stimulating properties; such extracts give precipitates with phosphotungstic acid which similar extracts from ordinary peat do not give.

The presence of silica in most tissues has been demonstrated by Gonnermann (*Zeitschr. physiol. Chem.* 1917, **99**, 255). Expressed as percentage of total ash the quantities are as follows: Hair, 3–20; milk, 0·3 to 0·4; thymus, 8; adrenals, 7–16; blood corpuscles, 3; serum, 2–3; muscle, 2–4; intestine, 2–13. Good effects appear to have been observed after the administration of silicic acid in cases of tuberculosis, and while most herbs contain considerable quan-

tities of silica it has been found that those herbs which are used by peasants in Middle Europe for the cure of tuberculosis contain the largest quantity of silica.

GEOLOGY. By G. W. TYRRELL, A.R.C.Sc., F.G.S., University, Glasgow.

Economic Geology.—W. H. Emmons has published a revision and amplification of his memoir on the enrichment of sulphide ores under the more general title of "The Enrichment of Ore Deposits" (*U.S. Geol. Survey,* 1917, *Bull.* **625**, pp. 530). This is a most exhaustive treatment of the physics and chemistry of the phenomenon of enrichment, which generally results from the solution of ores near the surface and their reprecipitation in depth.

The stanniferous clays of the Kinta district (Perak), regarded by Scrivenor as of glacial origin and of Permo-Carboniferous age, are now relegated by W. R. Jones (*Q.J.G.S.* 1917, **72**, pt. 3, 165–97) to a much later date, and are ascribed to the weathering of a stanniferous granite, and of the adjacent schists and phyllites. Ninety per cent. of the ore is derived from mines situated near the junction of granite and country rocks. On the contrary, in the Cooktown (Queensland) tinfield, described by E. C. Saint-Smith (*Queensland Geol. Survey,* Publ. No. **50**, 1916, pp. 211) the recent alluvial deposits supply by far the greater portion of the tin won, although there are tin lodes connected with a granite that cuts the Gympie (Permo-Carboniferous) formation.

The great difficulties of arriving at the origin of the typical West Australian gold deposit are well illustrated in two recent memoirs (E. de C. Clarke, "The Geology and Ore Deposits of Meekatharra, Murchison Goldfield," *Geol. Survey of West Australia,* 1916, *Bull.* No. **58**, pp. 342; E. B. Feldtmann, "The Geology and Ore Deposits of Kalgoorlie: Part 3, East Coolgardie Goldfield," *ibid. Bull.* **69**, 1916, pp. 152). The rocks have been so intensely altered by ancient earth movements that it is extremely difficult, if not impossible, to recognise their original characters, or their relative ages and structural relationships. The Coolgardie memoir is the third of a series describing the famous Kalgoorlie field. The Moonta and Wallaroo field (South Australia) is primarily a copper-mining area (R. L. Jack, "The Geology of the Moonta and Wallaroo

Mining District," *Geol. Survey of South Australia*, 1917, *Bull.* **6**, pp. 135). The lodes are contained in a pre-Cambrian complex of sediments, acid lavas, and granite intrusions, the introduction of the ore being due to the latter.

The ore deposits of three areas in the western United States are exhaustively described in the memoirs referred to (E. S. Bastin and J. M. Hill, "Economic Geology of Gilpin Co. and Adjacent Parts of Clear Creek and Boulder Co., Col.," *U.S. Geol. Survey, Prof. Paper* **94**, 1917, pp. 379: J. B. Umpleby, "Geology and Ore Deposits of the Mackay Region, Idaho," *ibid. Prof. Paper* **97**, 1917, pp. 129; F. C. Schrader, "The Geological Distribution and Genesis of the Metals in the Santa Rita-Patagonia Mountains, Ariz.," *Econ. Geol.* 1917, **12**, 237–69). The two Professional Papers contain complete and valuable discussions of the principles of ore-deposition in the areas described. The Colorado deposits carry primarily gold-silver ores, but uranium, tungsten, copper, and iron ores also occur. The structural types include veins, stockworks, and magmatic segregations. All are believed to have been formed in Tertiary times, and to be genetically related to the Tertiary igneous rocks of the region. The Mackay region of Idaho carries contact deposits of copper ore and veins representing two periods of mineralisation, the earlier of which includes copper, lead-silver, and tungsten deposits, and the later being worked primarily for silver and associated lead. A discussion of the contact deposits shows clearly that a granite magma has supplied large quantities of iron, alumina, and silica to the adjacent limestones, as well as sulphide ores.

A reprint of Chapter IV of the great Report of the Royal Ontario Nickel Commission forms a publication of the Ontario Bureau of Mines (W. G. Miller and C. W. Knight, "Nickel Deposits of the World," *Ont. Bur. Mines*, 1917, pp. 286). This of course contains a full description of Sudbury, Ont., the greatest nickel-mining district of the world. A lively controversy is proceeding between the advocates of contrasted theories of the origin of the Sudbury ores. Coleman and numerous other investigators ascribe the deposits to magmatic segregation followed by re-arrangement of the material through solution and re-deposition in certain places; the authors of this report, however, support the theory of deposition by heated waters circulating through brecciated and sheared zones.

A. M. Bateman proposes a modification of E. Howe's theory that the nickel ore was intruded in a molten state (*Econ. Geol.* 1917, **12**, 391–426). He believes that the intrusion of the Sudbury norite took place in two stages, the first of which gave rise to the "nickel eruptive," the second to the southern granite and finally the differentiated ore, which was partly re-arranged by subsequent hydrothermal action. This view is intended to reconcile the unassailable elements in the conflicting theories mentioned above. In a short paper Coleman vigorously defends the theory that the nickel ore has resulted from the gravitative differentiation of the Sudbury norite micropegmatite sheet (*Econ. Geol.* 1917, **12**, 427–34). He makes strong points of the world-wide association of nickel ores with this kind of igneous rock; and of the absolutely unweathered character of the enclosing igneous rock in normal cases where faulting and subsequent re-deposition has been inoperative, a feature inexplicable on the theory of hydrothermal replacement.

Two useful statistical compilations by G. C. Lloyd ("Report on the Resources and Production of Iron Ores and other Principal Metalliferous Ores used in the Iron and Steel Industry of the United Kingdom," *Advisory Council, Dept. of Scientific and Industrial Research*, 1917, pp. 145), and W. G. Fearnsides ("The Mineral Requirements of the British Iron and Steel Industries," *Trans. Soc. Engineers*, 1917, pp. 7–109) summarise the available resources of iron ores and other materials used in British iron and steel production, especially in view of war conditions. British coalfields are receiving close attention just now, as shown by the publication of a second edition of Part 4 of the Geological Survey Memoir on the geology of the South Wales coalfield (A. Strahan *et alia*, "The Country around Pontypridd and Maesteg," *Expl. of Sh.* 248, 1917, pp. 160), and of a further instalment of the results of the revision of the central coalfield of Scotland (R. G. Carruthers and C. H. Dinham, "The Economic Geology of the Central Coalfield of Scotland: Area VIII. East Kilbride and Quarter," *Mem. Geol. Survey Scotland*, 1917, pp. 52).

T. C. Cantrill describes a boring made for coal in Silurian rocks near Presteign, Radnorshire (*Geol. Mag.* 1917, **4**, 481–92). In spite of his warning that the rocks contained no coal, the promoters persisted in sinking the bore to a depth of

888½ ft., the only useful effect being to provide additional stratigraphical information upon the district. The incident shows how necessary it is, in order to prevent waste and disappointment, that exploratory borings of any kind should be made under the direct supervision of the Geological Survey.

M. C. Campbell has written a general introduction to a comprehensive report on the coal resources of the United States, which is in preparation in view of the growing demand for fuel, and of the probability that the United States may soon be called upon to supply less favoured countries ("The Coal-fields of the United States," *U.S. Geol. Survey, Prof. Paper* 100–A, pp. 1–33). The estimated quantity of unmined coal within 3,000 feet of the surface is 3,538,554,000,000 short tons.

In a general review of the oil-shale possibilities of the United States, D. E. Winchester has examined material from formations ranging in age from the Devonian to the Eocene (*Econ. Geol.* 1917, **12**, 505–18). The richest shale belongs to the Green River formation (Eocene), which underlies an extensive area in Colorado, Utah, Wyoming, and Nevada, and compares favourably in quality with the Scottish material.

Prof. P. G. H. Boswell has published *A Supplementary Memoir on British Resources of Sands and Rocks Used in Glass Manufacture, with Notes on Certain Refractory Materials* (Longmans, Green & Co., 1917, pp. 72), which gives all the information acquired since the publication of his first memoir in 1916. The survey of British resources of glass sands is now fairly exhaustive. Collateral inquiries into deposits containing constituents such as potash and alumina, essential to the glass industry, have been made. The possibilities of crushed rocks in glass manufacture have been more completely investigated, and a description is given of the commoner American glass sands.

Prof. Boswell promises a further work dealing with foundry sands, and in two preliminary papers on the subject (*Journ. Soc. Chem. Industry*, 1917, **36**, pp. 21 reprint; *The Foundry Trades Journal*, August 1917, pp. 16 reprint) he emphasises the importance of mechanical analyses, and of the chemical analysis of the grades present, rather than the bulk chemical composition of the sands.

A large amount of geological work is included in the account of the discussion upon refractories by the Faraday Society (*Trans. Faraday Soc.* 1917, **12**, 1–189). Prof. Fearnsides writes

on the application of petrographical methods to the study of refractories; Drs. Howe and Strahan report on the work of the Geological Survey in regard to refractory raw materials; and Prof. Boswell discusses the properties and resources of refractory sands in this country.

In a discussion of "The Problem of the Great Australian Artesian Basin" (*Journ. and Proc. Roy. Soc. New South Wales*, 1917, **51**, 135–208), A. L. Du Toit agrees with Prof. J. W. Gregory in regarding the water as of composite nature, originating from three sources: residual (Mesozoic), plutonic, and rainfall of an earlier epoch (Tertiary). In opposition to Prof. Gregory, however, he holds that the meteoric supply is at present dominant, but that the water actually stored has been derived in large measure from the above sources.

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JAGGAR, T. A., jun., Live Aa Lava at Kilauea, *Journ. Wash. Acad. Sci.* 1917, **7**, 241–3; On the Terms Aphrolith and Dermolith, *ibid.* 277–81 (these terms are introduced to replace the barbarous words "aa" and "pahoehoe" applied to different types of surface structures in lavas); Thermal Gradient of Kilauea Lava Lake, *ibid.* 397–405.

FRATER, M., The Volcanic Eruption of 1913 on Ambrym Island, New Hebrides, *Geol. Mag.* 1917, **4**, 496–503.

Gregory, J. W., The Ambrym Eruptions of 1913–14, *ibid.* 529–40.

Daly, R. A., The Genetic Classification of Underground Volatile Agents, *Econ. Geol.* 1917, **12**, 487–504.

Adams, F. D., and Bancroft, J. A., On the Amount of Internal Friction Developed in Rocks during Deformation, and on the Relative Plasticity of different Types of Rocks, *Journ. Geol.* 1917, **25**, 597–637.

Cotton, C. A., Block Mountains in New Zealand, *Amer. Journ. Sci.* 1917, **44**, 249–93.

Keyes, C. R., Orographic Origin of Ancient Lake Bonneville, *Bull. Geol. Soc. Amer.* 1917, **28**, 351–74.

Kindle, F. M., Deformation of Unconsolidated Beds in Nova Scotia and Southern Ontario, *ibid.* 323–34.

Coleman, A. P., Wave Work as a Measure of Time: A Study of the Ontario Basin, *Amer. Journ. Sci.* 1917, **44**, 351–9.

Davis, W. M., The Great Barrier Reef of Australia, *ibid.* 339–50; The Structure of High-standing Atolls, *Proc. Nat. Acad. Sci.* 1917, **3**, 473–9; The Isostatic Subsidence of Volcanic Islands, *ibid.*, 649–54.

Daly, R. A., A New Test of the Subsidence Theory of Coral Reefs, *ibid.* 664–70.

BOTANY. By E. J. Salisbury, D.Sc., F.L.S., East London College, University of London.

M. Henri Blin, in *La Nature* for October, deals with the problem of the feeding of cattle under present conditions and calls attention to the value, as fodder, of several species that in normal times are almost neglected.

As the plants cited in this connection are also common in Britain, the practical data given indicate that in this country also much might be done to augment the depletion of fodder consequent upon restricted importation.

The Furze (*Ulex europæus*) utilised for this purpose in Brittany is equal in value to one-third or even half the quantity of good hay, whilst Broom (*Sarothamnus scoparius*) also has about half the feeding value of the latter. Heather, too, can be utilised for this purpose, and, like the foregoing species, has the merit of affording nourishment at a time when supplies are usually lowest. Even mistletoe can be used as forage, and

the leaves and young shoots of Elm, Lime, Hornbeam, Willow, and Poplar are not to be despised. Useful data are furnished as to the quantities in which these various fodders can be supplied, together with analyses showing the proportion of nitrogenous material, fatty material, and hydrocarbons which they contain.

Morphology.—Prof. Marloth, dealing with the occurrence of bulbils in South African plants (*S. A. Jour. Sci.*, April 1917), calls attention to the fact that of the twelve examples cited, nine either do not fruit or fail to produce seed. The question arises, Is the barrenness a result of bulbil formation or is the latter an outcome of the former? The production of bulbils by a sterile hybrid *Micranthus* and by *Gladiolus cuspidatus* when the latter is attacked by *Uromyces gladioli* leads the author to support the second alternative.

Two important papers dealing with the gametophytes of *Psilotum* and *Tmesipteris* appear in the *Transactions of the Royal Society of Edinburgh* (vol. lii. pt. 1, Nos. 3 and 4). In the first of these Mr. Darnell-Smith describes in particular the spore and germination of *Psilotum*, and in the second Prof. Lawson describes in detail the structure of the prothallus of *Psilotum* and of *Tmesipteris*. In both genera the gametophytes are remarkably alike. The main body is branched, cylindrical in form, and of a light brown colour. Associated with the absence of green pigment and the subterranean habit, the nutrition is saprophytic and entirely dependent upon an endophytic and non-septate mycorrhizal fungus. This latter is not, as in the gametophytes of Lycopodium, confined to a definite zone, but distributed throughout the adult prothallial tissue.

Numerous rhizoids are developed from the surface and growth takes place by means of apical meristems terminating each branch of the gametophyte. Both antheridia and archegonia occur on the same prothallus. The former are developed from superficial cells, and at maturity are spherical structures with a single layered wall containing numerous coiled multiciliate gametes. The most striking feature, which distinguishes these antheridia from those of either *Equisetaceæ* or *Lycopodiaceæ*, is that they project from the surface in place of being embedded in the prothallial tissue.

The archegonia like the antheridia arise from any part of

the surface of the gametophyte. The venter is embedded and terminated by a straight neck of four rows of cells.

The differences between the prothalli of the two genera mainly depend on the size of the male and female organs, and their relative abundance.

Taxonomy.—In the September and October numbers of the *Naturalist*, Mr. D. A. Jones contributes a list of the Mosses and Hepatics of Denbighshire. Of the species and sub-species enumerated, over one hundred are new records for the county and to these must be added some forty additional varieties. Amongst the rarer mosses may be noted *Barbula gracilis, Trichostomum crispulum* vars. *nigro-viride* and *brevifolium, Encalypta rhabdocarpa* and *Amblystegium Kochii.* The Hepatics include *Moerckia Flotowiana, Cephaloziella byssacea* v. *asperifolia, C. stellulifera, C. myriantha,* and *C. Limprichtii.*

Lists are given of species characteristic of the different geological formations.

The same author describes and figures several new varieties of Mosses (*Journal of Botany*, October 1917), viz. *Campylopus setifolius* v. *intermedius, Bryum pallens* v. *euryphyllum, Porotrichum alopecurum* v. *aridulum,* and *Pterogonium gracilis* v. *harlecense.*

In the same journal Mr. Baker contributes an account of the flora of Burnham Beeches, and Dr. Wernham describes a number of new species of tropical American Rubiaceæ.

In the *Naturalist* for December, Mr. F. A. Lees gives a useful account of the species of *Erodium* found as colonist-aliens in Yorkshire.

Ecology.—J. S. Henkel, writing on Forest Progress in the Drakensberg (*S.A. Jour. Sci.*, December 1916), describes the process of succession from veldt to forest. In the early phases scattered bushes of *Protea,* together with *Cussonia, Celastrus buxifolius, Leucosidea sericea,* and *Myrsine africana,* which appear in the order named, compete with the veldt grasses and by their shade gradually eliminate them. Later as the shrub vegetation increases, lianes and seedlings of various trees appear, and the veldt-grasses become replaced by bush-grasses and ferns. The *Leucosidea* bushes are suppressed by the forest trees and shrubs, till finally the climax phase is reached in which the prevailing arboreal forms are *Podocarpus thunbergii, Olinia cymosa,* and *Myrsine melanophleas.*

In the *Annals of the Natal Museum* (vol. iii. 1917), Dr. Bews gives an account of the Plant Ecology of the Drakensberg Range. The soil is very poor in soluble mineral salts, especially lime and phosphates, and is chiefly composed of mineral matter, the organic and volatile constituents only representing about 5 per cent. of the total. The veldt formation, which attains an altitude of over 8,000 ft., is the most extensive. From the veldt of the Midlands it is distinguished by the tussock habit of the dominant grasses and their more xerophytic character. The *Andropogon* associations are common, but the *Anthistiria* association, so frequent at lower altitudes, is but poorly developed. The occurrence of veldt in place of bush appears to be dependent not only on lower rainfall, but also on the occurrence of frosts and exposure to desiccating winds. Where the rainfall is high and there is adequate protection, bush replaces veldt, so that the former is particularly characteristic of south-eastern slopes.

Maquis or fynbosch occupies the steep and unstable slopes covered with loose talus. The dominant species are *Cliffortia* spp. *Myrsine africana*, *Passerina* spp., *Phylica paniculata*, *Metalasia muricata*, and a number of Compositæ and Ericaceous plants. This sclerophyllous vegetation is the starting-point of a double succession, on the one hand through rocky fell-field to tussock veldt, and on the other through rocky scrub, dominated by *Greyia sutherlandi*, and scrub dominated by *Leucosidea sericea* to mountain bush. This final phase is characterised by *Podocarpus* spp., *Celtis*, etc. It exhibits a marginal belt which, like the corresponding region of our own British woodlands, has a floristic composition very similar to the scrub.

Marshy spots along the stream-banks and the vleis are occupied by an association chiefly of Cyperaceous plants, together with a number of other monocotyledons.

On the cliffs the pioneer vegetation consists of various Cyanophyceæ followed by Bryophytes. This cryptogamic flora gives place to various types of chomophytic association depending on the illumination and degree of exposure.

C. A. Jensen has investigated the effect of decomposing organic matter on the solubility of some of the inorganic constituents of the soil (*Jour. Agric. Research*, May 1917). Filtered extracts of decomposing hay, sweet clover and barley hay were prepared and utilised for the extraction of a clayey

and a sandy loam respectively. It was found that these organic solutions gave no reaction either with phenolphthalein or methyl orange, but nevertheless had a pronounced effect on the solubility of the calcium and phosphoric acid in these soils. The amount of phosphoric acid dissolved from the soil by the organic solvents was from 1·7 to 5·4 times that extracted by distilled water. The solubility of magnesium and iron was also considerably increased by their use. It is suggested that the beneficial effect of mulches may be in part due to these facts.

PLANT PHYSIOLOGY. By WALTER STILES, M.A., University, Leeds. (Plant Physiology Committee.)

Permeability.—There are a number of expressions in common use in plant physiology which serve very largely to screen our ignorance of the problems under discussion. Among these is the term permeability. This word is used in a variety of senses by different writers, and cases can be cited of the same writer using it in different senses in the same paper. The simplest sense in which the word permeability may be used is in regard to the capacity of a membrane for allowing a substance to pass through from one side to the other. In the case of the cell, however, we are dealing with a structure much more complex than a membrane. Outside we have the cell wall, pressed against this is the cytoplasm of the cell, and within this is the vacuole. At the surface of separation of the cell wall and cytoplasm, and also of the cytoplasm and vacuole, we may suppose layers (the ectoplast or plasmatic membrane, and the tonoplast or vacuole wall respectively) with properties different from those of the main part of the cytoplasm. The most general sense then in which the expression permeability can be used in regard to the living cell is when it indicates simply the capacity of the substance to pass into or out of the cell, and this is the sense in which it is generally used. Some writers, however, speak of permeability when they mean the capacity of a substance to pass from the external medium through the cytoplasm, into the vacuole, or even from outside the cell through the limiting layer (plasmatic membrane) into the cytoplasm. It is sometimes not clear what a writer does mean. There is thus a necessity for definition of the term permeability, and in this article it will be taken in its ordinary general sense as

referring to the capacity of the cell for allowing the passage of a particular substance into it from the external medium, or out of it into the external medium. Such a use of the term is not ideal, but it is the most general, and in the present state of our knowledge a more exact use is scarcely possible.

In regard to permeability phenomena it is impossible to generalise to any extent, for not only will the permeability of the cell to different substances be different, but plant cells differ among themselves both morphologically and physiologically, and may exhibit different permeability properties.

In the living plant permeability phenomena always have to do with solutions, and the permeability to the solvent may be perfectly different from that to the solute. The solvent is always water; in this review of recent advances in our knowledge of permeability we will consider first the question of permeability to the solvent.

Permeability to Water: (a) Seed-Coats.—Denny (*Bot. Gaz.* **63**, 373–97, 1917) has investigated the effect of temperature on the passage of water through the seed-coat of the pea-nut (*Arachis hypogæa*). The measurements were made by means of an osmometer in which the membrane separated pure water and a solution. Contrary to the results previously obtained by A. J. Brown for barley, Denny found the temperature coefficient (Q_{10}) lower than that usual in chemical reactions (2 to 3); it was, however, higher than the temperature coefficient of diffusion (about 1·3). The coefficients were found to decrease with rise of temperature, *e.g.* in one set of experiments Q_{10} between 5·2° C. and 15·2° C. was 1·628, while between 35° C. and 45° C. it was 1·344.

A surprising result obtained by Denny was that water passed through the membrane from the outer surface more rapidly than in the reverse direction. Great variations in the permeability were exhibited by the seed-coats of different species, and it is shown that the thickness of the membrane is not the determining factor.

In a later paper the same author (*Bot. Gaz.* **63**, 468–85, 1917) concludes that lipoid substances, tannins, and pectins are important in determining the permeability of seed-coats to water. This conclusion is based on the change in permeability brought about by extracting different seed-coats with various solvents. Thus in the case of peanut and almond, extraction

of the seed-coats with hot water increased the permeability, but the permeability of grape fruit and squash seed-coats was not affected. In all cases except that of grape fruit extraction with solvents of lipoid substances increased permeability, and treatment with calcium chloride brought about a similar result.

(*b*) *Living Tissue.*—Delf (*Ann. of Bot.* **30**, 383–410, 1916) has made a study of the effect of temperature on the rate at which water passes out of onion leaves and dandelion scapes when these are under the influence of solutions slightly stronger than isotonic (so-called subtonic solutions). The rate of water loss was measured by the decrease in length of the tissue. The method of measurement was very delicate, depending on the use of an optical lever, and the changes in length of the tissue could be continuously observed. Curves between shrinkage and time were obtained with 0·18 and 0·3 molecular solutions of cane sugar at a series of temperatures between 5° C. and 42° C. These curves are practically logarithmic, and the rates of shrinkage at different temperatures are proportional to the tangents of the angle made with the time axis by the tangents drawn to the different time-shrinkage curves at points of equal shrinkage. Temperature coefficients (Q_{10}) of rates of shrinkage and consequently of rates of water loss were found in this way. They exhibited a good deal of variation, *e.g.* onion 1·4 between 5° C. and 15° C. and 3·0 between 30° C. and 40° C.; dandelion, 2·3 between 10° and 20° C., and 3·8 between 20° C. and 30° C.

Observations on the influence of temperature on the intake of water by potato tuber and carrot root have been made by Stiles and Jörgensen (*Ann. of Bot.* **31**, 415–34, 1917). The amount of water absorbed was measured from time to time by the increase in weight of a number of thin uniform discs of the tissue immersed in water. Curves were plotted between time and increase in weight (swelling) for a number of different temperatures, and temperature coefficients of swelling calculated in the same way as those of shrinkage were calculated by Delf. Here again considerable variation in the temperature coefficients was exhibited, *e.g.* Q_{10} for potato between 10° C. and 20° C. was found to be 3·0, while that for carrot for the same interval was only 1·3.

Having regard to the complexity of the system under investigation, the variations exhibited in the results of both Delf

and Stiles and Jörgensen is not surprising, as the swelling or shrinkage depends among obvious factors on the resistance to stretching of the cell wall, the passage of water through the protoplast, the passage of water through the cell wall, and on the previous history of the tissue. Under these circumstances it is obvious that conclusions drawn from temperature coefficients should be made with extreme caution.

Permeability to Dissolved Substances: (a) Seed-Coats.—A. J. Brown and F. Tinker (*Proc. Roy. Soc.* B, **89**, 373–9, 1916) have continued their investigations on the permeability of the seed-coat of barley. Seeds were steeped in solutions of aniline, phenol and acetic acid, and the extent of absorption of these substances determined by direct analysis of the seeds. With aniline and phenol the substance accumulates in the seed to such an extent that the solution inside the seed is about three times as strong as that outside. Acetic acid, on the other hand, is not "heaped up" to the same extent, and for stronger solutions (the ratio acetic acid : water between 0·5 and 0·9) the strength of solution inside the seed remains constant at 80 per cent.

(*b*) *Living Tissue.*—A valuable criticism of the plasmolytic method used for permeability estimations has been offered by Fitting (*Jahrb. f. wiss. Bot.* **57**, 553–612, 1917). It has previously been generally assumed that a determination of the permeability of the cell can be obtained from the differences between isotonic coefficients as determined plasmolytically and the same coefficients determined physico-chemically. If a substance is impermeable these values should be the same; if it is permeable the plasmolytically determined isotonic coefficient should be higher. Fitting points out that earlier determinations of isotonic coefficients by the plasmolytic method were subjected to various sources of error in that (1) no account was taken of exosmosis from the cells, (2) the relation between plasmolysis and time was neglected, (3) the gradations between the salt solutions used were not sufficiently fine, (4) the coefficients were determined in relation to potassium nitrate instead of sucrose, and (5) salt solutions may have a possible influence on permeability. Among possible errors in the physico-chemical determination of isotonic coefficients the assumption that the van 't Hoff law holds for the concentrations used is likely to be of importance. The general conclusion is drawn

that it is impossible to estimate permeability by a comparison of isotonic coefficients as determined physico-chemically and by plasmolysis.

A method allied to the plasmolytic method has been adopted by Brooks (*Amer. Journ. Bot.* **3**, 562–70, 1916) for observations on permeability. When scapes of dandelion are cut into strips they curl, and when put in a hypertonic solution the curvature is reduced. If the substance enters the cell the original curvature is gradually regained. The rate at which this takes place is regarded as a measure of permeability of the cells to the solute. From such observations Brooks concludes that sucrose and salts of univalent kations bring about a rapid increase of permeability, while divalent and trivalent kations cause a great decrease. The same writer (*Amer. Journ. Bot.* **3**, 483–92, 1916) comes to a similar conclusion from measuring the rate of exosmosis of electrolytes from dandelion scapes into distilled water after their immersion for 15 to 25 minutes in isotonic solutions of sodium chloride, calcium chloride, and cerium chloride.

Wodehouse (*Journ. Biol. Chem.* **29**, 453–8, 1917) points out that the cells of the marine alga *Valonia* are so large that from each one of them 1 to 5 c.c. of sap can be obtained, a quantity sufficient for performing chemical tests. He found that nitrates and potassium are present in the cell in higher concentration than in the external medium; sodium, calcium, and chlorides were equally abundant within and without the cell, while magnesium and sulphates, which are present in sea water in notable concentration, are practically absent from the cell sap. The selective permeability thus demonstrated is shown not to be due to the cell wall, which has no selective properties.

Artzikovskii and Shelyakina (*Bull. Acad. Sci. Petrograd*, 1043–62, 1916) maintain that various acids (hydrochloric, sulphuric, nitric, phosphoric, acetic, etc.) and salts of heavy metals in high concentrations $\left(\frac{N}{8} \text{ to } N\right)$ do not destroy the functioning of the cell membrane in *Begonia rex*. However, the observations of Hind (*Ann. of Bot.* **30**, 223–38, 1916) show that all acids pass rapidly into plant cells, and Stiles and Jörgensen (*Ann. of Bot.* **31**, 415–34, 1917) show that this absorption, even with very dilute solutions, is accompanied by

a lessening of swelling of the tissue as compared with that which takes place in distilled water, while in solutions of a strength of only $\frac{N}{50}$ a shrinkage results after a short preliminary swelling period, and this shrinkage is recognisable as that accompanying injury.

Osterhout has continued his researches on the electrical conductivity of *Laminaria* under various conditions, and has drawn a number of conclusions from his results based on the assumption that the electrical conductivity is a measure of the permeability of the tissue. A discussion of these conclusions would, however, be premature before a full account of the apparatus and method has been published, together with the evidence for accepting electrical conductivity as a measure of permeability.

ANTHROPOLOGY. By A. G. THACKER, A.R.C.Sc.

THE first place in recent anthropological literature must again be given to the *Journal of the Royal Anthropological Institute*. The publication for the first half of 1917 (vol. xlvii. pt. 1) is even more interesting than usual. The first article is Prof. Arthur Keith's Presidential Address, which is entitled "How can the Institute best serve the needs of Anthropology?" The address consists largely of a survey of the history of the Institute and of the growth of anthropological science in England. The Anthropological Institute grew out of the Ethnological Society, which was founded in 1844. Among the early leaders of the Society there appear the names of men who were pioneers in other branches—often in several other branches—of science, such as Huxley, Spencer, Russel Wallace, Hooker, Galton, Murchison. In these days of high specialisation, one is amazed at the versatility of these men. In answering the question which gives the title to his address, Prof. Keith says that in his opinion the most important duty of the Institute is to maintain its publications, and that after that, the most urgent matter is to provide for the upkeep of the library. Also, "the claims of anthropology for a place amongst the subjects fostered and taught at universities" should be pressed. And, finally, Prof. Keith mentions the point, which has often been insisted upon in SCIENCE PROGRESS,

that, in the words of Sir Richard Temple, our statesmen must be made to perceive the "administrative value of anthropology."

The longest and probably the most important article in this issue is a contribution by the Rev. S. S. Dornan on "The Tati Bushmen (Masarwas) and their Language." This seems to me to be one of the most important pieces of anthropological research which has appeared for several years. The term Masarwa is applied to the northern Bushmen by the Bechuanas. They live in the Bechuanaland Protectorate, the Kalahari, the western parts of Southern Rhodesia, and probably extend into German South-West Africa and into Portuguese West Africa. That there are also Bushmen in the eastern parts of Southern Rhodesia and in Portuguese East Africa is certain, but the author is doubtful whether these are properly described as Masarwas. The particular group of Masarwas to which this paper mainly refers is the group living in the Tati district. The author says: "I do not think there is much doubt that the Pygmies and the Bushmen are closely connected, probably originally the same people, and the differences in colour and habits of life are due simply to difference of habitat, environment, and mode of living, the one being forest dwellers and the other desert inhabitants." Mr. Dornan believes that these Tati people are almost pure Bushmen, having had very little admixture of Bantu or Hottentot blood. Many interesting details concerning their ways of life are given. The author dissents entirely from the generally accepted theory of the origin of the Hottentots, namely, that they arose from a wholesale crossing of negroes with Bushwomen. He does not think it possible that negroes, capturing Bushwomen and children and keeping them as wives and slaves, would lose their own language and adopt that of the conquered race. He thinks the reverse would occur. He then gives it as his opinion that "physically and linguistically the Hottentots and Bushmen were one people in the remote past, but they have lived so long apart that the degree of relationship, at least in the languages, is very slight." The language, which is studied very fully, is distantly related to Namaqua. No ethnologist should miss this paper.

Another interesting paper is that by Dr. C. G. Seligman on "The Physical Character of the Arabs." The population of Northern Arabia is, he says, predominantly long-headed, and

that of Southern Arabia round-headed. This brachycephaly is due, at least in part, to Mesopotamian influence; and the interesting conclusion is reached that the round-headed element in North Africa is due not to an infusion of the so-called Alpine type of Europe, but to the immigration of these Arabian round-headed people.

Other papers in the *Journal* are: "Ancient Royal Hindu Marriage Customs," by Pandit Vishwanath; "Notes on Some Beliefs and Customs of the 'Orang Dusun' of British North Borneo," by J. H. N. Evans; "Some Ibo Burial Customs," by N. W. Thomas; and "Studies in Primitive Looms," by H. Ling Roth.

The *American Anthropologist* for the first half of 1917 (vol. xix. pts. 1 and 2) contains various interesting papers. "Similarities in Culture," by W. D. Wallis (in Part 1), a paper continuing the never-ending discussion on borrowing *versus* convergence as explanations of identities in culture, may be specially mentioned. Wallis is a cautious supporter of the convergence-theory. Other papers are: "Game Totems among the North-eastern Algonkians," by F. G. Speck; and "Variations in the Glenoid Fossæ," by L. R. Sullivan.

Recent contributions to *Man* are mostly very brief, but a paper by Dr. F. Romanet du Caillaud on "Burgundian Switzerland" (November) should be mentioned. The author thinks that most of the German Swiss are descended from Burgunds, not from Alemans.

ARTICLES

THE VISCOSITY OF PURE LIQUIDS

By SIR EDWARD THORPE, C.B., LL.D., F.R.S.,
Emeritus Professor of Chemistry, Imperial College of Science and Technology, London

UNDER this title, Prof. Svante Arrhenius, the Director of the Nobel Institute, Stockholm, has published recently an important communication dealing with the viscosity, or internal friction, of homogeneous liquids[1] in which he discusses this property in relation to other physical characteristics, and incidentally throws additional light upon the question which has exercised many minds since the time of Poiseuille, who first experimentally established the principles of a convenient and accurate method of measuring the viscosity of liquids, namely, What is the connection between this particular physical property and the chemical nature (using that phrase in its most comprehensive sense) of the liquids?

It has already been pointed out by the writer of this article in memoirs published in the *Philosophical Transactions of the Royal Society* in conjunction with the late Mr. J. W. Rodger[2] that the viscosity-values of the paraffin hydrocarbons and of the ethers at their normal boiling-points, when they are presumably under comparable conditions of temperature, are identical in each class respectively. This regularity was not observed under similar circumstances in the case of other homologous series: in general an increment of CH_2 brings about a diminution of the viscosity-coefficient.

Starting from the fact that, as a rule, the specific weight at the boiling-point in these homologous series decreases with the increasing number of CH_2 groups, and from the assumption

[1] *Meddelanden från K. Vetenskaps Akademiens Nobelinstitut*, Band 3, No. 20. Stockholm: Almquist & Wiksells Boktryckeri, A.E.; London: William Wesley & Son, 28, Essex Street, Strand.

[2] *Phil. Trans.* A. **185**, 397 (1894), and **189**, 71 (1897).

that viscosity is intimately connected with density, as originally maintained by Laplace and recently supported by Batschinki (*Zeitsch. f. Physikal. Chemie.* **84**, 643, 1913), Arrhenius finds that regularities become apparent if we have regard not to the viscosity (η) itself, but to the quotient $\eta : \sqrt{s}$, where s is the specific weight of the liquid at its boiling-point. The nature of these regularities, and the extent of their uniformity in the several groups of correlated substances, may be seen from the following excerpts from the lengthy tables of results furnished by the observations of Thorpe and Rodger, as calculated by Arrhenius. The tables are too long to be given in full, but the examples quoted are typical of the whole, which comprises eighty-eight liquids, comprehended under twenty groups of homologues or correlated substances.

Values of the Viscosity Coefficient η, the Density s, and of $\eta : \sqrt{s}$ at the Boiling-point

Substance.	$\eta \cdot 10^{5}$.	s.	$\eta \cdot 10^{5} : \sqrt{s}$.
Pentane	200	0·5995	256
Iso-pentane	203	0·6057	261
Hexane	204	0·6103	261
Iso-hexane	205	0·616	261
Heptane	199	0·6144	254
Iso-heptane	198	0·616	252
Octane	198	0·6124	253
Acetaldehyde	219	0·773	249
Ethyl ether	205	0·695	246
Methyl propyl ether	211	0·700	252
Ethyl propyl ether	210	0·687	253
Dipropyl ether	212	0·674	258
Methyl isobutyl ether	210	0·689	253
Ethyl isobutyl ether	208	0·674	253
Methyl formate	312	0·957	319
Ethyl formate	289	0·878	309
Propyl formate	278	0·808	309
Methyl acetate	266	0·882	283
Ethyl acetate	253	0·830	278
Propyl acetate	246	0·792	275
Methyl propionate	255	0·842	278
Ethyl propionate	242	0·796	271
Methyl butyrate	250	0·805	279
Methyl isobutyrate	251	0·805	280

Substance.	$\eta \cdot 10^5$.	s.	$\eta \cdot 10^5 \sqrt{s}$.
Carbon bisulphide	305	1·222	276
Methyl sulphide	253	0·832	277
Ethyl sulphide	234	0·739	272
Benzene	316	0·809	350
Thiophen	339	0·988	341

The general result of this comparison between the viscosity-coefficients and the density at the respective boiling-points—that is, at the temperatures at which their vapour pressures under normal atmospheric pressure are the same—of these eighty-eight liquids, when we exclude the strongly associated alcohols and acids, in other words hydoxylated compounds, is to show that the quantity $\eta : \sqrt{s}$ is of the same order of magnitude. Further, in each of the natural groups the values of $\eta : \sqrt{s}$ are very nearly constant. This is true not only of the saturated hydrocarbons and the ethers, but also for the groups of esters. The tables show that the individual members of each group of alkyl iodides, bromides and chlorides are found to have very nearly the same value, the mean values for the monohalogen compounds being respectively 278, 289, and 302. Hence in these substances chlorine acts a little more strongly than bromine, and bromine somewhat more strongly than iodine; that is, viscosity tends to diminish very slightly with the increase in the atomic weight of the halogen. At the same time it ought to be noted that all the allyl halides have the same viscosity-coefficient at the boiling-point when treated by Arrhenius's method, as will be seen from the following table:

	$\eta \cdot 10^5$.	s.	$\eta \cdot 10^5 : \sqrt{s}$.
Allyl iodide	344	1·661	273
Allyl bromide	315	1·336	273
Allyl chloride	262	0·9219	276

Although even in this case chlorine appears to act a little more strongly than the other halogens.

In the case of products containing two halogen atoms, methylene and ethylidene chlorides are found to have identical values of $\eta : \sqrt{s}$, viz. 320; this is further almost identical with the values for carbon dichloride and chloroform.

The group of the ketones also shows a practically uniform value in mean of 270.

These regularities, it must again be stated, are not perceived in the case of highly associated molecules, such as water, certain acids, and alcohols, which are known from other phenomena to have an abnormal molecular complexity. In the case of the higher members of the $C_nH_{2n}O_2$ series of acids, this molecular complexity breaks down with increasing molecular weight: and it is found that propionic acid (343), butyric acid (343), and iso-butyric acid (341) have the same value of $\eta . 10^6 : \sqrt{s}$.

Considering that the regularities observed by Arrhenius occur among substances which are chemically unrelated, it would further seem that chemical nature and constitutive influences are, at most, only subordinate agencies in affecting the internal friction of liquids. Or, in other words, chemical nature has no greater influence on the viscosity of liquids than the chemical nature of the gases has upon their relations to the ordinary gaseous laws—a generalisation which needs further evidence before it can be wholly accepted.

Of late years, as the experimental difficulties have been gradually surmounted, considerable attention has been paid to the influence of high pressures on the viscosity of liquids. Faust has shown from observations on ethyl ether, carbon bisulphide, and ethyl alcohol under pressures up to 3,000 atmospheres that viscosity increases very rapidly with pressure, especially at low temperatures, and that the rate of increase is far greater at high than at low pressures. The part played by internal pressure on viscosity has been variously stated by Van der Waals and Tammann. The question has now been re-examined by Arrhenius in the light of Faust's experimental results. It is to some extent complicated by the circumstance that one of the substances examined by Faust, namely alcohol, is a highly associated liquid, and, as in the case of all such liquids, its behaviour departs from that of liquids of normal complexity. The constants of Van der Waal's equation would appear to change with temperature, and it is inapplicable to liquids far below their critical points. The quantity a, which represents the molecular attraction, in all probability decreases with increasing temperature. In the case of ethyl ether the viscosity is nearly proportional to the square of the

total (internal and external) pressure, but this is not found to hold good with other liquids. On the whole, the logarithmic formula

$$\log\eta = \log\eta_0 + l(p - p_0 + \pi - \pi_0)$$

agrees fairly well with the experimental data.

Arrhenius points out that as the viscosity of liquids under high pressures increases in accordance with an exponential formula, it follows that in the interior of the earth where the pressures are enormous, the viscosity of the magma, which is very viscous even at low pressures, reaches such a high value that it may be regarded as a solid substance when acted upon by sudden changes of pressure—a conclusion rendered probable by other considerations and in harmony with our experience of the propagation of seismic disturbances.

Dr. Phillips's observations of the viscosity of liquid carbon-dioxide (*Proc. Roy. Soc.* 87 A, **56**, 1912) in the neighbourhood of its critical point are specially interesting from the circumstance that we are here concerned with a change of physical state, and a consequent alteration in the direction of change of viscosity with temperature. Gases increase in viscosity with an increase of temperature, whereas liquids decrease. In the case of gaseous carbon dioxide the increase is about 0·35 per cent. for 1° C. At or near the critical point the viscosity is nearly independent of the temperature and is dependent only on the density. Arrhenius has discussed these observations and finds that his formula expresses them satisfactorily, not only for the liquid state but also for a short interval of the gaseous condition. It seems very probable that the formula with a change of numerical constant is valid so long as the sum of the internal and external pressures does not exceed 220 atmospheres, which is consonant with the fact that viscosity is nearly independent of pressure below one atmosphere.

Attempts to express the change of viscosity with temperature by some simple and comprehensive formula have been made by a number of investigators, but the results, as rational expressions, are not wholly satisfactory. Bingham has sought to connect *fluidity*, the reciprocal of viscosity, with vapour pressure, and is of the opinion that at a sufficiently high temperature the fluidity of ethers and hydrocarbons is a linear function of the vapour pressure. This regularity is not generally

true, and even where it is observed seems to be restricted to comparatively short intervals. From data obtained from published observations Porter found that fluidity (f) and vapour pressure (p) are connected by the formula

$$\log f = A \log p + B$$

which after differentiation in regard to temperature (t) gives

$$\frac{d \log f}{dt} = A \frac{d \log p}{dt}$$

By means of this formula Arrhenius has recalculated the values of A for water and mercury and finds that so far from being constant, they decrease in the ratio of 1 to 0·48 in the case of water over the interval of 150° C.; and from 1 to 0·63 in the case of mercury in an interval of 180° (40°–220°) and thereafter increase. Non-associated liquids show the same peculiarity: A has a minimum value at a certain temperature, which in the case of these liquids seems to be below the boiling-point, with a variable range from about 45° in the case of benzene to about 105° in that of octane. The value of A has been calculated for a number of liquids from the data given in the memoirs already cited.[1] Although Porter's equation is pretty nearly fulfilled in some cases it is not in others, and is especially inexact in the two instances he adduces as evidence of its validity.

In the case of associated liquids like the aliphatic alcohols and certain of the acids, the value of A, although fairly uniform, shows change with temperature and, as with normal liquids, gives indication of a minimum, depending probably on the complexity of the molecule. In all strongly associated liquids, *e.g.* water, A steadily decreases with t below the boiling-point. No simple relation between the value of A and molecular weight is apparent in the different homologous series. In the case of the alcohols it seems to increase as we ascend the series. In the case of the aliphatic acids, so far as these have been observed the reverse occurs: formic acid has the largest value of A, propionic acid the smallest, butyric and *iso*butyric acids have the same value.

Confining himself to the data furnished by the two memoirs

[1] Thorpe and Rodger, *loc. cit.*

just referred to, in which the errors of observations are small (about 0·2 per cent.), Arrhenius finds that the influence of temperature on viscosity may for non-associated liquids be expressed by

$$\frac{d\log\eta . v^{\frac{1}{3}}}{dt} = \frac{K_1}{T^2}$$

Where v is the specific volume and η the viscosity of the liquid at the absolute temperature T and K_1 is a constant. K_1 is nearly proportional to the absolute temperature T_6 of the boiling-point of the liquid, so that $K_1 : T_6$, if common logarithms are used for calculating K_1, does not for normal liquids change between greater limits than 1 and 1·2. Exceptions are found in the case of liquids in which the value

$$K_1 = T\frac{d\log\eta . v^{\frac{1}{3}}}{dt}$$

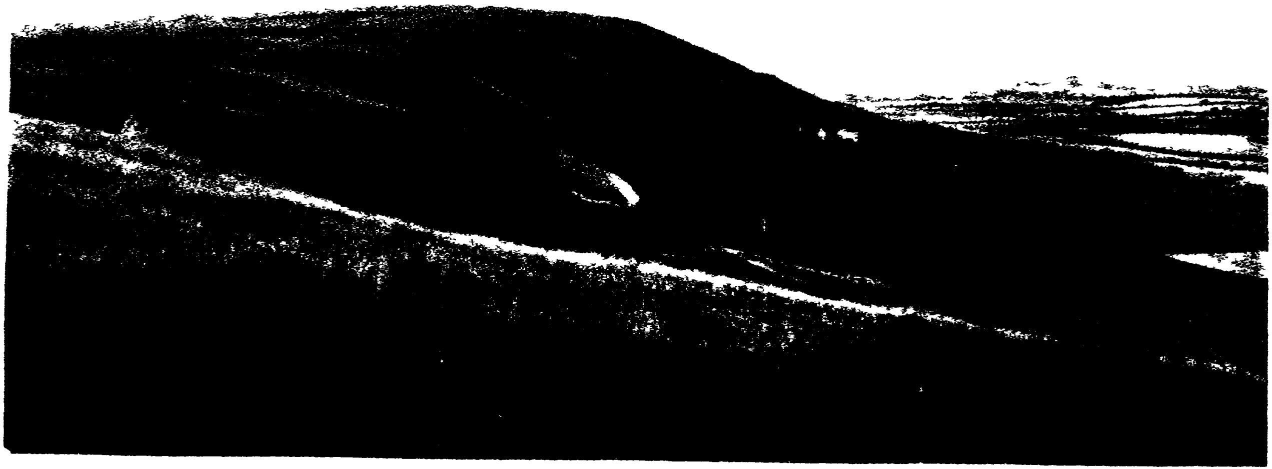
shows no minimum below the boiling-point. For associated liquids the values of $K_1 : T_6$ are much higher, viz. between 1·25 and 4·32 (trimethyl carbinol). There is also found a close parallelism in these liquids between the values of $\eta_6 : \sqrt{s}$ and $K_1 : T_6$. In a homologous series $K_1 : T_6$ increases with the boiling-point, recalling the analogous behaviour of Trouton's Constant. Hence the analogy in the change with temperature of the vapour pressure and that of the value of the viscosity multiplied by the specific volume of the liquid.

The following tables contain a summary of the values of $10^6 . \eta_7 : \sqrt{s} = A$, and of $K_1 : T_6 = B$ arranged in order of increase of A for the several groups of liquids upon which the above general deductions are based:

Normal Liquids

	A.	*B.*
Unsaturated hydrocarbons	233	1·002
Acetaldehyde	249	1·005
Paraffins	257	1·078
Aromatic hydrocarbons (4 c. C_6H_6)	267	1·088
Ketones	270	1·074
Mercaptans	275	1·012
Iodides	277	1·073
Acid anhydrides	279	1·153
Esters	288	1·169
Monobromides and acetylene dibromide	299	1·079

	A.	*B.*
Monochlorides	296	1·129
Ethylidene dichloride, CH_2Cl_2, $CHCl_3$, C_2Cl_4	320	1·081
Thiophen	341	1·243
Benzene	350	1·406
Dibromides (of ethylene, propylene, and isobutylene)	365	1·296
Ethylene dichloride	381	1·177
Carbon tetrachloride	400	1·452
ASSOCIATED LIQUIDS		
Water	290	1·800
ACIDS		
Formic acid	507	1·800
Acetic acid	398	1·387
Propionic, butyric, and *iso*butyric acids	342	1·322
ALCOHOLS		
Methyl alcohol	380	1·562
Allyl alcohol	422	2·078
Other primary alcohols (including amyl alcohols)	483	2·507
Secondary alcohols	552	3·066
Tertiary alcohols	579	3·784



Ditchling Beacon

Fr[illegible]

THE DOWNS AND THE ESCARPMENTS OF THE WEALD

A NEW VIEW OF THEIR GEOLOGICAL HISTORY

By MAJOR R. A. MARRIOTT, D.S.O.

The Argument

The 'denudation' of the Weald has been so constantly the theme of teachers of geology, as exemplifying changes of landscape engraved on the earth's surface by geological forces, that it might be supposed that the processes by which the Weald has arrived at its present condition were well understood.

This is far from being the case. Several theories have been advanced, but these are based on inferences lacking real justification; and, when it comes to describing the initial processes, no direct impression is conveyed to the mind, by which it can picture what the original condition of the landscape was supposed to be. In nearly every case the assumption has been made that the chalk was once continuous and covered the whole Weald: and this is followed by attempts to explain its subsequent disappearance between the North and South Downs. It is extraordinary that an inference with so little to support it should have held its ground unchallenged for so long a time.

It will be shown that a better explanation lies by way of anti-climax in the by no means improbable fact that this mountainous mass of chalk had never any existence. This explanation has the merit of providing a simpler and perfectly intelligible stage-setting to begin with, while it affords a clear grasp of the probable changes throughout the history of the Weald.

Seen in this light, the nature-lesson taught by the Weald is as wonderful as it is instructive.

The cue which has been missed lies in the recognition of the quasi-permanent character of the chalk *massif*, and its resistance to disintegration and erosion by frost and rain. One instinctively imagines the chalk that one is familiar with as a soft and friable stone; but compare the effects of rain on chalk, with a turf covering, and its effects on sandy or clayey strata such as are the outcrops in the Weald, and one will find that the chalk downs are so resistant to the wearing-down processes that they might in comparison be termed "the everlasting hills."

To this is due their now dominating position over the major portion of the wealden clays and sands, which, prior to the Quaternary period formed heights (calculated by Prestwich to be at least 2,800 ft.) from which fragments were washed down and dispersed over the present summits of the chalk, which at that time constituted the flanks of the wealden range. Or, in other words, these chalk elevations were once only the foot-hills encircling higher mountains. These mountains have melted away under erosive forces, while the chalk plateaus have been able to defy them.

The proofs of this process are numerous. Shortly stated, they are:

(1) The absence in the area of the Weald of flints, the product of the chalk, except in the neighbourhood of the escarpments.

(2) The non-existence of any residual flint deposits on the plateaus and summits such as would undoubtedly result from the removal of the soft parts of the upper chalk.

(3) The presence of chert and ragstone, and ferruginous sandstone peculiar to the Lower Greensand spread over parts of the highest of the chalk plateaus.

(4) The perfectly simple history of the 'transverse' valleys on this assumption. The explanation of these valleys is avowedly a great difficulty with all other theories.

Again, it is a constant feature in every chalk region that its boundaries are elevated above the bordering strata, generally with the same bold escarpments, which shows that, while other strata suffer from erosion, the turf-covered chalk offers a passive resistance to it.

It may emphasise the probability of the truth of the foregoing to say that the requisite proofs and 'exhibits' were

postulated, before they were obtained by personal investigation and by research, in former literature on the subject.[1]

As the question stands at present, the chalk either covered the Weald or it did not. The onus of proof that it did so will be seen to rest with those who make this assertion; so far the assumption has been unsupported by any real evidence.

The Downs and the Escarpments of the Weald

There runs from the Straits of Dover a horseshoe-shaped line of chalk elevation westward, which, passing by Guildford, trends south to Petersfield, where it takes an easterly course ending in the headland of Beachy Head, and forms a range of hills which, for their uniformity and general aspect, have no exact parallel in Europe. The reason of their singular character is due to the accident of having a unique origin, which stands revealed after a lapse of time, long, even as time is reckoned in geology. The area of the Weald, encircled by these chalk ramparts, cannot fail to strike the imagination as something of special significance, and to arouse speculation regarding the process by which it was formed.

For nearly a century it has provided a ground of debate for contending theories; and now there seems to be a general agreement that the chalk of the South Downs was once continuous with that of the North Downs, rising like a dome over the whole Weald, and that the intervening chalk by *some* process was subsequently swept entirely away. There is the difficulty!

Was this removal due to the operation of the sea, to the ice of the glacial period, or to the ordinary action of rain and river? While some refer it to marine action, others demand

[1] Whilst discussing some twenty memoirs in turn, on the subject of the denudation of the Weald, Mr. Topley, in his *Geology of the Weald*, quotes only one geologist, Mr. G. P. Scrope, who was opposed to the extension of the chalk over the whole Weald. He writes: Mr. Scrope saw no proof of this, but supposed that the chalk and greensands thinned away toward the central district, which "had been elevated above the sea *before* the deposition of the chalk, and had formed a ridgy island in that sea" (*Quarterly Review* on Lyell's "Principles of Geology," April, 1835). The only comment made by Mr. Topley on this opinion was that the resulting surface, when upheaved above the sea, would be very much the same as what is termed a "plain of marine denudation." It will be seen that the real difference is a crucial one. 'Denudation' being a technical term in geology does not express the process illustrated in the present article. The transformation of the Weald would be more correct.

the intervention of ice,[1] though there is no direct indication of either. The expert opinion seems, however, to favour a combination of sea action followed by a period of ordinary sub-aerial erosion, *i.e.* by rain, river and frost action.

The idea of the past continuity of the chalk over the whole Weald is generally illustrated by a diagram of a section of the Weald, which, being contracted out of all proportion, forces on one an erroneous impression as to the height of the chalk escarpments as compared with the breadth of the intervening area. This diagram, which shows the chalk, as supposed to be once continuous, rising to a height of some 3,000 ft. surmounting the Weald, is reproduced in most descriptions and tends to act as a misleading sign-post to every subsequent investigation. A section to scale is on Plate I taken from Topley's *Geology of the Weald*, in which the true proportions are approximately given, and the assumption of the continuity of the chalk appears far from being well founded. Had the chalk of the escarpments been a little further removed from the action of the anticlinal[2] upthrust, it would have been generally horizontal—even now the mean dip of the South Downs is very small—and the present theories would never have been held; certainly, not so firmly.

The Weald was undoubtedly a closed area. If one continues the direction of the points of the horseshoe across the Channel, they will join up with a small area round Boulogne (the *Bas boulonnais*), identical in its features with the Weald, showing that the whole was once entirely circumscribed by the chalk (see Plate II). Afterwards, owing to changes which resulted in the depression of its Channel portion in a synclinal[2] curve, the sea was allowed to penetrate, forming the Straits of Dover. We do not know when this occurred, but on this small dimpling of the earth's surface depended the whole course of England's history. Incidentally, but for this, there would probably have been no British Empire!

That is the staging. I now wish to suggest another interpretation than that given by the current theories regarding

[1] There is no doubt, however, that once at least during the maximum intensity of the Glacial Epoch (that of the Chalky Boulder Clay probably) the Coomb Rock and "Head" were formed by the melting of *local* ice sheets covering the south of England.

[2] Anticlinal = convexity of strata forming a mound, while in a synclinal formation the strata make a basin-like curve.

the past history of this area. It is that the chalk never covered the Weald, but that the sandstones, sands, clays, etc., of the latter once formed an island or bank, laved by the sea in the depths of which the chalk was being deposited ; so that when movements of the earth's crust gradually forced up the wealden strata to form the present anticlinal, the chalk fringing it was uplifted at the same time to form its skirting. The process is indicated by the slight dip north and south of the two lines of Downs : in the valley of the Ouse the steady rise from the sea at Newhaven to the escarpment of the chalk is well marked ; so is also the gradual drop from the North Downs to the Thames Valley.

Everything points to the beds subjacent to the chalk having been pushed up by the anticlinal thrust well above the general level of the chalk strata (see dip of greensand in Plate I) ; and it can be shown that this region, after its emergence, has in the main been subjected only to agencies of the simple character that prevail to-day.

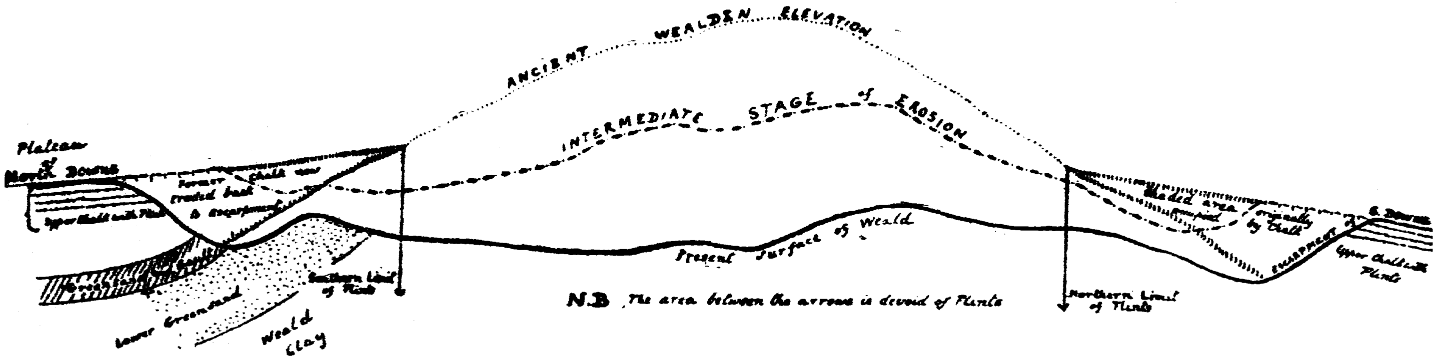
There is no need to call in the forces of the sea nor of the glacial epoch ; we have only to consider how it happens that the chalk, which at one time only represented the lower slopes of a mountain region, has now come to form an escarpment, from which the panorama of the Weald is viewed, as a wide basin, below it.[1]

There is an analogous case of an anticlinal area in North America, where a portion of the Appalachian range presents the same kind of ' transformation ' scene. High mountains thrust up by a similar wrinkling of the crust, and then worn away faster than the adjacent slopes, which formed their setting, have changed the mountains into a valley, while the more durable flanks have become the highlands.[2]

This ' differential erosion ' is exactly what has happened in the case of the Weald.

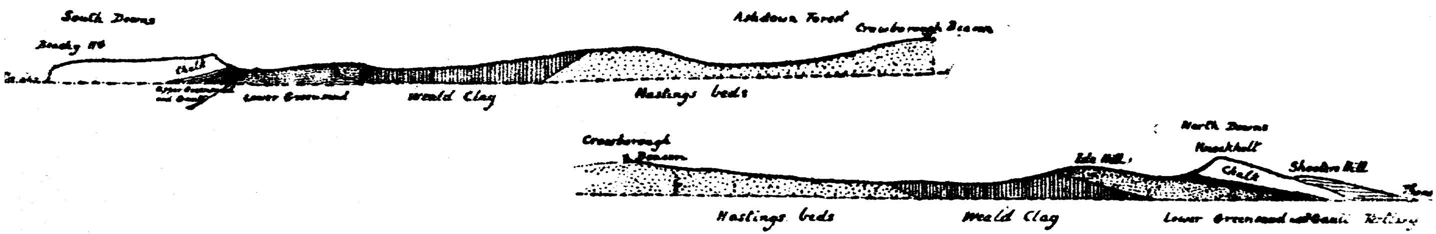
[1] Col. Greenwood, in his book *Rain and Rivers*, 1857, strongly insists on the fact that the Weald is really a *hill*, not a valley. It is only the great plain of the Weald Clay surrounding the central part that can in any sense be described as a valley.

[2] The process in this instance is thus described in Chamberlin and Salisbury's *Geology*: " In the structural adjustment which goes with the erosion of folds, it often happens that the valleys come to be located on the anticlines, after they have been worn down, while the outcrops of the hard layers on the flanks of the anticlines become the mountains."



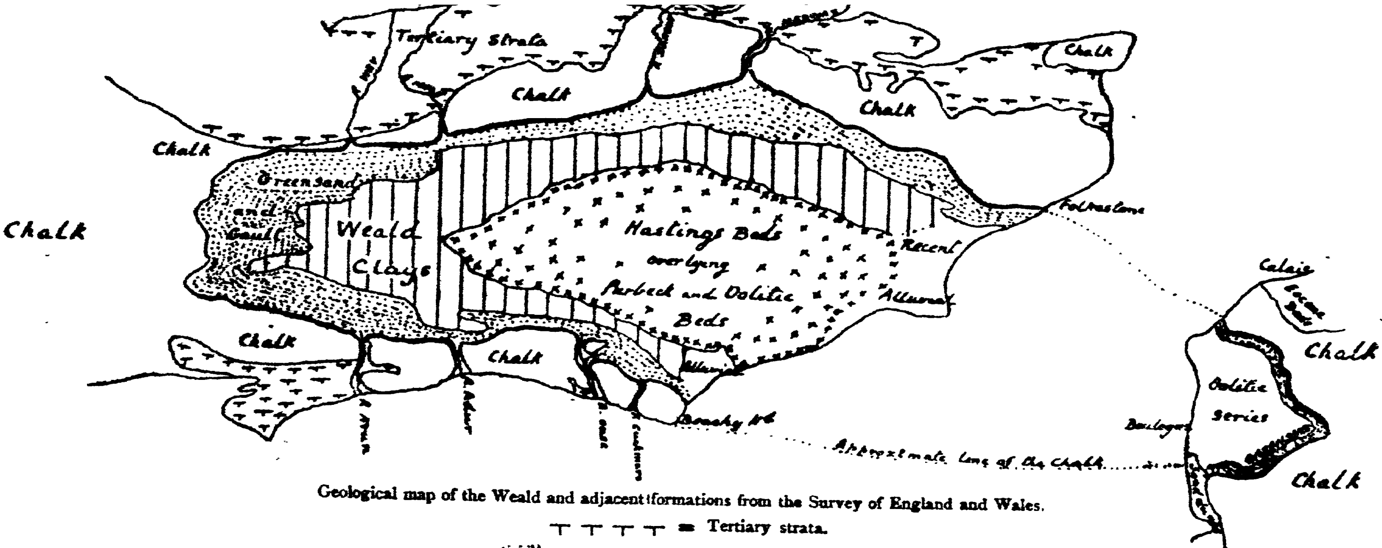
The “dip” of the greensands and gault is taken from the diagram, . 24 of the British Museum *Guide to the Antiquities of the Stone Age*. This diagram states that “on the North Down plateau surface re found ‘eoliths’ and fragments of chert and ragstone from the lower reensand outcrop.” The dip, if correct, will show on extension a nuch reduced space originally occupied by the chalk of the North Downs, which, in reality, seems to be the case.

Rough section from Lewes to Caterham, not to scale and contracted.



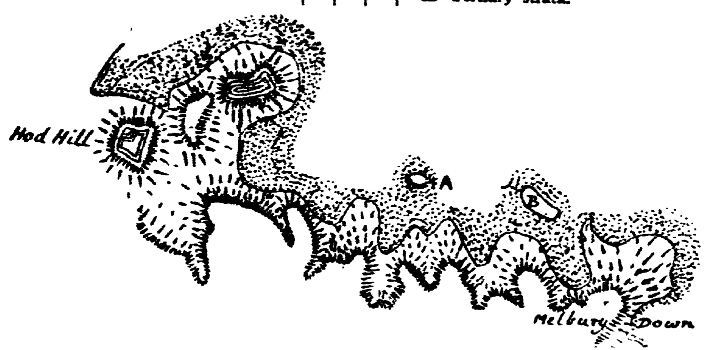
Section of Weald approximate to scale, from Topley's *Geology of the Weald*.

PLATE I



Geological map of the Weald and adjacent formations from the Survey of England and Wales.

T T T T = Tertiary strata.



Outline of Dorset-Wilts Chalk Frontier from Melbury Down to Hod Hill. A and B chalk outliers. Dotted surface = greensand.

On referring to the note in Plate I it is obvious that the cherts and ferruginous sandstone fragments peculiar to this greensand formation were washed on to the chalk plateau from an originally higher level, though now the greensand is well below it.[1] The significance of this appears to have escaped notice. The friable nature of chalk is well known, but its resistance to erosion when protected by turf or humus is not so apparent.

Before discussing this question of the chalk erosion, I would call attention to one feature of the section which strongly supports the interpretation here put forward, and furnishes a most striking proof of its general correctness, in that it so graphically acounts for the absence of flints, except along the original margins of the chalk, which rested on the flanks of the loftier wealden range. The limit of flint dispersion from the north escarpment is really less in the main than shown in this Plate.

How does the nature of chalk lend itself to the idea of its resistance to sub-aerial erosion?

In the chalk formation the question of erosion bears a distinct and remarkable character. Chalk is absorbent, and can contain its own weight of water. Consequently there are no elevated surface streams, and nothing to act as a constant carrying or denuding agent of surface material.

Apart from direct or flank attack by the sea and by rivers, chalk is mainly removed in solution by subterranean streams, hence its peculiar resistance to surface weathering and its 'dry' valleys.

The turf covering the chalk enables it to drink in water like a sponge, and thus to last for ages without reduction of general level. Rain, on falling, sinks down immediately until it meets with the impermeable stratum of the lower chalk, and then seeks a more horizontal exit, emerging as a spring from that level to wear back the escarpment and form coombs, or following the dip of the chalk to the sea, until it finds an outlet; thus initiating (and probably entirely forming) the 'dry' valleys, by carrying off lime in solution and thus causing the subsidence of the overlying chalk.

Mr. Topley, though he made the following observation,

[1] With the exception of Hindhead and Leith Hill, which are higher than the chalk plateaus. It is noteworthy that there are no flints to the south of Leith Hill, and that the contiguous chalk escarpment here has an unusually greater dip.

bringing him very near to the truth, failed to see its significance. The quotation is as follows [1]:

"But there is another point beside the relative hardness of the rocks. We find that the escarpments are composed of *porous* rocks, whilst the plains at their feet are generally formed of *impervious* rocks. Therefore a great part of the rain, which falls on the top, or dip-slope, of the rock forming the escarpments *soaks into it*, and has little or no mechanical denuding effect. But a part of this water issues as springs at the base of the escarpment, where the impervious bed occurs, and thus tends to wear back the face."

Again, on p. 265 in explanation of the *coombs*:

"At the foot of the escarpment there are frequently springs, some of them of considerable power. These do not break out at the prominent points of the escarpment, but in the receding curves. Where these curves indent the escarpment deeply they are called *coombs*. As a rule, the deeper and more strongly marked the coomb, the stronger the spring which it contains."

In the face of these clear statements, which indicate that the springs form the coombs, it is difficult to understand why the 'dry' valleys are considered not to be yet explained, nor why there is "hesitation" in accepting the idea of subterranean erosion.[2] (See Appendix, *Underground Erosion*.)

The subterranean passage of water is well known in limestone areas, where it causes sink-holes and caverns—though not infrequently in chalk districts a modified form of these also occurs—but with chalk it acts somewhat differently, and owing to less cohesion in the mass, brings about a gradual subsidence in the neighbourhood of the underground flow whereby the chalk assumes that rounded appearance of its ridges and the curves of its valleys which are so marked a feature of chalk contours.[3]

Were it otherwise, the valleys would not be dry as the upland valleys universally are, nor would they form these

[1] Topley's *Geology of the Weald*, p. 274.

[2] In the Lewes-Falmer Valley there is striking evidence that the upper chalk, with its flint strata, which now forms the top-soil, has been let down intact hundreds of feet as a gradual subsidence caused by the subterranean erosion of the chalk by the formerly invisible stream of the Winterbourne, and by its still hidden tributaries.

[3] The V-shaped valleys are assisted in their formation by faults.

modified 'curves of the catenary' except in accordance with the mathematical law regarding these particular forces which are brought into operation.[1]

If erosion attacked the surface as in other formations, the surface, which all over the body of the chalk in the south of England is mainly the upper chalk with its bands of flints at vertical distances of a few feet apart, would, on erosion of the soft parts, leave behind thick deposits of flints all over the surface.[2]

Now, whereas the lie of the chalk of the adjacent southern counties is comparatively horizontal, and the top stratum is generally the flint-bearing upper chalk, while nowhere are there deposits of residual flints left behind, as the insoluble products of any weathering agency, the natural presumption, startling though it is, is that we are treading the same surface that was exposed ages ago, and that the turf-covered chalk, from a surface point of view, is practically indestructible by ordinary erosion.

Further, it should be remembered that such residual flints, if found, would be very different in many cases from ordinary gravel, since where landslips have occurred the flint bed is seen often as a weighty network of branches running root-like in every direction, or perhaps in solid blocks sometimes of a hundred pounds weight, and in tabular sheets. Of such there are no signs, neither on the plateaus nor in the valleys.

This negative evidence is very strong. A few valleys contain deposits of flints, but they are not extensive, nor are the flints of any large size. Mixed with these deposits, however, we get occasionally the peculiar products of the lower greensand, as noticed by Mr. G. Clinch in *The Sculpturing of the Chalk Downs*,[3] who says:

"Generally speaking the 'dry' chalk valleys do not contain any considerable amount of deposits of hard and insoluble matter. Many, indeed, are quite free from such accumulations; but some are partially occupied by beds of

[1] It might be mathematically demonstrated that such curves *must* be caused by withdrawal of material from underneath them.

[2] This has occurred in the Hythe beds, which contain bands of Ragstone (often very cherty) and of Hassock, soft calcareous sandstone. Water percolating has acted mechanically and chemically, removing the softer parts, so that a residual drift of the insoluble parts of the Rag is left, forming a kind of gravel.

[3] *Geological Magazine*, vol. vii. February 1910.

rearranged chalk ; chalky matter interspersed with sand from the *wealden* beds : flints worn and unworn ; *ferruginous sandstone from the lower greensand :* and masses of sarsen-stone." (The italics are mine.)

These products peculiar to the wealden beds were before noticed with reference to Plate I, but the same is true of the Southern Downs. Close to the summit even of Firle Beacon (830 ft.), one of the highest points of all, I have found large fragments of the ferruginous sandstone of the lower greensand weighing several pounds, and one boulder of great size, of the same material, embedded in the detritus. This contains a large number of flints, but they also are foreign to the soil in which they rest. To find these fragments on a pinnacle, as it were, of the Downs, with the entire absence of signs of ice-action of any magnitude such as to transport them to this great height, points inevitably to the fact that they were washed down from mountains to the northward now swept entirely away.

This shows, moreover, that in the vast period during which the wealden mountains have been reduced to their present level, the plateaus and upper slopes of the chalk have so little changed that stones carried on to them in Pliocene times still repose on their surface.[1]

To stand on this elevation of Firle Beacon, amongst the debris of a still more lofty range, and to realise that all has completely vanished, leaving in its place an abyss of 800 ft. below one, is as sensational an experience, in its way, as any active exhibition of the forces of nature.

Not only in the Weald, but elsewhere whenever the various branches of the chalk formation abut on strata of another age, as in the Yorkshire and Lincolnshire Wolds, or even when uptilted and intermediate between Tertiary and Secondary rocks, as in the Isle of Wight, they dominate them, often with the same bold escarpments, thus showing fixity of tenure and greater endurance.

If we look at the outline of the chalk borders to the north of Dorsetshire (see inset, Plate II) we find the same high, abrupt, escarpment-like termination of the chalk which is so definite a feature of the Weald ; but, with this difference, that while there is nothing like the continuous even edge of chalk escarp-

[1] This also explains the frequent occurrence of palæolithic flint implements on the very surface. This has constituted a puzzle hitherto.

ment enclosing the Weald, there are, on the contrary, long peninsulas and island outliers of chalk; while the crest line follows a very sinuous course.

The reason for this disparity is this. In the Weald the absence of spurs and outliers of chalk is an indication that there was higher ground from which the streams descended, making frontal attacks on the chalk and spreading east and west until they found suitable exits, which have resolved into the main arteries of the present day.

Such a history certainly goes to explain this uniformity and absence of outliers, while it clearly indicates how the main valleys—so often quoted as presenting a geological difficulty—have been formed. This question of these river-valleys is dealt with below.

Starting with this presumption, one cannot fail to be struck with the difference in outline and aspect of the Dorset-Wilts chalk frontier, which speaks eloquently of an immunity from sub-aerial erosion possessed by the chalk summits, as compared with the Greensand and Kimmeridge clay flanking it.

Here it is evident that the erosion of formations adjacent to the chalk started at a fairly even level with the chalk. The same differential erosion was in operation; but rivers, only under a mild compulsion of gravity, constantly shifted their beds, enlarging a bay here, and leaving peninsulas and islands there, to follow the usual meandering habits of lowland water systems.

It is perhaps because, under such conditions, the leaving of outliers would be inevitable, that investigators have been hitherto misled as to the cause of the Weald denudation, and have been induced to infer that some other factor of a more sweeping character was responsible for the regular line of escarpment.

The Transverse Valleys

In our opinion the various causes of the denudation of the Weald are still to be sought, and the question may still be considered an open one. Especially do we think this to be the case when considering the point on which the whole of the question seems to turn, viz.: how to account for the gaps cut in the chalk escarpment to the north and the south.—F. R. Bennett, late of the Geological Survey, in *Ightham: the Story of a Kentish Village*" (The Homeland Association).

It is very difficult to visualise the process outlined by former geologists in their attempts to explain the denudation of the Weald, but one fact emerges from their assumptions, and that is, they one and all find the position and direction of the 'transverse valleys' to be a tough problem to work out.

Seeing that the prevailing idea among them was that of an original chalk 'dome' all over the Weald subsequently pared down into a pene-plain by the sea, removing the chalk and thus exposing the underlying anticlinal strata from which sub-aerial erosion has produced the present features, it is hard to understand why the results of differential erosion, advanced in this paper, should not have been recognised.

It stands to reason that if at one time the Weald, with its covering of chalk, was planed down to one level, thus removing the chalk 'dome,' the Weald clay, greensands and the chalk left on their flanks must have been exposed to the same conditions ; so that it follows that the present inequality in their elevation *must*, in that case, be the result of a much more rapid erosion of the sands, clay, etc., of the Weald, than of the chalk.

The gaps in the escarpment through which the rivers run —the transverse valleys—must have been created by the rivers themselves, as the chalk has only been subjected to a very small extent to 'fault' movements since it was laid down.

Therefore the present rivers are merely the descendants of the rivers which first found their exit to the sea through the chalk escarpments some 500 ft. above their present beds ; and one only has to substitute a hilly region of wealden strata for the planed-down chalk 'dome' to explain completely these transverse valleys and the debris of these wealden strata found lying on the summits and in some of the valleys of the Downs.

In this theory of an initial plain of marine denudation, in order to permit of an explanation of these north and south river-valleys, Mr. Topley, who is one of the chief exponents of the process by which denudation was effected, is obliged to postulate a slight elevation of the wealden beds above the chalk left on the flanks of the "plain of marine denudation" in these words : "In the plain of marine denudation we should have a *comparatively* plane surface *sloping from a central line*

to the north and the south" (the italics are mine). In other words, we should have to all intents and purposes a ridge of wealden beds *above* the chalk, with which I agree.

Breaks in the continuity of any deposit must constantly occur; and, if in order to account for the present non-existence of the chalk in the central Weald it is assumed that it never was there, one is on safer ground than in assuming a method of plain production by the sea, which is not even a plane surface; nor, as far as experience goes, the ordinary result of sea action, which tends to forming concave surfaces rather than convex.

One has only to postulate instead of a "comparatively plane surface *sloping north and south from a central line*," a ridge of wealden beds thrust up higher than their adjacent chalk to explain all the present phenomena, *ab initio*, without the intervention of the sea at all.

Mr. Topley very justly disposes of the probability of sea or ice-action having moulded the Weald features, from the absence of any of the indications usually left by ice-sheets or glaciers; and shows also how all the gravels of the Weald are such as would be furnished by the wealden strata themselves and by the relics of the worn-back escarpments of the chalk.

Though it is certain that during this long geological period, perhaps reaching back to the "London Clay," changes of elevation have taken place in the body of the chalk, there is no evidence of any great relative change in its several portions. Various problems still remain for investigation; viz. that of the Clay with flints, the Lenham Beds,[1] the Coomb Rock, etc.; but the new interpretation of the glacial periods, which I have given elsewhere,[2] together with a juster view of the processes that have been at work within the Weald, serves to explain this Rock, and incidentally to punctuate the immense antiquity of man, who has inhabited this area during a succession of climates each with its special fauna, and has outlived the span of existence allotted to successive species of elephant, with perhaps only one break in continuity

[1] These beds, which are found at different elevations on the Downs, have lately, from their fossil contents, been assigned to the Miocene period by Mr. Bullen Newton.

[2] *Changes in Climate* (The Glacial Epoch explained). Price 1*s*. 1*d*. (W. Baxter, Printers, Lewes.)

of his history,[1] since this final transformation of the Weald began.

APPENDIX

The following description of the plateau drift found on the North Downs Plateau, quoted by S. Laing in *Human Origins* (Watts & Co.) from a paper by Professor Prestwich, has a direct bearing on this denudation. He writes :

" Great, however, as is the antiquity shown by these relatively modern instances, they sink into insignificance compared with that evidenced by a recent discovery, which I quote the more readily because it rests on the authority of the late Professor Prestwich. It is afforded by the upland gravels in Kent and Surrey, which are scattered over wide areas of the chalk downs, at elevations far above existing valleys and water-sheds, and which could have been deposited only before the present rivers began to run, and when the configuration of the country was altogether different. Mr. Harrison, at Ightham in Kent, who is an ardent field geologlist, recently discovered what have been named eolithic, or pre-palæolithic

[1] There is no doubt in my mind that the Coomb Rock, in which Moustierian and other flint implements have been found, was a diluvial wash-down on the melting of a cycle of glaciation, which at one time gave rise to an ice-sheet spreading over the south coast of England. This was probably of local origin, and was caused by a greater intensity of glaciation, due to an increased eccentricity of the earth's orbit for northern hemisphere winters. Leverrier stated that such occurred 200,000 years ago, and a still greater eccentricity 850,000 years ago. From the supposed age of some flint implements in Kent's cavern—700,000 years—I am inclined to think the Coomb Rock was formed at the earlier date.

The significance of the enclosed Moustierian implements is very great under the point of view of man's antiquity ; for, from the general absence of wealden debris in the Coomb Rock, it seems to have been formed after the Weald had been worn down below the chalk level. Thus the pre-palæolithic implements (Harrison's), found on the North Down plateau, now, after a quarter-century of controversy, hall-marked as human, must date back to a time enormously more remote, *i.e.* before they were washed down from higher wealden mountains to their last resting-place on the top of the Downs. These flints comprise no weapon for the hunt or for offence, being in most cases for the purpose of rubbing down various parts of the body. This, besides expressing the paucity of human wants at that time, implies existence in an age before any glaciations had taken place—the sub-tropical Pliocene times—when body-hair was a sufficient covering, and vegetable diet was forthcoming at all seasons of the year.

These plateau implements, moreover, do not mark the beginnings of man's manufacturing power, since in many instances they are 'combination' tools, showing great ingenuity of adaptation. If a really primitive culture be sought, such will be found in the Darmsden flints, which from their crudity might well represent the dawn of invention. (See Vol. II., Part II., *Proceedings*, Prehistoric Society of E. Anglia, J. Reid Moir.)

implements in considerable numbers and in various localities in these gravels of the great southern drift at an elevation of 750 ft. above the sea-level. These discoveries led Professor Prestwich to institute an exhaustive inquiry as to these upland drifts; and the startling conclusion he arrived at is that the oldest of them, the great southern drift, in which the implements are found, *could have come only from a mountain range* 2,000 *to* 3,000 *ft. high, which formerly ran from east to west in the line of the anticlinal axis, which runs down the centre to the present Weald of Kent,* between the North and South chalk Downs, and which has since worn down to the present low forest-ridge by sub-aerial denudation" (the italics not in the original).

"The reasoning by which this inference is supported seems irresistible. The drift could not have been deposited by the present rivers, or during the present configuration of the country, for it is found at levels 300 ft. to 400 ft. higher than the highest watersheds between the existing valleys. It consists not only of chalk flints, but to a great extent of chert and sandstones, such as are found at present in the forest-ridge of the Weald and nowhere else. It must have been brought by water, for the gravels are to a considerable extent rounded and waterworn. Judging from the size of the rolled stones, this water must have travelled with considerable velocity; and it must have come from the south, because the cherts and grits are found only there, and because the levels at which the gravels are found are in that direction. By following these levels as far as the present surface extends, which is to the southern edge of the greensand, it is easy to plot out what must have been the continuation of this rising gradient to the south, and what the elevation of the southern range, in which these northward-flowing streams took their origin. Prestwich has gone into the question in full detail, and his conclusion is that the height of this wealden ridge must have been at least 2,800 ft., or, in other words, that about 2,000 ft. must have disappeared by denudation.

"This is the more conclusive because Prestwich approached the subject with a bias towards shortening rather than lengthening the periods commonly assigned for the glacial epoch and the antiquity of man.

Underground Erosion

From the following it will be seen that the official view has not yet accepted the idea of subterranean erosion.

In *Ightham: The Story of a Kentish Village*, Mr. F. J. Bennett, inserted as an addendum, p. 130:

"It is a matter of much satisfaction to me that Prof. Gosselet's *creuses* should occur in a chalk area, as I have long held that the dry chalk valleys are largely due to subsidence from underground erosion, and have tried to get this opinion officially recognised, but without success.

"The solution of the problem afforded by these *creuses* has, as he states in his paper, occupied Prof. Gosselet during the ten years that he has been making the geological map of Artois; and, after much hesitation, he has always come back to the idea that they are due to subsidence arising from combined surface and underground erosion, but more especially to the latter, which he considers hitherto to have been underrated."

As regards the sinuous valleys, we have this opinion expressed by Prof. Prestwich which has a bearing on the question, in his *Water-bearing Strata of the Country round London.* He says "that streams and rivers may be considered as representing (but only in porous strata) in definite lines on the surface, a water-flow agreeing in its general direction with that which takes place, bodily, in the strata below."

A still more definite conclusion was formed by M. D'Archiac in his *Mémoires de la Société Géologique de France.* This geologist paid great attention to the subject of the water-bearing strata of the Tertiary and Cretaceous series of France, and confirmed, as the result of his experience, the rule laid down by the Abbé Paramelle, on perfectly independent grounds, in his observations on springs, viz.: that the subterranean currents of water follow the same law, with reference to their course, as those which flow on their surface.

The Durability of Chalk Earthworks

Many archæologists have remarked on the permanence of outline of tumuli and earthworks made in the chalk. The resistance of the chalk to sub-aerial erosion is well illustrated by the permanence of the huge figures outlined on the chalk slopes, such as "The Long Man" of Wilmington, Sussex, and "The Giant" of Cerne Abbas in Dorsetshire.

Both these figures, which have no depth, but merely outline, take us back most probably to the Druidic cult of Bel (Baal), the Sun God.

In spite of their exposed position to the elements and on steeply inclined slopes of chalk upland, they have not been eroded, but have remained, but for the growth of herbage, as definite as when made.

The ramparts also of Maiden Castle (near Dorchester), be-

lieved to be of much greater age, which were possibly, though not necessarily, repaired in Roman times, point as well to immutability of sectional outline in positions, too, where the full force of the elements has been operating for a time sufficient, with ordinary earthen materials, to merge to a large extent the outline of rampart and fosse. This really stupendous work is formed of a triple ring of sharply angular ridges and ditches as much as 60 ft. deep in many places, which show no accumulation of detritus such as one would imagine must accrue from a slope of some 40 degrees, and for the greater part of its two miles of circumference is extremely well preserved. The grass slopes are now being patched with turf in some places, but I ascertained that the bottom of the ditches, which are still almost V-shaped, has remained untouched.

THE ELECTROCULTURE OF CROPS

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"THE electrification of growing vegetables was first begun in Britain. Mr. Maimbray at Edinburgh electrified two myrtle trees during the whole month of October 1746, when they put forth small branches and blossoms sooner than other shrubs of the same kind which had not been electrified. Mr. Nollet, hearing of this experiment, was encouraged to try it himself."

This quotation from Joseph Priestley's *History and Present State of Electricity*, published in 1767, records the simple experiment which formed the introduction of the subject of electroculture, and which was soon to be followed by numerous other experiments, which were repeated again and again at intervals, and are still being repeated in our time.

The subject is one the development of which through its various phases is particularly interesting to follow, not because of the achievements of the investigators therein, but on account of the light that is thrown on the factors making for success or failure in the solution of scientific problems.

It is instructive in the light of our present knowledge to take a survey of the field of investigation as it is presented by the works of Priestley and his contemporaries, who worked and wrote when experimental science was still in its infancy. It is interesting to note the variety of observations made by these "philosophers" who were engaged in this preliminary survey of natural phenomena. To us who live in an age of specialisation in study this method of attack may appear strange and wasteful, but it is rather astonishing to realise the acuteness of the observational powers of these philosophers, by whose efforts were thus collected a large number of elementary observations, many of which formed the nucleus for enormous development.

And the early method of investigation may not have been so wasteful after all, for we know how, to-day, fresh fields of investigation are opened out by new combinations of subjects.

In reviewing, then, the course of investigations in the time of Priestley, we find that the subject of electroculture was as favourite a one for examination as other branches of electrical science. We cannot avoid asking ourselves, therefore, how it is that while the study of electricity and its many industrial applications has developed into enormous importance, electroculture in the meantime has remained practically stationary for a century and a half, and this in spite of its obvious economic importance.

We probably find the answer to this question in the stagnation of the science of the living plant. The development of electroculture depends not only on the development of our knowledge of pure physics, but also on the development of our knowledge of the activities of the plant. While physics has developed so rapidly, the science of the living plant remains very much where it was when Woodward and Stephen Hales performed their experiments. While some excuse for this may be found in the political and economic conditions which have determined the position of agriculture, the main reason for this state of affairs must be that the science of the living plant has not attracted the genius which has been bestowed on electrical science for instance.

For the sake of simplicity we shall only deal in this article with the form of electroculture in which electricity is discharged through the air to the plants from an overhead wire system, kept charged at a high potential by an electrical machine, or simply charged by atmospheric electricity collected at a higher altitude. This is the only form of apparatus for electroculture which has been employed on anything like a commercial scale, although very numerous experiments have been also made by passing currents through the soil in which the experimental plants are growing.

The subject, even with the restriction which we have indicated in the last paragraph, has such an enormous literature that we shall only attempt to cite the work of a few investigators typical of the various periods in which they made their observations. From these typical investigations we shall see how it appears to be manifest that electricity in certain

cases exercises a remarkable influence on plant life, but that, on the other hand, many observers, with apparently as much justification, insist that electricity, if it has any influence at all on plant growth, has a harmful influence. The same discrepancy appears both in the case of small- and large-scale crop experiments. We shall attempt to correlate this discrepancy with other plant physiological investigations and quote parallel examples from recent electroculture research, and thus we hope to impress on the reader that this discrepancy is probably not due to a fault on one or other side of the authors of these discordant observations, but to a lack of realisation on both sides of the exact position of the problem—a lack of knowledge of the life of the living plant, and a lack of knowledge of the experimental conditions of the electric discharge employed. This discrepancy, which has lasted for a century and a half, is not likely to be removed before a changed outlook is brought about, but as soon as this results it seems likely that out of all this apparently futile research on electroculture may arise a knowledge useful to mankind. This is likely to be the case not only in the particular example of electroculture, but in all that concerns stimulation of plants.

The first detailed description of experiments comes from the Abbé Nollet, the French court physicist, who, hearing of Maimbray's experiments, tried some experiments himself in the following year, 1747. Nollet filled two pewter vessels with similar samples of earth, and in the two vessels equal quantities of mustard seed were sown. After two days one of the vessels was subjected to the influence of the electric discharge for about ten hours—namely, from 7 a.m. to 12 noon, and 3 to 8 p.m. The other vessel was kept as control in the same room and at the same temperature, Nollet pointing out the usefulness of M. Réaumur's invention for measuring temperature. The next day both vessels were exposed to the sun. On the day following three seeds had germinated in the electrified vessel and produced seedlings three lines high; in the non-electrified control no seeds had germinated. The experimental vessel was again electrified in the evening, for three consecutive hours; the next morning it was found to contain nine seedlings seven to eight lines high. At this time still no germinations had taken place in the control vessel, although towards evening the first seedling appeared in it. In the afternoon of this same

day the experimental vessel was again electrified for five hours, and electrification was continued up to the eighth day. At that time all the seeds in the electrified vessel had germinated and were fifteen to sixteen lines high, while in the control vessel only two or three seedlings had appeared, and these were no more than three to four lines high.

Similar observations on the stimulation by means of electricity of the plant in various stages of its life—germination, the growing period, opening of dormant buds—were made by numerous observers at this period, for instance Jallabert, Menon, and Nürnberg.

About this time atmospheric electricity had become a favourite subject of investigation, and we find suggestions to the effect that atmospheric electricity is an important environmental factor in the life of the plant. Thus Father Beccaria of the University of Turin, writing in 1775, says: "With regard to atmospheric electricity it appears manifest that nature makes an extensive use of it for promoting vegetation"; and again: "Besides, the mild electricity by excess, (positive electric action of low tension,) which, as I have observed for these many years past, constantly prevails when the weather is serene, certainly contributes to promote vegetation, in the same manner as experiments have shewn us that this is likewise the effect of artificial electricity *without sparks*. And is it not likely that the former kind of electricity promotes vegetation still better than the latter can do, since nature increases it and lessens it in such circumstances and at such times as particularly require it?" Similar views were put forward at the same time, and with perhaps even greater force, by the Abbé Bertholon, who designed an apparatus, the electro-vegetometer, for collecting atmospheric electricity and distributing it over growing crops.

There were, however, a few observers who concluded from their experiments that electricity was either harmful to vegetation, or at least did not stimulate it. The earliest of these appears to be Koestlin, who in 1775 reported that negative electricity is harmful to vegetation; but probably it was the testimony of the famous Dutch physicist Ingen-Housz which carried most weight, and who on account of his repute as a plant physiologist has been often quoted in favour of the theory that stimulation of vegetation by electrical means is

impossible. However, as Ingen-Housz connected his plants directly with the collectors, and he describes the plants as shrivelling up after treatment, his experiments simply indicate the possibility of killing plants by electricity.

In 1789, the year following Ingen-Housz's more elaborate experiments, D'Ormoy found that the electric discharge stimulated the germination of mustard (and lettuce) seed. This, it will be observed, was Nollet's original experiment.

The few experiments we have cited of the many performed in this first period of half a century are not only typical of the eighteenth century, but we find the same repeated in the nineteenth century; the experiments differ only in the people who perform them, in the means of producing the discharge, and in their being usually conducted on a larger scale; but in all we find the same absence of any realisation of the actual position of the problem.

We find that a lively interest in the action of atmospheric electricity on the plant had developed in this country before the end of the first half of the nineteenth century. Thus Forster in 1844 reported the results of some experiments with chevalier barley in which atmospheric electricity was collected by a horizontal wire and conducted to the soil by vertical wires at either end of the horizontal one. Forster found the electrified crop assumed a darker green colour and grew more rapidly than the non-electrified control, while the yield of grain from the electrified plot was double and the yield of straw triple that of an average crop. In 1846 William Sturgeon, lecturer at the Manchester Institute of Natural and Experimental Science, went so far as to say that Forster's experiments "have commenced a new era in electro-cultural enquiries; and their flattering results have induced several persons, electricians and others, to try the same plan on crops of various kinds of the last year's growth." However, it was no new era that was inaugurated by Forster's experiments; it was simply a repetition of older experiments that resulted, the methods employed often being less refined than those of earlier workers. Nevertheless, Sturgeon's paper on the Electroculture of Farm Crops, published in the *Journal of the Highland and Agricultural Society* for March 1846, contains some very trenchant remarks on the position of the subject. At that time the importance of manuring was coming into great prominence as a result of

Liebig's researches and writings, yet Sturgeon realised that, in spite of the obvious benefits that were resulting from the application of pure chemistry to the questions of soil and plant, a greater knowledge of the life-activities of the plant would be required before agriculture could develop to its fullest extent. As this writer has been completely neglected, we feel justified in giving one or two quotations from his paper. He says, for instance, "By what powers, or by what physical forces, do the organs of plants display, and keep in operation, their respective functions of vegetable life, is a problem of vast importance in the basement of agricultural science, and in every other branch of vegetable culture. This grand problem, the solution of which has not yet been accomplished, nor, indeed, scarcely attempted, presents the most formidable, and, at the same time, the most noble bulwark yet to be assailed in our inquiries respecting the functions of vitality in the vegetable kingdom." And again: "The rules of his art will always enable the practical chemist to be of much service in providing food for plants, although it may require a higher order of investigations than those he is in the habit of pursuing to discover the character and operations of those forces which stimulate the organs of vegetables to avail themselves of the food thus supplied for their use."

However, in spite of these laudable views put forward in 1846, the subsequent history of electroculture is little else than a repetition of the earlier. An enormous number of researches have been conducted on the subject, but the vast majority are on exactly the same lines as the older ones, and the results are similar, *i.e.* the majority show favourable influence on germination, growth, and final yield resulting from electrification, a minority show no such improvement resulting. No leading principles are brought out and no contributions from other branches of science throw light on the subject.

It was during this period that the science of the physiology of plants was obtaining a certain amount of recognition, but unfortunately instead of developing along its own lines as the science of the living plant, and evolving its own guiding principles, it became subservient on the one hand to morphological botany, on the other to chemistry, with the result that although much has become known about individual processes in isolated organs over short periods of time, and still more con-

jectured, we know next to nothing about the inter-relation of the various processes which make up the life of the plant and their variation in different phases of that life. It is no wonder, therefore, that the scientific agriculturist has always been very sceptical of the results obtained in plant physiology, and that there has resulted an apparent discrepancy between the results obtained in agricultural practice and in physiological experiments.

Hence it is not surprising that in regard to the contributions of plant physiology to electroculture there is very little to be said. In all the physiological researches conducted in order to solve problems of electroculture there is a lack of realisation of the electrical problems involved and a neglect to inquire into the progress of pure physics, so that there is a general idea that it is unessential in these experiments to trouble about the conditions of discharge of electricity, and that results obtained, for instance, from experiments with currents of low E.M.F. through germinating seeds can be used as arguments in regard to experiments with electric discharge through air on actively growing and maturing plants.

A favourite subject of investigation by the physiologists was the effect of electricity on *protoplasmic movement.* In 1837 Amici and in 1838 Becquerel and Dutrochet studied the influence of an electric current on the movement of protoplasm of *Chara vulgaris.* Similar experiments were conducted at a later date by Heidenhain (1863), Kuhn (1864), and others, on protoplasmic movements in the leaves of *Vallisneria.* Similar experiments have been performed up to our own day, but nothing fruitful for our subject has resulted from them.

A few investigators have attacked the problem from the point of view of determining whether the beneficial effect of electroculture is due to an acceleration of the *assimilatory process.* For this purpose a current was passed through a piece of a water plant, as by Thouvenin (1896), or through an aerial leaf, as by Pollacci (1907), and in each case an increased rate of assimilation was recorded as a result of the passage of the current. But these experiments are open to so much criticism that it is impossible to draw any conclusions from them. This holds also for arguments derived from *in vitro* experiments, such as those of W. Löb (1905), who argues from the supposed formation of formaldehyde from carbon dioxide

and water under the action of the silent discharge, to the increase in crop production resulting from the electric discharge.

It was proposed by Gassner in 1909 that the beneficial effects so often observed as a result of the electric discharge are chiefly due to an influence on the *transpiration* rate. This writer observed that more than twice as much water was transpired by the plants subjected to the electric discharge than the non-electrified control plants. He suggests that this is simply brought about by the formation of air currents by the silent electric discharge, and these alone would be sufficient to explain the increase in evaporation. However, the general criticism we have levelled against all the physiological experiments dealing with electroculture hold for these as for all the others. But that the water relations of the plant are influenced by the electric discharge is an opinion fairly generally held. Thus Nollet was struck with the more rapid rate of evaporation from electrified liquids than from non-electrified.

The *respiration* of plants under various electrical conditions has formed the subject of an investigation by Knight and Priestley published in *Annals of Botany* for 1913. However, these various electrical conditions are not those of the actual electrocultural experiments in the field, and the results of many of the experiments of these authors, as they themselves admit, are of dubious interpretation on account of the experimental arrangement.

Recently a paper appeared by R. Stoppel in which the cause of the *movements* of the leaflets of *Phaseolus multiflorus* was traced down to atmospheric electrical changes, and the author from this observation proceeds to far-reaching generalisations as to the importance of atmospheric electricity in the life of the plant.

We see, therefore, that the physiological investigations in reference to electroculture have dealt with numerous plant processes: assimilation, transpiration, respiration, irritability, protoplasmic movement. In no one case have the experiments been conducted in such a way that they give us any information as to the influence of any definite electrical conditions on any one of these processes at any definite stage in the history of any plant or any plant organ. How much farther off are they therefore from even suggesting a solution of the problems of electroculture?

Exactly similar criticism can be levelled against those who regard the benefit resulting to growing crops from the electric discharge as due to changes brought about in the soil; with this school it is therefore not necessary for us to deal further.

It is strange that the greatest and most remarkable contribution to electroculture should have come from a physicist, namely S. Lemström, who was Professor of Physics in the University of Helsingfors. His work is remarkable not only because it was the first in which comparatively large areas of land under crops were subjected to treatment, but because he pursued the subject with great energy through his lifetime right up to his death in 1905. He carried out experiments in many countries under a great variety of different conditions, and so collected a great deal of empirical information as to the conditions under which the discharge affected the life of the plant. Further, he made many experiments with the object of discovering the manner in which the discharge affected the plant, but in this matter his lack of knowledge regarding the life of the plant prevented him from expressing himself in a way which would appeal to plant physiologists or scientific agriculturists. Nevertheless, it is likely that some of his observations and ideas may prove to be sound when correlated with later experiments. His researches make it clear that the overhead electric discharge will affect the life of the plant in all its phases: germination, vegetative growth, and maturation. Lemström in his experiments used the unidirectional discharge from an influence machine, but he obtained favourable results when either the positive pole or the negative pole of the influence machine was connected to the overhead network. Similarly he was able to obtain favourable results whether the discharge was applied in the day-time or at night. He sums up his experience, however, by stating that the best results are obtained (1) with the network positively charged; (2) by applying the discharge in the morning and the evening; and (3) by having the general conditions favourable for plant growth.

Although Lemström used an influence machine giving a very high potential, the overhead network in his experiments was only charged to a few thousand volts or even less. That neither he nor subsequent investigators should have derived advantage from this observation for the construction of more efficient apparatus is at least surprising. All experimenters

appear to have been obsessed with the idea that extremely high voltages are necessary, while at the same time in the construction of apparatus they neglect arranging for sufficient output of current.

It is unfortunate that none of the physicists who associate their names with electroculture investigations should have taken the trouble to work out the physical questions involved. It was left to a pure physicist, working from quite another point of view, to put forward the considerations which enable us to formulate the conditions of the discharge (see J. S. Townsend, *Phil. Mag.*, 1914).

That the publication early in this century of Lemström's book on Electricity in Agriculture and Horticulture, translated into several languages, should have given rise to a large number of fresh experiments is not surprising. It is unfortunate that neither plant physiology nor scientific agriculture should have been sufficiently advanced at this time to give their contribution to the subject. As a result recent development has consisted almost entirely in the application of all possible devices for the production of high-tension current which electrical industry has evolved for other purposes. However, a certain amount of empirical information has been collected, for instance, in this country by Mr. J. E. Newman, Mr. William Low, and Miss E. C. Dudgeon, but our knowledge of the subject is not greater than when Lemström left it, in spite of improvements in apparatus.

We hope we have made it clear in the foregoing pages that the history of electroculture presents an ever-recurring cycle of experiments having as their object the proof or disproof that the electric discharge has a beneficial effect on vegetation. That both these results should be obtained regularly in the cycle should be sufficient to inform us that there must be something fundamentally wrong in the method of inquiry. This, in our opinion, is to be found in the neglect of quantitative measurement of the discharge, and in the lack of knowledge of the science of the living plant. This may perhaps be more easily understood if we give a few details of a parallel series of investigations by H. Molisch published in 1912.

Molisch was concerned with the influence of radium emanation on plant life. We find in Molisch's work a realisation of the principles of the application of stimuli, that the effect

depends on the intensity of the agent and the length of time during which it is applied; further, that the effect of the stimulus may first appear a considerable time after the application. Molisch gives evidence that he possesses a good knowledge of the living plant; thus he realises that the same agents react quite differently on a plant in its resting period, in its period of germination, and during its active growth.

Molisch, therefore, from his first empirical experiments which showed that radium emanations would induce growth in a resting organ (flower-buds) in winter time, does not conclude that radium emanation will always have a beneficial influence on plant life. He determines by a carefully planned series of experiments that a definite quantity of radium emanation applied for a definite period will induce the opening of flower-buds; but if the organ is not in its resting condition, application of the emanation makes no difference in the rate of development. Further series of experiments deal with the action of various quantities of emanation applied over definite times, to germinating seeds or growing seedlings. It was generally found in these cases that a much smaller quantity of emanation than that applied to resting organs has a distinctly harmful effect and hinders development, but in the case of certain species, with extremely small quantities of emanation a slightly increased rate of growth was observed.

It is not surprising that such a series of experiments should have given rise all over the world to experiments on the value of radio-active manure, but it is remarkable that some of the investigators, in whose experiments an arbitrary and unknown quantity of agent was used and which was allowed to act throughout the whole season, should argue from the negative results obtained by them that radium emanation is without influence on plant life, and that Molisch's experiments are consequently disproved.

There thus seems to be the possibility of the production of a cycle of experiments, dealing with the use of radium emanation, similar to that which already obtains in regard to electroculture, and this in spite of Molisch's original well-planned work.

If we now take in review the electroculture experiments and consider them in the light of the work on radium emanation with which we have just dealt, we find that all investigators

show the same lack of realisation (1) of the necessity for quantitative measurement of the electric discharge, (2) that a stimulus may act differently on the plant at different stages of its life, (3) that the effect of the stimulus depends on its intensity, (4) that the effect of the stimulus depends on the time for which it is applied, and (5) that the effect of the stimulus may appear a considerable time after it is applied. Numerous examples of this can be quoted.

That in the electric discharge we possess a means which can be used as a stimulus at most periods of plant life seems clear from the greater number of experiments, but how indifferent, or even hostile, plant physiologists have been to the general conception of stimuli is perhaps best illustrated from some experiments of Gassner on electroculture published in 1907. First of all he finds that with *Pisum* and *Helianthus* the electric discharge has no influence on germination and growth, the electrical treatment lasting eight to fourteen days, and being used for fourteen hours daily. The electric discharge was so strong that light phenomena appeared round the plants and the plants were black on account of precipitated dust. But with barley he concludes that the discharge has a favourable influence. We may quote in detail one of his experiments on this plant. The electrified series consisted of three pots where the distances between the discharging points and the plants were respectively 10 cms., 21 cms., and 35 cms. On March 12 thirty barley grains were sown in each pot. On March 16 the seedlings began to appear, and on the same day the discharge was started. On March 17 all the seedlings had appeared; no difference was visible between any of the experimental pots, nor between experimental and control. On March 18 the electrified were visibly in advance of the control and more in advance the smaller the distance between the discharging point and the plant. On that day the first leaf had appeared in 16 plants in the pots 10 cms. from the discharging point, 12 in the pot 21 cms. from the point, and 4 in the pot 35 cms. away; the numbers for the two controls were 1 and 3. In the further course of the experiment the electrified developed quicker than the control, but soon the plants farther away from the discharging point were ahead of the others. On March 27 the experiment was stopped.

Consideration of Molisch's experiments will render these

observations readily understandable. It is clear that Gassner was dealing with a phenomenon of the same type as Molisch, in which the intensity of the agent and duration of its action determine the result. It is regrettable that such a view should not have presented itself to Gassner and that he should simply have attempted to explain his experimental results in the light of a single process, namely, evaporation of water.

Limitation of space has prevented us in this article from going into detail in regard to the very numerous experiments which have been performed on electroculture. We hope, nevertheless, we have made clear our main object, which is to emphasise that the condition for development of electroculture is a sounder outlook in plant physiology.

It is true that in the past the contributions of plant physiology to our subject have been negligible, and what advances have been made have come from the physicist. Nevertheless, although a knowledge of physical methods is essential for intelligent research in electroculture, the problems involved are essentially problems of plant physiology. With a sounder outlook in plant physiology, the apparent discrepancy between plant physiology and agriculture should disappear, and great developments in the methods of crop production should take place. It is only when research, based on this new outlook in plant physiology, has become established, that we shall be able to judge how far the application of stimuli, including the electric discharge, may be of economic importance in the production of crops.

LONDON,
November 2, 1917.

POPULAR SCIENCE

THE STRUCTURE OF MATTER

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PART II

Structure of the Atom

All modern theories of the structure of the atom are essentially electrical in nature. That is, an atom is regarded as consisting of small particles of matter, each carrying an electrical charge. Before entering on the problem of atomic structure it is necessary to recall very briefly one or two points about electricity itself which are of importance for our present purpose.

It has long been known that any substance which has been rubbed by a dissimilar substance exhibits the property of attracting to itself light particles of matter, such as paper or cork. The substance which has acquired this property is said to be electrified. Since the attraction can be made to vary in amount (by altering the extent of the rubbing or by the use of different materials), we infer that there must be such a thing as *quantity* of electricity. Very early experiments also demonstrated that two pieces of the same material when rubbed with a second substance exhibited towards one another a certain repulsion or repulsive force. On the other hand, each piece vigorously attracted the substance with which it had been rubbed in the first instance. Such observations lead naturally to the idea that electricity is of two kinds, called positive and negative respectively, and further, that two pieces of matter carrying electricity of the same sign repel one another, whilst oppositely charged pieces attract. Electrical charges of various amounts can be given to a piece of material, and can be removed from it by suitable means. If

two substances highly charged with electricity, the one carrying positive electricity, the other negative electricity, are brought sufficiently close together, a spark may be observed to pass from one to the other, as a result of which the materials are found to possess no charge whatsoever, provided the initial amounts of positive and negative electricity were the same. It is possible, therefore, to discharge the electricity from a substance by neutralisation with electricity of the opposite sign. It follows that an uncharged or electrically neutral substance does not necessarily contain no electricity at all ; it may really possess equal amounts of the positive and negative kinds. Such early observations and deductions do not give us, however, much information about the mechanism of the process of electrification. The charges of electricity here considered reside essentially on the surface of the materials examined. The first evidence that electricity could be transported through the body of a solid was obtained when an electrical current was propagated through a wire by joining the two ends of the wire to the poles of a battery. That an electric current, or electricity in motion exists under these conditions is shown by its effect on a small magnet suspended close to the wire, the magnet being rotated through a certain angle when a given quantity of electricity passes through the wire. The modern view of an electric current is that it consists of a stream of very tiny material particles called *electrons*, or corpuscles, each of which carries an electric charge. The sign of this charge has been shown in various ways to be negative. These electrically charged electrons existed in the wire all the time ; their movement from one end to the other was brought about by joining the wire to the battery, one pole of which acts like a reservoir head applying a certain electrical pressure or potential which causes the electrons to stream just as water streams from high pressure to low. Since the electrons are thus shown to be present in the wire, they must constitute part of the atoms of which the wire is itself composed. In virtue of the fact that it allows electricity to pass through it, a metallic wire is called a conductor, its conducting power being due to the presence of electrified particles distributed throughout its mass. But wire is by itself an electrically neutral piece of matter. It follows therefore, that there must be exactly as many positively charged particles as

there are negative. We now know, as a matter of fact, that in a piece of metal some of the atoms have each lost an electron, so that the atomic residues are positively charged, the charge being necessarily the same in amount as that carried by the detached electron.

Certain aqueous solutions, notably solutions of acids, bases, and salts, likewise allow electricity to pass through them. They are also called conductors, but the mechanism of the process is known to differ from that in metals. In the case of solutions the carriers of electricity are particles identical in mass with atoms, of which some are charged positively, some negatively. The solution as a whole is electrically neutral, there being just as many positively charged atoms, or ions as they are called, as there are negatively charged ions. These ions are present all the time in such solutions. Their presence is shown when we cause them to move by putting on an electrical pressure or potential through the intermediary of electrodes. It is then found that the positive ions move in one direction, the negative in the opposite. Each ion is attracted towards the electrode which has electricity opposite in sign to that carried by the ion. When an ion reaches an electrode it gives up its electricity to the electrode, the ion becoming thereby discharged. It is now simply a neutral atom. Several things may happen to this atom; *e.g.* it may be deposited on the electrode as in electro-plating, or it may go off in the form of gas, as happens when we electrolyse a solution of common salt and obtain gaseous chlorine at one of the electrodes—each molecule of chlorine, by the way, consisting of two atoms of chlorine which have united with one another after being discharged at the electrode—or finally, the discharged atom may react with the water, as happens in the case of the sodium atom, giving rise to new reactions such as the production of hydrogen gas and caustic soda, the latter in the dissolved state. The essential difference between the conduction of electricity in a metallic solid and that in a solution is the fact that no chemical or other change is produced in the metal, whilst marked chemical changes may be produced in the solution.

Such facts as these lead us to think of electricity as something which may be added to or taken from an atom or molecule—the latter being simply two or more atoms linked together.

Recent investigation has shown, however, that we cannot add or subtract any amount of electricity we please. There is a certain limiting amount below which we cannot go. This amount is, in fact, identical with that carried by a single electron. Electricity, or, more properly speaking, electrical charge, is built up of certain natural units, each unit being identical in magnitude with the charge on an electron. That is, electricity has a discrete structure, just as matter has.

The discovery of electrons and their rôle in electrical phenomena was made by Sir J. J. Thomson. It was shown that electrons constitute the cathode rays, a stream of very fine particles which is produced when an electric discharge is passed through a gas at low pressure. The stream of electrons is made manifest by a glow in the tube, the glow itself being due to the collisions of electrons with gas molecules, the latter suffering ionisation. The stream could be deflected by a magnetic field, showing that the particles composing it were electrically charged, and the direction of the deviation indicated that, on the usual convention of positive and negative electricity, the electrons were negatively charged. In addition to the cathode rays, in which the electrons are produced from the matter composing the electrodes, we likewise meet with their production from radio-active elements, from the alkali and alkaline earth metals, and in general from any material which is "excited" in a suitable manner, *e.g.* by exposure of the material to short wave or ultra-violet light. It seems natural, therefore, to regard the electron with Sir J. J. Thomson as "one of the bricks of which atoms are built up."

The existence of this natural unit of negative electricity having been demonstrated beyond doubt, the question naturally arises—is there a similar unit of positive electricity? So far, the answer is in the negative. We have not yet encountered a natural unit of positive electricity which is indivisible and capable of independent existence as in the case of the electron. At the present time ***positive electricity really denotes absence of a certain amount of negative electricity***, i.e. absence of a certain number of electrons. Thus, it is possible to remove an electron from an atom, say an atom of sodium, leaving us with a positively charged atom-residue or ion. The quantity of positive electricity on the ion is necessarily equal in amount, though opposite in sign, to that of the electron itself. A very

accurate measurement, carried out by Millikan, of the charge on an electron gives the value $4{\cdot}774 \times 10^{-10}$ electrostatic units. This value depends, as a matter of fact, on the determination of the Avogadro Constant N, to which we have already referred.

As already stated, the charge on the electron is the smallest quantity of electricity capable of independent existence. Similarly the electron itself is the smallest portion of matter capable of independent existence. The primary quality of matter is mass, and an electron is the smallest mass of material conceivable. No smaller mass has ever evidenced itself, though it has been carefully sought for. It is known that the mass of an electron is one eighteen-hundredth part of a single atom of hydrogen. That is, the mass of an electron is 5×10^{-28} gram.

This quantity is so very small that its removal from an atom (of hydrogen, say) leaves us with a positively charged residue, the hydrogen ion, which possesses a mass practically identical with that of the neutral hydrogen atom itself.

The remarkable conclusion at which we have arrived is, then, this : the smallest particle of matter capable of existence is known to us only in the electrified state—an unelectrified electron is physically inconceivable—and the amount of electricity which it carries at the same time is the smallest quantity capable of existence. This "coincidence" of matter and of electricity is exceedingly significant. In fact, Sir J. J. Thomson has shown that the whole mass of the electron is electro-magnetic. That is, in the electron, matter and electricity have become one. It is impossible to define the one except in terms of the other.

With this brief sketch of what we mean by electrical charge and the concept of the electron, we may approach the problem which we had set out to consider, namely, the structure of the atom.

Atoms, as usually met with, are electrically neutral. They cannot consist, therefore, of electrons alone, for these would give rise to negatively charged systems, and further an assemblage of electrons, owing to their mutual repulsions, could not possibly produce a stable arrangement. An atom must contain at the same time some source of positive electricity. At present we know relatively little about the positive elec-

tricity in the atom. The first successful attempt to deal with the structure of the atom from the electronic standpoint is that of Sir J. J. Thomson (8), who postulated a sphere of positive electricity of uniform density throughout which the electrons were distributed. This was frankly an attempt to give stability to the atom rather than a final statement of the nature and location of the positive charge. Stress was laid on the problem of distribution of electrons in such an atom, especially with the view of showing how certain chemical properties possessed by actual atoms—notably the periodicity in properties as expressed in the Periodic Law—could be accounted for in terms of electrons. The problem is to find how the electrons will distribute themselves in a sphere of positive electricity, when successive additions of electrons are imagined to be made to the system. To simplify the problem, Sir J. J. Thomson considered the electron distribution in one plane, namely, the plane through the centre of the atom. The results were by no means complete, but they showed in a striking manner that the first step towards the elucidation of atomic structure had been taken. We shall follow out a few of the simpler steps in this imaginary process of building up an atom model.

If one electron be added to the positive sphere it will obviously go to the centre and remain. If a second electron be added, the two will take up positions equidistant from the centre, their distance apart being identical with the radius of the sphere. Three electrons will distribute themselves at the apices of an equilateral triangle. Four electrons were shown to be incapable of stable equilibrium in one plane. With five electrons the equilibrium state reached corresponds to a regular pentagon, one electron being at each corner. Six electrons, however, would not form a stable hexagonal system. Instead, one electron goes to the centre, the remaining five forming a regular pentagon. This is called a two-ring system. Similarly eleven electrons distribute themselves in such a way that two form the inner ring, and nine the outer. With successive additions one finds that the two-ring system becomes unstable, and a three-ring system makes its appearance. This occurs when seventeen electrons are present, and the three-ring system persists until we reach thirty-two electrons, when a four-ring system appears. In this way we can build up

more and more complex atomic models. It will be evident at once that, as this process goes on, we have periodic changes in the type of distribution involved. Thus, the atom containing one electron has this electron at the centre. The atom containing six electrons has again one at the centre, the remainder forming the outer ring of the two-ring system. The atom with seventeen electrons is three-ring, one electron being in the centre, then a ring of five, and lastly a ring of eleven. The atom of thirty-two electrons is four-ring, one electron in the centre, then a ring of five, then a ring of eleven, and lastly a ring of fifteen. This recurrence of the same type—one central electron—at various intervals as the atomic weight increases, leads us to expect a recurrence of similar properties. This is the analogue of the Periodic Law.

Although this demonstration of periodicity in properties was a very great step, the question of the distribution of the positive charge was still left quite unsettled. More recently, Rutherford (9) has developed an atomic model, in which the positive charge resides on a nucleus at the centre of the atom, this nucleus being itself small compared with the dimensions of the atom as a whole. The Rutherford atom somewhat resembles a planet surrounded by satellites. We can speak of the nucleus and the "atmosphere" of electrons rotating around it, as constituting the atom. In spite of its minute size, the nucleus is mainly responsible for the mass of the atom. An atom of hydrogen, for example, differs from an atom of lead, in that the nucleus of hydrogen is very much less massive than the nucleus of the lead atom, and at the same time the number of electrons in the atmosphere of the hydrogen atom are very few compared with the number in the atmosphere of the lead atom. We shall come to the question of the numbers of satellite electrons in a moment. It is not easy to enter into the reasoning which suggested this structure to Rutherford. Suffice it to say, that the concept is based essentially upon the behaviour of an atom when an alpha particle from some radio-active source collides with it. Rutherford has shown that an a particle—which is a positively charged particle of matter whose mass is the same as that of an atom of helium—passes with ease through the outer "atmosphere" of electrons, which surround the centre of any atom, but that the a particle is violently deflected from its

path when it approaches the centre of the atom itself. The angular deflection of the *a* particle has been determined, and from such measurements Rutherford has inferred the existence of a central nucleus in every atom, the nucleus having the dimensions and mass already mentioned. It must not be concluded, however, that the nucleus is all positive electricity, although its effective charge is positive. The nucleus contains a number of electrons which are held extremely tenaciously, much more firmly indeed than the outer " atmosphere " of electrons. It is these outer satellite electrons which give rise to the chemical reactivity of the atom, its valency, etc., and it is upon these outer electrons that ultraviolet light acts when it detaches an electron from the atom. The outer " atmosphere " consists of a number of concentric rings of electrons—at any rate, that is the simplest view of its structure—the numbers in each ring going through certain changes according to the nature of the chemical element. The outermost ring of all appears to contain those electrons which confer valency on the atom, enabling it to interfere with the corresponding outer ring of another atom, in other words to unite with another atom. The question of the structure of the nucleus is practically unsolved at the present time. It is unlikely that much information will be gained regarding it until we are much more familiar with the relatively simpler problem involved in the outer " atmosphere " of electrons.

As regards the actual number of these outer electrons, we are already in a position to say that the number is very nearly one half of the ordinary atomic weight of the atom concerned. In the case of nitrogen, for example, the number of electrons is seven, in oxygen eight, in sodium eleven, and so on. In the case of hydrogen it is now fairly certain that the outer atmosphere consists of a single electron. That is, the neutral hydrogen atom consists of a positive nucleus carrying unit positive charge, round which a single electron rotates. It follows, therefore, that the unhydrated hydrogen ion is itself the hydrogen nucleus.

The atomic model of Rutherford is the one attracting most attention at the present time, and mainly for this reason: the system is essentially an *un*stable one, that is, unstable from the standpoint of classical mechanics. Within recent years, however, Planck has put forward a new concept known as

the energy-quantum theory, which is really a new system of statistical mechanics. Planck has applied it in the first instance to radiation phenomena. According to the quantum idea, emission of radiant energy takes place discontinuously, not continuously as on the classical electro-magnetic theory. In other words, emission of radiation takes place in terms of units, called quanta, and a substance is only capable of emitting a whole number of such quanta, each quantum being itself indivisible. This view, although utterly heterodox, has proved itself of so much value not only as regards the quantitative treatment of radiation, but also in the hands of Einstein, Debye, and others in connection with molecular heats and other problems, that it must be taken into account in all processes which involve energy exchanges, and therefore in problems of atomic structure. What is particularly important for our present purpose is that Bohr (10) has recently succeeded in demonstrating that the Rutherford atom-model, though unstable on the basis of classical mechanics, is not necessarily so on the basis of the quantum theory; and, in fact, Bohr has shown, that by introducing the quantum idea into the Rutherford atom, a number of very important relations in spectroscopy, notably the Balmer Law of series, can be satisfactorily deduced. It would take us too far to discuss further the Rutherford-Bohr atom-model. There can be no question, however, but that it represents the most successful attempt hitherto made to solve, even partially, the question of the structure of the atom. The reader who is interested should consult the papers of Bohr and of Rutherford already referred to.

Chemical Reaction

As already mentioned, a chemical change in its simplest significance means a change in the nature of a molecule. The molecule may lose one or more of its atoms, and these may be replaced by others. Thus two substances A and B may react with one another to give quite new substances C and D, the differences consisting essentially in a different molecular structure. Let us take a simple case and try to visualise it in terms of the considerations dealt with in the preceding section.

A molecule of hydrogen contains two atoms of hydrogen. According to Bohr, the hydrogen molecule consists of two

hydrogen nuclei, each carrying unit positive charge, and between these nuclei the two electrons rotate in an orbit the plane of which is at right angles to the imaginary line joining the two nuclei. This system has been shown to be a stable one, provided the nuclei as they vibrate to and fro do not exceed certain limits of amplitude. There is a similar limitation to the diameter of the orbit of the electrons. A molecule of chlorine contains two atoms of chlorine, each of which consists of a nucleus and an atmosphere of 17 electrons. Since the chlorine atom is univalent, just as the hydrogen is, there must be one valency electron present in each atom of chlorine whose special function it is to keep the two atoms joined together in the chlorine molecule. That is, we think of a chlorine molecule as consisting of two nuclei, each nucleus surrounded by rings of electrons, 16 electrons in all (situated relatively close to the nucleus), and farther out a ring of two electrons rotating in an orbit just as in the case of the hydrogen molecule. If now a hydrogen molecule collides with a chlorine molecule, and if there be sufficient displacement of parts produced, it is conceivable that each molecule is broken in such a way that a chlorine nucleus with its attendant 16 electrons unites with a hydrogen nucleus by means of the two rotating valency electrons so as to give rise to an unsymmetrical molecule containing one chlorine nucleus with its sixteen electrons rotating close to it, and one hydrogen nucleus with a ring of two electrons rotating perpendicularly to the line joining the nuclei. This new kind of molecule indicates that a chemical reaction has taken place between the hydrogen gas and the chlorine gas with the production of a new substance, hydrochloric acid.

This single example may serve to show that chemical change involves a rearrangement of electrons and nuclei. At the present time, however, we are not in a position to follow this line of treatment of the mechanism of chemical change in general, for the simple reason that very considerable doubt exists as to the mode of distribution of the electrons in a single atom of fairly high atomic weight, a state of affairs which obviously prevents us attempting the more complex problem of electronic arrangements inside molecules containing several atoms.

Without attempting to account for the position and mode

of vibration of every electron in a compound molecule we are able nevertheless to make some progress in understanding the mechanism of chemical change by following a more generalised train of reasoning. We have seen from our previous considerations that a molecule may decompose or react with another provided the nuclei become sufficiently widely separated from one another. To bring about this incipient dissociation of the molecule we must add energy to it in the form of heat, for it is well known that many substances dissociate or decompose when their temperature is raised. In other words, we can think of a molecule in the non-reactive state as containing a certain amount of energy which we shall call the mean or average internal energy. We may likewise conceive of this energy being increased by absorption of heat until it reaches a stage, which we may call the critical stage, at which the molecule becomes chemically reactive. By carrying out certain measurements on the speed at which a reaction proceeds at different temperatures it is possible to calculate what the critical energy is for a given kind of molecule, and in this way compare the relative reactivities of different molecular species in numerical terms. This kind of investigation represents a new branch of the subject which has been followed only within the last few years. It is too early yet to properly assess its value A very wide field of research opens in the direction of correlating the structure of molecules with the critical energy which has to be added to them to make the molecule reactive.

The Solid State of Matter

The characteristic feature which distinguishes the solid state from the other two is the existence of crystalline form. (Amorphous solids, such as glass, are grouped as liquids of very high viscosity.) A true solid separates from solution or from vapour in the form of crystals, having definite and characteristic properties, such as the melting point for example. (Amorphous solids or supercooled liquids possess no definite melting point.) Crystalline form exhibits many varieties, into the details of which it is unnecessary to enter, as our present considerations deal only with one or two types belonging to the regular or cubic system.

For many years the solid state presented greater theoretical

difficulties to the investigator, who aimed at elucidating its molecular mechanism, than was presented by the gaseous or even by the liquid state. The gaseous state was indeed relatively well understood at a time when practically nothing was known about the solid except the external crystalline form. Within the last decade, however, very great progress has been made in visualising the processes operative in solids, so that at the present time it is safe to say that we know more about solids than we do about liquids. This progress has been made along two distinct lines: the first in the domain of atomic heat and energy content, as a result of Einstein's (11) application of the quantum theory to the energy content of physical resonators or vibrating atoms, the second in the domain of the inner structure of solids of which the crystalline form is the outward and visible sign, chiefly as a result of the investigations of W. H. and W. L. Bragg, with the aid of the X-ray spectrometer (12). The first line of attack has given us considerable information regarding the mode of motion of atoms in simple solids; the second chiefly concerns itself with the problem of the mode of spatial distribution of atoms in a crystal.

The existence of crystalline form indicates that, whatever the mode of motion of the ultimate particles may be—and motion of some kind must be present in order to account for the temperature of the substance—the existence of crystalline form indicates, that the particles do not undergo translational movement from one part of the solid to another as is the case with gases and liquids. In the simplest case, namely, that of a solid consisting of atoms all of one kind, a piece of copper for example, the most likely mode of motion is that of vibration of the atoms in three dimensions in space over a fixed centre of gravity. At absolute zero the atoms are quiescent. As temperature rises, the recent work of Einstein, Nernst (13), and others has shown, that for a certain range, there is practically no motion. As the temperature is raised still further a certain number of the atoms are vibrating in the manner referred to, and the amplitude of such vibrations goes on increasing until, at a high temperature, which we identify as the melting point, the amplitude of vibration becomes equal to the mean distance of the atoms apart. As a result of this condition the crystalline form can no longer be maintained.

In other words, the solid melts, and the atoms are free to move from place to place.

It has long been known that the atomic heats of solid elements, especially metallic elements, at ordinary temperature are very nearly the same quantity, namely six calories—the well-known Law of Dulong and Petit. Boltzmann, many years ago, showed that this value was to be expected if the mechanical principle of equipartition of energy between degrees of freedom held good. Boltzmann based the numerical result upon atomic vibration, possessing three degrees of freedom, the view still held. It was known, however, that several of the elements, notably the element carbon, did not possess the value six for the atomic heat, but a smaller quantity. Further it was shown later that, on lowering the temperature, the atomic heats of all the elements investigated diminished considerably. This result could not be explained on the basis of the classical mechanics employed by Boltzmann, for the classical theory requires absolute constancy in the value of the atomic heat. It was only when Einstein introduced the quantum theory, as already mentioned, into the problem, that a way out of the difficulty presented itself. It would hardly be possible, however, to follow this line of argument further in the present instance. We shall turn, therefore, to the second line of investigation, namely, that of crystal structure by means of X-rays.

X-rays are now known to consist of vibrations of exactly the same nature as light, with this difference, however, that the wave length of X-rays is incomparably shorter than, say, visible light. Thus yellow light has a wave length of about $5{,}900 \times 10^{-8}$ cm., whilst that of X-rays is of the order 10^{-8} cm. itself. From what has been said in connection with molecular dimensions it will be realised, that the average wave length of the X-ray is just about the same order of magnitude as the diameter of a molecule, and therefore of an atom. This accidental comparability in size has made possible the investigation of crystal structure by means of the rays.

Since X-rays possess the properties of light it would be expected that a beam of the rays could be dispersed into a spectrum provided we had at our disposal a diffraction grating of suitable dimensions. For ordinary light it has been found possible to produce a spectrum by means of fine lines ruled

on a plate. This arrangement, however, would be far too coarse to produce dispersion of the X-ray beam. In fact, we require small obstructions of the dimensions of atoms or molecules themselves to function as a grating for X-rays. The idea that the ultimate particles composing crystals could function as the required grating for the rays appears first to have been suggested by Laue. The ingenious device of using a piece of crystal as a reflection grating is due to W. L. Bragg. With this modification very much simpler results were obtained than those of Laue himself. We have to think of a crystal as built up of layers of particles, and at each layer the beam of X-rays suffers a partial reflection. If the beam itself is homogeneous, we can observe its single line spectrum, repeated at intervals, that is, at certain angles with respect to the plane of the grating, *i.e.* the layers of the crystal. This reflected beam of X-rays cannot be detected by any optical method, but, as the Braggs have shown, we can utilise the property of ionising a gas, which is possessed by X-rays, to determine the position of the reflected spectral line. By a well-known expression in physical optics we can calculate the distance apart of two successive diffracting planes or layers of particles in the crystal, provided we know the wave length of the light employed, and the angle which the diffracted beam makes with the diffraction plane. These data were obtained by the Braggs for a number of crystals, and for various positions of one and the same crystal. As a result of these measurements we now know the values of the spacing of the particles in a number of crystals, that is the distance of the particles apart as we consider them from different directions in the crystal. To give a single illustration of the order of magnitude concerned it may be stated that in the case of rock-salt (sodium chloride) in one particular plane, the spacing is 2×10^{-8} cm. We now come to a question of the greatest importance. Are the particles, which have been referred to, the molecules, say NaCl, or the separate atoms? The Braggs have discovered that the atom and not the molecule is the fundamentally important individual in crystals. In other words, the concept of the molecule has disappeared for these solids. It is impossible to say whether a given sodium atom belongs to a given chlorine or not, since any single sodium is symmetrically placed with respect to six chlorine atoms. The whole crystal

may be regarded as one complete molecule, but it is evident that the ordinary concept of a molecule has vanished. We are forced to regard the solid state as quite distinct from that of the liquid or gaseous state even of the same substance. From this point of view it is scarcely to be expected that anything like continuity can exist between the solid and liquid states, such as Andrews first discovered for the liquid and gaseous states.

To appreciate the reasoning upon which the above conclusion is based it is necessary to assume some slight acquaintance with nomenclature of crystal structure. The Braggs found in the case of rock-salt, to take a concrete case, that the relative spacing of the spectra obtained depended upon the particular face chosen for examination. That is, the diffracting particles were spaced regularly, but the distance from particle to particle varied according to the direction considered. This is, of course, quite to be expected, and does not tell us anything with regard to the nature of the diffracting centres themselves. The Braggs, however, observed that whilst the spectra obtained from the (100) and (110) faces diminished in intensity quite regularly as the order of the spectrum increased (the normal behaviour), the spectra obtained from the (111) face showed alternate strong and weak lines. This alternation constitutes one of the most important of the results obtained in these investigations. To grasp its significance, it is necessary to make use of an experimental fact established independently, viz. that the efficiency of an *atom* of a substance to function as a scatterer of X-rays is proportional to the atomic weight, the heavier the atom the more intense the reflection. Applying this fact to the peculiar spectra obtained from a particular face of rock-salt, the Braggs concluded that the alternations in intensity indicate successive layers of chlorine and sodium atoms. That is, *the real physical units of which crystals are built up are atoms and not molecules.* Each atom has, as it were, a separate and individual existence. Since any single sodium atom is united to six chlorine atoms we see at once that our ordinary idea of valency is breaking down badly in the solid state.

One of the most interesting cases investigated by the X-ray method is the structure of the diamond. It has been found that the carbon atoms are arranged at the centres of tetrahedra,

an interesting fact in view of the stereochemical principle first enunciated by Le Bel and by van t' Hoff regarding the disposition of the four valencies of the carbon atom in the formation of carbon compounds. Further, on viewing the structure of the diamond from a certain angle, it is found that an arrangement quite analogous to the six-membered ring of the benzene molecule manifests itself, the carbon atoms forming a hexagonal figure.

Before leaving this subject, it may be mentioned that a number of very ingenious applications of Braggs' work have been made by Langmuir (14), in connection with heterogeneous catalysis and absorption of gases upon solid surfaces.

From the very cursory review of the subject which has been given above, it will be clear that the last decade has advanced our knowledge of the structure of matter in a very decided manner. Many problems, of course, still call for elucidation. We have not yet arrived, for example, at a really satisfactory expression for the temperature—pressure—volume relations of gases and liquids. The same may be said of the problem of the structure of the molecular layer which separates two phases in contact. We are still largely in the dark as regards the connection between capillary effects and the influence of the electric charge upon surfaces, a problem which has a very direct bearing upon the stability of colloids, and through this subject upon many problems in physiology and bio-chemistry. In atomistics itself there is room for almost unlimited research into the problem of what constitutes the chemical reactivity of different atomic structures, the influence of temperature and external conditions upon electronic movement and equilibrium states, and finally the still unsolved problem of the constitution of the atomic nucleus.

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CORRESPONDENCE

To the Editor of "Science Progress"

THE FLUVIATILE THEORY OF THE ORIGIN OF THE OLD RED SANDSTONE

From J. W. Evans, D.Sc., LL.B.

Dear Sir,—In Mr. G. W. Tyrrell's interesting article on "The Origin of the Old Red Sandstone" (Science Progress, October 1917, pp. 333–8) he characterises Prof. Barrell's paper on the dominantly fluviatile origin of that formation as the first detailed study from that point of view, but mentions that Mr. C. B. Crampton and Mr. R. G. Carruthers had taken up a similar position in 1914 in the Caithness Memoir of the Geological Survey. He adds that Mr. E. B. Bailey and H. B. Maufe had approached the theory of fluviatile origin in other recent memoirs, and so had Prof. J. W. Gregory in his *Geology of To-day* (1915), and furthermore the late J. G. Goodchild in 1904 came very close to the fluviatile view. This solution, however, of the problem goes back much farther. Prof. T. G. Bonney in his Presidential Address to the Geological Section of the British Association in 1886 (Proceedings, 1886, published in the succeeding year), pp. 616–17, remarks, "The Old Red Sandstone of Scotland and of Wales indicates a yet further continental extension towards the south-east. . . . The Devonian period introduces us in the greater part of Great Britain to an epoch of limited and exceptional deposits, and of widely prevalent terrestrial conditions. It seems almost certain that the Old Red Sandstones of Scotland and Wales are of freshwater origin—the débris of rivers, formed either in lakes or possibly in part as sub-aerial deposits. Streams of considerable volume and of some strength are indicated by the materials. . . . It is easy to ascertain the source of the materials of the Scottish Old Red Sandstone. They are as obviously the detritus of the Highland mountains—then

probably a far grander and loftier chain—as the Nagelfluh and the Molasse of Switzerland are of the Alps."

Five years later in a thesis on *The Geology of the North-east of Caithness*, after describing the Old and New Red Sandstones as "the monuments of two great continental periods which prevailed over wide areas in the northern hemisphere," I observed (note, p. 46): "It seems probable that some of the widespread conglomerates of Caithness and the Moray Firth may have been deposited sub-aerially by mountain torrents (intermittently swollen by heavy rains) as they reached lower and less hilly regions." . . . After referring to Prof. Bonney's address, I continued: "The intercalated fish-bearing beds in the Moray Firth would represent a short period during which lacustrine conditions occurred. Of course the sub-aerial deposits need not have been contemporaneous in different places; in Caithness they probably represent a period when more continental conditions prevailed than during the deposition of the Caithness flags"; and the suggestion was made that "lakes and streams afford a greater variety of environment than the sea—a variety that is probably an important element among the conditions favourable for rapid evolutionary change." I subsequently emphasised the torrential character of many of the Old Red Sandstone deposits in a contribution to the *Handbuch der regionalen Geologie* which was in the press at the time of the outbreak of the war. It may be added that Walther early took a similar view of the origin of the Old Red Sandstone.

J. W. EVANS.

[I am much obliged for Dr. Evans's gentle reminder that the theory of the fluviatile origin of the Old Red Sandstone dates back at least as far as Prof. Bonney's Presidential Address of 1886 and Dr. Evans's own work of 1891, and I much regret having omitted their names from my short synopsis of the history of views regarding the Old Red Sandstone. Of course the essay-review was only intended to summarise for geologists an interesting theory of the origin of the Old Red Sandstone, developed in detail for the first time in the publications under review. It was not intended to be an exhaustive account of the formation, or I should have dealt with the literature in much greater detail.—G. W. T.]

A NATIONAL UNION OF SCIENTIFIC WORKERS

From Norman Campbell, *General Secretary, N.U.S.W.*

Dear Sir,—May I venture to draw the attention of your readers to the advertisement, which appears in this number of Science Progress, of the formation of a National Union of Scientific Workers.

The general object of the Union is sufficiently suggested by its name. It exists to improve the status and economic position of scientific workers, and it will hardly be necessary to argue to the readers of Science Progress that the highest interests of science in its relation to the national life are intimately bound to such an improvement. Any doubt which is felt concerning the Union will probably be based on a belief that its objects will be attained, better or as well, by associations of a type already existing.

It should be explained then that the originators of the Union have not the slightest wish or intention to trespass on the ground already occupied by existing societies; but they think there is a work to be done which none of them have attempted or could attempt by reason of their origin and constitution. It is certainly a necessary precedent to any improvement in our status that professional qualifications should be maintained and raised, and that unprofessional conduct should be suppressed by the methods of such societies as the Institute of Chemistry or of Electrical Engineers. It is also important that the methods of public meetings, speeches, and deputations should be worked to their fullest capacity, as they are by the British Science Guild. But however active and efficient are the bodies which undertake such work, there it yet another method of attaining our ends which remains unexploited and yet is, in the opinion of those who have founded the Union, the most powerful of them all. It is the method of the Trade Union.

I know I am daring greatly in mentioning those words. Experience has taught me that there are people who, as soon as the words Trade Union are mentioned, rush to the conclusion that the first act of our association will be to call all chemists out on strike, picket the doors of universities against

blackleg professors, set up elaborate restriction on output, and rule that no research worker must publish more than one paper a year. And the strange thing is that the people who conclude thus rashly are the last to admit that the power and influence which organised labour has gained for itself in the State are really due to the adoption of such practices. We believe that this power and influence is simply the natural consequence in a democratic community of the form of organisation which the manual operatives have adopted, and that it will never be obtained by other groups in the community unless they follow the example and unite themselves in truly representative and all-inclusive associations. It is from the number of their members and the width of their membership that they derive their strength; it is in this, and not in the details of their policy, that we wish to imitate them.

And perhaps a second purpose which may be usefully served by the Union should be indicated. There are many sectional scientific associations in existence and there are more forming; all are striving to promote the interests of their branch of the profession. It is by no means impossible that, in the near future, these interests may clash; we would suggest that the best means of avoiding the disastrous disputes which would ensue is the formulation of a common policy in a common society.

Other questions concerning the Union are, I hope, sufficiently answered in the pamphlet for which every one interested is asked to write; but one important question may be answered here. The originators of the Union feel it would be ridiculous for them or for any other self-appointed body to attempt to lay down permanently even general conditions for a Union intended to embrace all scientific workers. While, therefore, they express in the pamphlet some suggestions of what the Union may be or may do, they are devoting their energies entirely to the formation of a preliminary organisation which shall enable a meeting to be summoned in which every one will be represented who cares to be. The meeting thus summoned will determine the future of the Union, and may possibly determine it in a manner unexpected by its conveners. The explanation of this preliminary organisation is the main object of the pamphlet; but we would urge that dissent from any of the views which the present committee may indicate is not a

sufficient reason for standing aloof from the preliminary organisation. The general meeting which we are summoning will not have the authority we desire for it unless in it are represented every kind of scientific worker and every shade of scientific opinion.

I am, Sir,

Yours faithfully,

NORMAN CAMPBELL.

NOTES

God-Man or Ape-Man

Some months ago I was reading during an air-raid Sir James Frazer's admirable *Studies in Greek Scenery, Legend, and History*, in which he describes how, even in the day of Marcus Aurelius and Pausanias, the heroic age of Greece was already a matter of antiquity. This was so early as A.D. 174; and even then Pausanias described "shrunken or ruined cities, deserted villages, roofless temples, shrines without images and pedestals without statues." In one neglected fane he saw a great ivy-tree rending the stones asunder, and in others nothing left but the tall columns standing up against the sky—as they are seen to-day. More than half a century earlier Plutarch had declared that the world in general and Greece in particular were depopulated by wars and civil brawls—so much so that Greece could now scarcely put 3,000 infantry in the field, the number that Megara alone had formerly levied for the Persian wars. But the decadence extended even to the minds of the people; for the great race which had worshipped the godhead of man in the superb figures of Apollo and Artemis, and which had built the Parthenon for the glory of Science, was now succeeded by persons who offered flesh and cakes to the alleged sceptre of Agamemnon or who sought to preserve vineyards from stormy weather by running round them carrying bleeding pieces of a white fowl! And while I read those pages the half-human fools who made the present war were engaged in dropping bombs on the women and children of London in the hope that they were winning some kind of victory by doing so!

Some weeks later I was at Delphi itself—where one can still imagine Apollo descended from Parnassus standing at the mouth of the Castalian Gorge,

> In perfect wisdom, perfect beauty dight.

In this figure I see the sum of the ancient civilisation. In it I see also the truest religion of man; of which the divinity

is—man himself.[1] To me, Apollo was the personification of humanity divine.

There are two ways of thinking, the way of the god-man and the way of the ape-man. As persons by their friends, so shall we judge races by their gods. We know the fighting glutton-gods of the Teutons, the monstrosities of India, the hybrids of Egypt, the fetishes of the African, and the silly saints of the Byzantines—idols of nature, war, lust, fear, or decadent melancholy. But what other people than the ancient Greeks have ever symbolised their own divinity in such forms as those of Pallas, Apollo, the Muses, Prometheus? Observe that these are mind-gods—not brute-gods or nature-gods like those of lower races. Moreover, they are divine, not only through their attributes, but by their very health, vigour, triumph, and beauty. Not mere symbols of the Thing-that-Is, like Jehovah and Allah; not figures of an unproductive meditative fakirism, like Buddha; not even like that higher vision of Pity—higher yes, but one who still gazes ineffectually upon a world of sorrow which he cannot cure and which ends by crucifying him again, as in the present war: unlike these, the great intellectual gods of ancient Greece taught us not only to Be but to Do—to attain by our own efforts, wisdoms, and virtues. The men who worshipped them were themselves gods—who gave us the beginnings of our civilisation of to-day. Theirs was, I say, the only true religion—that in which man is his own deity. It is the religion of every poet, every man of science, every artist, every discoverer, every inventor, every good and wise person; for the wreaths and the offerings which these men place before their god are the flowers and the fruits of their own lives—dedicated to Man. With the addition of the Sermon on the Mount, this should be the creed of all of us to-day.

Suddenly that age ceased. But why? The climate, the mountains, the valleys, the rocks, the springs of Greece remained; the same fields and the same trade-routes could have fed the same populations; the same ideals could have kept them great. Was it the irruption of barbarians, was it the change of economical conditions, was it the entry of some new endemic pestilence, was it political servitude that debased them? The two first could scarcely have destroyed them had they not

[1] See the *Poetry Review*, March–April 1918.

been already debased. From political servitude they and other races had often suffered before and since and had yet thriven well. The malaria must have been more serious for certain tracts in Greece, but not for all—such as the Attic Plain and the Theban Rock; and even the Greeks partly knew how to deal with it, while other civilisations have fallen without it or similar causes.[1] No—I think that the decline of Greece, as of most other countries, was chiefly due to the fact that long-continued prosperity, wealth, and overcrowding teach men that more is to be won for themselves by cunning, dishonesty, and even crime than by conscientious work. The lying politician, the servile general, the cheating trader, the perfunctory workman then become predominant; the councils of the nation lose both honesty and wisdom; art becomes facile and impressionist; and science and philosophy cease because the study of them rewards no one. In short the religion of the god-man, which consists in the worship of man, is replaced by the religion of the ape-man, which consists in the worship of self. The people clamour only for their rights, and their duties are forgotten.

Si experimentum requiris, circumspice. If we do this we shall find ourselves to-day in a great and dismal jungle with innumerable creatures peering down at us, and all repeating at once the single word Rights, Rights, Rights. There are the legions of large pot-bellied gorilla-men demanding their right of having a place in the sun, and killing everything that comes their way in the hope, apparently, of getting more sunlight by doing so. Then there are vast herds of chacma-men, walking about slowly with bags of tools in their hands, but refusing to use them, even to save the lives of their comrades, unless they are first propitiated with an abundance of roots. Further to the east, there are hordes of mandrill-men, hideously and obscenely coloured, flying from the gorilla-men but slaying every one else they meet in order to obtain they know not what. Here and there, up in the boughs, there are long-bearded langur-men, calmly teaching philosophies which they know to be lies to such few anthropoids as wait to listen to them; and

[1] My theory of the probable entry of malaria into Greece about the time of the Persian wars and its effect on the people has received much confirmation from the historical labours of W. H. S. Jones (Macmillan & Bowes, 1907), but I think that it was only one of the causes of the decadence.

right away out of danger in the tops of the trees the agile gibbon-men leap from branch to branch throwing down sticks (called policies) for the combatants to fight with and excitedly crying " Fight, fight : get your rights, get your rights." Meanwhile there is great darkness throughout the whole forest; for the clouds and the thunders of God roll over it, and we hear His voice saying, " I gave you no rights at all ; I gave you only duties." R. R.

The Mittag-Leffler Institute (P. E. B. Jourdain, M.A.)

The Pasteur Institute at Paris always seemed to that eminent mathematician, Prof. G. Mittag-Leffler of Stockholm, the editor of perhaps the most important mathematical journal in the world—*Acta Mathematica*—a model of what such an Institute should be. It has fulfilled the mission of an establishment intended to be a focus of scientific research far better than any present Academy or university. Indeed, universities are often too much concerned with the work of teaching to help greatly the progress of science, and Academies suffer under certain other inconveniences Hence arose the magnificent project of Prof. Mittag-Leffler and his wife, which is described in the short pamphlet published at Uppsala in 1916, under the title, *Institut Mathématique des époux Mittag-Leffler.* Prof. Mittag-Leffler and his wife have bequeathed all their property, including their house at Djursholm, near Stockholm, to help, after the death of both of them, in the task of founding an Institute, two of whose objects are to be the development of pure mathematics in the four countries of Sweden, Denmark, Finland, and Norway, and to make known and appreciated beyond the frontiers of these countries their contributions in "the most exalted sphere of the life of the mind." Besides the preservation and increase of the library, for which the above-mentioned house was specially built, and which house, to judge from the frontispiece to the pamphlet, is an exceedingly fine structure, the Institute will have the power to grant travelling and other scholarships for work in pure mathematics to young people of either sex belonging to the above four Scandinavian countries. But it would seem that the most important part of the bequest is the provision for a gold medal to be awarded at least once every six years for thoroughly important discoveries in pure mathematics. The recipient of a medal will also be presented with a diploma and a finely bound set of *Acta Mathematica*, and will be invited to come personally to Djursholm for the presentation. For this last purpose he will be granted a travelling indemnity. The other point of international importance seems to be the appointment of a scientific Director and possibly certain other officers : in particular the Director is to be an eminent mathematician who is to devote himself entirely to his own scientific researches, to be, of course, qualified for the post of Director of the Institute, and, from what appears, there is to be no restriction as to his nationality. His position is to be, from a material point of view, "more advantageous than that of any professor of mathematics in one of the universities of the four Scandinavian countries." It is particularly to be noticed that the future Institute will be devoted to the progress of *pure* mathematics, and that, though it is evident to any thinker that a very large part of the progress of science as a whole depends on the development of pure mathematics, this development has been hitherto almost unnoticed by those benevolent persons who have wished to help the progress of science by gifts of money and other property.

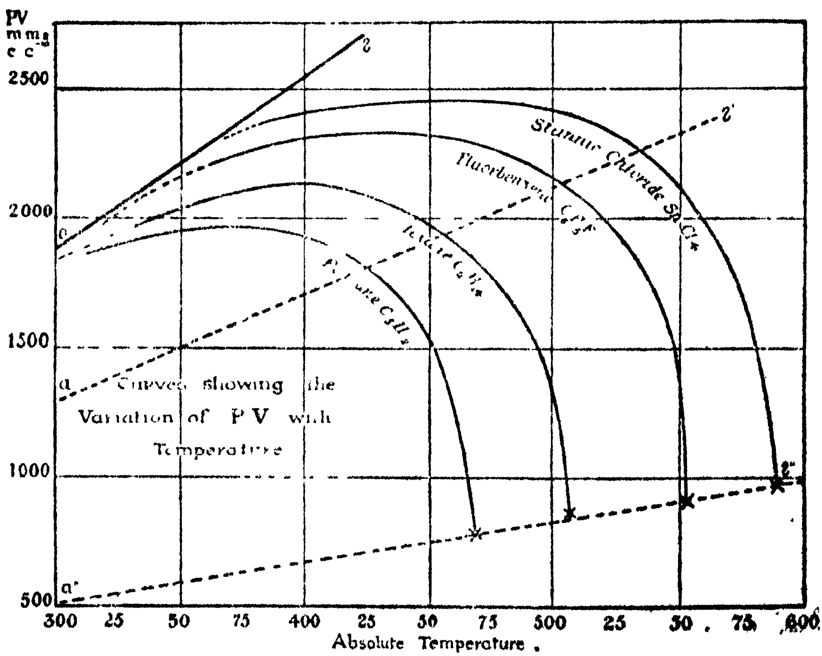
Some Points connected with the Liquid State (Gervaise Le Bas, B.Sc.)

Some have supposed that there is a sort of compactness in the arrangements of the molecules in the liquid state. Whether this be so or not, there is a change from the conditions obtaining in the vapour state. It is well known that pv, where p is the vapour pressure and v the molecular volume, is, according to the kinetic theory, a measure of the free space between the molecules. Two conditions alter the perfect gas relationships:

(*a*) The molecules have a definite size.

(*b*) There are mutual attractions.

This causes the values of pv for each substance, as they are traced to absolute zero, to be along curves similar to those found in the diagrams. The second



condition exists up to the critical point. The result is that the values of pv, and thus the free space, diminishes to absolute zero, when the points corresponding to each substance lie along $a'' b''$.

It is found that at the points similar to b'', the curves cut $a'' b''$ almost, if not quite, at right angles. It follows that, owing to the steepness of the curves at b'', the rate of diminution of the free space with temperature is enormous, if not infinite. The free space is then not zero, but a definite fraction of the space. Some circumstance thus prevents the molecules from collapsing to their volumes at absolute zero. It is not the vibration of the molecules as such, for, as already stated, the rate of variation of this with temperature is very great. The reason is probablyto be sought in the atomic vibrations, which cause the molecules to fill up the space available. The volume in the liquid gradually diminishes to that at absolute zero owing to the gradual diminution of the atomic spheres of activity

or vibration. This is in harmony with many physical properties of liquids—their compressibilities, etc.

It may be remarked that if at least two points of the curve are known, (*a*) the position of maximum pv and (*b*) the critical point, the whole of the curve can be traced (at least approximately). Maximum is at about 74 crit. temp. Curves for helium, hydrogen, oxygen, nitrogen, etc., might be found.

The curves themselves are constructed from data furnished by S. Young (*Journ. Chem. Soc.* 1880).

[NOTE.—There seem to be slight irregularities in the curves near the critical points. The curve for pentane seems to be the most regular, and cuts the line $a'' b''$ at right angles. A comparison of the other curves with this will show that they depart from this rule. On the other hand, the curve for fluorbenzene cuts the line vertically or nearly so. The curve for stannic chloride follows that for pentane.]

Recent Criticism of the Carnegie Trust

We have been asked to republish the following Report of a Committee of the British Science Guild upon the Carnegie Trust in connection with scientific research. The Report is taken from the *Journal of the British Science Guild*, and is based upon an article contributed by Prof. F. Soddy to SCIENCE PROGRESS for January 1917.

"The Carnegie Trust for the Universities of Scotland was founded by Mr. Andrew Carnegie in 1901, with a gift of £2,000,000. One-half of the annual income from this fund has to be devoted to the payment of students' fees in Scottish universities, and the other half is to be applied 'towards the improvement and expansion of the Universities of Scotland, in the Faculties of Science and Medicine; also for improving and extending the opportunities for scientific study and research, and for increasing the facilities for acquiring a knowledge of History, Economics, English Literature and Modern Languages, and such other subjects cognate to a technical and commercial education as can be brought within the scope of the university curriculum.'

"The annual income of the Trust has amounted in the past to rather more than £100,000, and, after defraying the expenses of administration, there has been left about £99,000 as the net revenue available for distribution under the two main heads of the scheme, or £49,500 for the part of it referred to above. In the future a very appreciable increase of revenue is to be anticipated.

"In the article contributed to SCIENCE PROGRESS for January 1917, Prof. F. Soddy, Professor of Chemistry in the University of Aberdeen, analysed the operations of the Trust, particularly as regards the promotion of scientific study and research. Prof. Soddy pointed out that, by a reasonable interpretation of the Trust Deed, the primary purpose of the income from one-half of Mr. Carnegie's gift was the encouragement of scientific study and research, including medicine, and that history and other subjects cognate to a commercial and technical education were to be regarded as ancillary beneficiaries; while the other subjects of a classical education were entirely excluded from participating in the fund. He showed, however, that in the case of the University of Aberdeen only 23 per cent. of the grants made had been allocated to the primary object, while 46 per cent. had been devoted to the ancillary object, and 19 per cent. to the objects which, in so far as they are not illegitimate, are ancillary. Up to September 1913, the Universities of Edinburgh and Glasgow had each received more than 60 per cent. of the total sums for the primary purpose of the fund, but the quinquennial distribution since then had been allocated to buildings chiefly for Arts accommoda-

tion, as well as for departments of science. As regards St. Andrews and Dundee, the position of the allocation of funds in the main has been between that of Aberdeen on the one hand and Edinburgh on the other. Of the total amount spent by the Trustees up to the end of September 1915, about 14 per cent. has been expended on a research scheme independently of the grants made to the universities. This has been spent mainly in providing Research Scholarships and Fellowships, and grants for research instruments—objects excellent in themselves, but more or less preliminary to the fostering of research.

"The main point put forward by Prof. Soddy is that the funds of the Trust are not in general being applied to the specific purposes for which they were intended, and are used for general university needs and to provide buildings and endowments for Art subjects, instead of the promotion of scientific study and research. In support of this contention, definite facts were stated which seemed to demand an equally definite answer if they are contested. The Guild therefore sent Prof. Soddy's article to the principals and representative professors of scientific subjects in the Scottish universities, and asked for an expression of opinion on the matter. Nine replies were received, but no attempt was made by any of the correspondents to refute the particulars given by Prof. Soddy as regards the allocation of the amounts received from the fund. The general opinion expressed was of a *laissez-faire* kind, with the addition of the following individual views: (1) That the Board of Trustees should consist much more largely of men who are professionally and actively engaged in scientific work, and have had experience of research. (2) That commercial education on a large scale should be taken in hand by the Trustees. (3) That a case had been made out for careful investigation, and that the matter should be considered by the British Science Guild to see what action, if any, is justifiable and practicable.

"As the chief object of the British Science Guild is to safeguard the interests of science and promote the application of scientific knowledge to national welfare generally, the matter is one to which the Guild is bound to give attention. After careful consideration of the material placed before it, the Guild has come to the conclusion that Prof. Soddy's serious charges should not be left unanswered, and that the diversion of the funds from their main purposes, as defined by Clause A of the Trust Constitution, and their use to strengthen the general finances of the Scottish universities, deserve the attention of those to whom has been entrusted the future of science in national reconstruction.

"The Guild is glad to note that three well-known men of science—Sir J. J. Thomson, O.M., President of the Royal Society; Sir David Prain, F.R.S., Director of the Royal Botanic Gardens, Kew; and Sir George Beilby, F.R.S.—were appointed at the last Annual Meeting of the Trust to fill the vacancies on the Board of Trustees, thus increasing the number of scientific representatives from none to three. Some adjustments in favour of scientific study and research may therefore be expected; but the Guild is of the opinion that the Trustees should consist chiefly of representatives of the scientific and other interests involved in proper proportions to ensure that the original intentions of the founder of the Trust are carried out justly.

"With regard to the commercial interests, it seems probable that their representatives have been thoroughly awakened by the war to the necessity for better education. It is desirable, however, that these interests should not be satisfied at the expense of, but in addition to, those of science; and for this reason the British Science Guild, believing that Mr. Carnegie's intentions admit of no dispute, desires to support Prof. Soddy's claims that future allocation of the Trust Funds should be made more liberally, specifically, and inalienably for purposes of scientific study and research than has been the practice hitherto."

* * *

We have just received, on going to press, some interesting remarks by Prof. Soddy on the Minute of the Executive Committee of the Carnegie Trust, sent to the British Science Guild and referred to above. It will be more proper to hold over the publication of these remarks until our next number.

A Proposal for the Formation of a British Universities Guild (By a Biologist)

This proposal is not an entirely personal and spontaneous one, but has been suggested by a recent, very efficiently managed agitation among the junior members of university staffs. This latter agitation is, of course, an entirely independent one, but it cannot long be continued without developing into some such propaganda as are here outlined. Frankly, the suggestion to form a British Universities Guild is to be linked up with the similar proposals that are now being thought about in relation to organised and unorganised labour, and is to be thought about primarily as an integral part of the general organisation of the productive workers of the country.

One of the better results of three years of war conditions is that some degree of discredit must now cling to those who are not engaged in *Service* of some kind. The stress of war needs has sent thousands of "gently brought-up" men and women to engage in monotonous and often disagreeable work—with results that have generally (but not always) been satisfactory. Possibly this experience may, in time, lead to the conviction that it is disgraceful for any normally minded and able-bodied man or woman to have been educated without attaining some *craft* or technique, some skill of mind and limbs that may be of service to the State. Reflection upon the conditions of the present time will convince any candid man or woman that immeasurable good would be attained if every one were taught (for, say, two years of early life) some useful handicraft. Now one might expand these suggestions, but in the meantime it will be sufficient to state what is the nature of the proposal that we have in view with regard to the organisation of university-trained men and women.

THE BRITISH UNIVERSITIES GUILD. OBJECTS

(1) The attainment of a higher degree of efficiency in all branches of Imperial and local, public, civil administration.

(2) The organisation of all men and women trained so as to apply original thought and an acquired craft or technique to the making of knowledge in the widest sense; or otherwise the organisation of *all* investigators, literary and historical or scientific.

(3) The application of the influence, which it is expected will be attained by the Guild, to securing a rational system of appointment of public officers and administrators.

(4) The maintenance of a supply of skilled investigators concerned with the development of industrial processes.

(5) The more complete organisation and study of the methods of education.

(6) The further improvement of the status and remuneration of all men and women employed in university, technical, secondary, and elementary education, and in original research of all kinds.

The direct objects for which it is suggested that the Guild should be established are those stated in paragraphs (1) to (5). The further improvement in the status and remuneration of those who will compose it is, it is true, an object in itself—one of some importance, it will surely be conceded. It is being recognised that the objects (1) to (5) cannot easily be promoted so long as the conditions under which university-trained men and women remain as they are at present. We can discern several ill-defined categories of employment of graduates: (*a*) non-professorial university, technical and secondary school teachers, (*b*) elementary

school teachers, and (*c*) investigators engaged in literary, historical, pure scientific, and technical research. The latter have not, yet, even the pretence of organisation. With very few exceptions all these categories of skilled workers are poorly paid, and the prospects of betterment are, as a rule, vague and highly unsatisfactory. In the past there were far more frequent opportunities of advancement in the shape of professorships, headmasterships, and heads and directors of public laboratories and other organisations for research. Now for some considerable time past, the increasing specialisation of higher teaching and research has tended to the increase of assistant posts rather than of the directorships, professorships, and other better-paid appointments. Certain other regrettable tendencies are apparent also: the university department in charge of a professor has come to be regarded as a *cadre* to be filled with assistants. The professor has come to exercise multifarious functions in the direction of one form of economic research or another, and it cannot be contended that the arrangement is satisfactory. Undoubtedly the organisation of the departments, as teaching institutions, tends to suffer by the tendency for the application of the activities of the professor towards the work of "administration"; and probably the personal research work of the professor also deteriorates under the pressure of the little trivialities of committee work.

At present these research assistantships are most poorly paid: salaries of £120 to £150 per annum are those of most of such appointments. Status is low, and there is little opportunity on the part of the assistant for the exercise of direction of research. Now it is clear that poor pay, lack of status, and insecurity of tenure most tend to deplete the vaguely defined profession of research of the best men and women. In the medical profession men and women usually undertake research only for a short time immediately after qualification. The work is badly paid, and it is generally understood that most medical research work is only preliminary to the more lucrative work of specialist, general practitioner, or public health officer. Investigation *must* suffer, for it is undertaken by a great number of young men and women who usually abandon it just about the time when they are becoming trained in special methods of research.

The objects stated in paragraph (6) must therefore be the main ones promoted by the Guild for an initiatory period. Nevertheless, the ulterior aims are the improvement of higher teaching and training; the creation of an organisation of workers skilled in those methods involving original thought, or able to employ those techniques or crafts (whether literary or scientific) which are the objects of a university training; and the assiduous endeavour to find for such workers proper places in a better State organisation. National efficiency in all departments of administrative and industrial productive work; the training of men and women to undertake specific duties; and the finding of just the right person for any post in the local or Imperial services—such must be regarded as the primary objects which the Guild ought to set out to promote.

Besides all this the Guild ought to endeavour to increase very greatly the output of independent, original research of every kind, both pure and applied. The present conditions with regard to research are now well known: they have been described in these pages many times during the last few years. Apart from a few meagre grants administered by the Royal Society, the British Association, and several Government Departments, there is no really adequate provision. Even those very limited sums available are usually placed at the disposal of university professors who already perform duties, or carry on personal research that ought to monopolise all their activities. The result is that the actual investigations are

made by assistants who have no control of the grants, little voice in the direction of the work, no representation in the Imperial or local authorities for whom the investigation is being made, poor status and pay, and scanty prospects of promotion. *Then* the result is obvious: men and women take up research work in the hope that it may, by-and-by, lead to more lucrative employment, and many find such and abandon the work of investigation at the time when they are best fitted to continue it with advantage to the State.

These remarks, it will be seen, apply more particularly to pure and applied scientific research, but it is not intended to make any distinction between this and literary, historical, and archæological research, or scholarship in any sense. Such work is even less organised, and those who engage in it labour under greater disadvantages than those who profess scientific work.

This statement of the objects of the Guild might be expanded with advantage, but some attention must be directed to the means by which they are to be promoted.

Constitution of the Guild

The conditions of membership must be such as to make the Guild inclusive in the greatest possible degree. Membership, it is suggested, should be open to all men and women possessing a university degree, and even this single qualification may, it is recognised, exclude some university-trained men and women who, for some reason or other, have failed or neglected to graduate. It should not be difficult, however, to devise some means of including the relatively few desirable persons belonging to this category.

Now, the greatest privileges conferred by a good university training are the habits of original thought, the knowledge of methods whereby a man or woman may be able to ascertain, at first hand, what is known about anything, and the possession of skill in some craft or technique. Such are the means whereby the Guild hopes to acquire power, but there are other opportunities. In a few months from now the great majority of university men and women will become electors in their own university constituencies, and there is really no reason why the Guild should not become possessed of considerable influence, in that it may become able to return quite a small party of Members of Parliament. This is the main reason why the possession of a degree is (with some rare exceptions possibly) the indispensable condition of membership.

Modes of Action

Parliamentary representation, therefore, must be the immediate object of the Guild, and in this respect it will be expected to act as a unit. The first work of the Council must be to organise a propaganda in each university constituency, to find and suggest candidates that are acceptable, and to take steps to obtain the money necessary to contest each seat.

It is hoped that these university contests will be something quite new in the history of British Parliamentary representation. It should be noted that graduates who are also university electors will (in general) be household electors in the constituencies in which they are resident. This household franchise might, therefore, be exercised in the usual way, the elector being free to support whatever political party he inclines toward at the time. On the other hand, the university suffrage should be used solely to return Members of Parliament pledged to support the declared policy of the Guild. Each university man or woman would thus act in a double capacity—in the capacity of an ordinary unprivileged citizen, and also in the capacity of a privileged person, trained in a certain way and pledged to

exercise that privilege for the promotion of some definite policy to which he is committed because of his status in the community. So, in a certain time, the Guild might be represented in Parliament by a small party sitting in permanent opposition, or rather neutrality, to the Government of the time and concerned only with the interests of its constituencies. Those interests, we have repeatedly pointed out, are the interests of the efficiency of administration and the proper place of independent, original thought in the State: they are not necessarily the mercenary interests of a section of the population.

Further Development of a Constitution

To attempt to make a constitution at this stage would be foolish. The first aim of the Guild ought to be to include as many as possible of university men and women (making the entrance and periodic subscriptions unusually low); then to take steps to influence the Parliamentary elections; and, lastly, to proceed to elaborate further modes of action. Perhaps the most obvious suggestion to make at the present time is that the Guild should proceed to segregate into Craft Sections, so that we might (for instance) have:

(1) *University Teachers*—that is, all professors, lecturers, demonstrators, etc.
(2) *Technical and Secondary Teachers.*
(3) *Elementary School Teachers.*
(4) *Literary Professional Workers:* thus, translators, indexers, librarians, abstractors, secretaries, literary, historical, and archæological investigators, and so on.
(5) *The Chemical Crafts:* pure researchers, analysts, industrial chemists, etc.
(6) *Physical Crafts:* public, civil, and consulting engineers, surveyors, architects, patent law agents, etc.
(7) *The Biological Crafts:* medical men who are solely engaged in investigation, public health medical officers and inspectors, bacteriologists, entomologists, fishery investigators, economic botanists.
(8) *Mathematical Crafts:* actuaries and statisticians.

This grouping is, of course, the merest suggestion.

Now, there are many other associations in existence: the Public School Headmasters, the National Union of Teachers, Associations of Analysts and so on—few of our Crafts have not already some degree of organisation. How to coalesce with, or co-operate with, or replace these existing organisations, or what are to be the relations of the Guild with them, must be the first consideration of the Craft Sections. But it may frankly be recognised that this union or co-operation may be so difficult as to be impossible; it may also be urged that there are numbers of professional men and women outside the existing associations; that there is no existing means of synthesising all these activities; and that the objects which the Guild desires to promote are not quite the same as the restricted objects of the existing bodies.

It is to be expected that many university men and women will either not join the Guild or will take little practical interest in its working if they do join. It is, perhaps, hardly to be expected that the older workers, and also those who belong to other craft organisations, will join in more than inconsiderable numbers, and so, for a time, the Guild can hardly hope to be fully representative of the trained higher craft workers of the country.

But the appeal made here is to the *young*. The first local Guild Councils must

see to it that all university men and women are persuaded to join immediately on graduation. All the professions to which we have alluded above will be recruited in ever-increasing degree from young university graduates, and so the Guild will come to obtain a greater and greater control of all those activities. The transition period may be left to the care of the existing organisations, and in a generation, or even less, the Guild will have come to full maturity.

It will then have resources of strength and influence not possessed as yet by any organisations except those of the medical men and lawyers, and those presently to be acquired by whatever shape the organisation of labour is about to take. Its great resource will be the youth, the full mental vigour, and the capacity for altruistic service exhibited by the young men and women at present leaving the universities. But it is not suggested that self-sacrifice should be the characteristic of the Guild any more than it is of the labour organisations or of those of law and medicine—or divinity.

It is foremost in the minds of those who are thinking about these matters that first of all the higher-educated workers must organise in order that they may secure decent conditions of life and a reasonable share of whatever public wealth is the result of their activities. Frankly, the Guild will tend to associate itself with the newer labour organisations about to emerge after the war, and this association may become a very close one. Such activities as those of the Workers' Educational Association would naturally appeal to it with much force. The existing organisations of university settlements and extension teaching, and the much greater development of technical and scientific studies among the craftsmen, would naturally become immediate activities with it. In very many ways it would fall into line with the Trade Guilds' movement—the organisation of production, not for private profit, but for the good of all in the State.

Some time ago Mr. Fisher said in a speech that the British universities had never co-operated or spoken with one voice. The means are suggested here whereby this desirable result may be obtained.

Science and the Cold-Storage Industry (From a Correspondent)

In a paper read before the Royal Society of Arts on December 19th, 1917, Prof. Wemyss Anderson made a strong appeal for scientific help to strengthen our knowledge of the preservation of perishable produce—particularly by low-temperature methods.

It appears that "the principal foodstuffs at present cold stored can be roughly divided into three classes:

"(1) Produce whose life history is finished, such as all classes of meats, poultry, rabbits and fish.

"(2) Produce whose life history is not finished, such as fruit and eggs.

"(3) Milk and produce from milk—cream, butter, and cheese.

"The mention of 'life history' is sufficient to show the peculiar blend of knowledge required to deal with proper understanding with the materials mentioned. It is astonishing to what heights the merchant and engineer have already soared; but, however keen the scientific penetration of those practically engaged in the cold-storage industry to-day, there can be no doubt that pure scientific, medical, and veterinary knowledge and research must be brought to bear on the subject."

The author is of the opinion that "every seat of scientific learning should have a refrigerating apparatus as part of its equipment—not a toy machine, but a self-contained cold storage with arrangements for a 'cold' reserve (large quantities

of calcium chloride brine) for periods of interruption. No research of any kind where temperature is a function can be considered complete that does not go down to the lowest limit reasonably obtainable, yet how many institutions are there where such investigations are possible? The lack of such facilities in the light of recent advances all over the world will constitute a serious disadvantage to our men of science, and the question must be taken up by every scientific body in the kingdom."

We are of the opinion that these suggestions should receive the most careful thought from our scientific institutions.

Uncut Books

Many people wonder why it is that books are still issued with uncut pages. Such people may be lazy or may be very busy; but in either case they might like to have the interesting explanation which Mr. Murray has been kind enough to send to us. He says:

"As regards uncut pages, there are very few books published nowadays with their pages uncut. For a fine book, I greatly prefer it in that form, as it leaves a margin of paper for trimming in the case of a book which one wishes to put into a good permanent binding later on. Moreover, for any one with leisure to read, I think it is a positive luxury to cut the pages with a good paper knife. I did this yesterday in the case of an old book which I had not taken from my shelves for many years, and enjoyed it thoroughly. Another objection to cut leaves in the old days was that certain people came into booksellers' shops and spent a long time there; in fact, they were able to get all they wanted out of a book without buying it! It was said that Southey could tear the heart out of a book by turning the leaves in a bookseller's shop. Many years ago my father subscribed one of Darwin's books in two forms, with cut edges and uncut edges, and in those days the uncut edges found such favour with the booksellers that a preponderant amount were sold in that form. But now everybody is in a hurry, and uncut edges are gradually disappearing except in the case of fine books. I am sorry for it, but the uncut form must now be regarded as a 'back number.'"

Notes and News (D. O. W.)

Workers in pure science were almost entirely ignored in the honours list published on New Year's Day, but the following awards may, perhaps, be of interest:—*Baronet:* Prof. J. Ritchie, Irvine Professor of Bacteriology at the University of Edinburgh. *Knighthoods:* Mr. W. N. Atkinson, who has contributed largely to our knowledge of the dangers of coal-dust in mines; Dr. J. T. Horder, Prof. J. Phillips, and Prof. H. J. Stiles, all three workers in medical science; Dr. A. Macphail, Professor of the History of Medicine, McGill University, and Mr. J. S. Keltie, who is retiring from the secretaryship of the Royal Geographical Society.

The appointments to the Order of the British Empire were largely made up of Government and railway officials; the scientific men whose names appear in the list are, for the most part, temporarily in Government employ. The vast amount of gratuitous work that is being done by science for the nation has been almost entirely overlooked.

Knights Commanders (K.B.E.): Mr. J. Cantlie (Red Cross Council); Col. C. F. Close (Director of Ordnance Survey); Dr. W. M. Fletcher (Secretary, Medical Research Committee); Dr. R. Robertson, F.R.S. (Superintending Chemist, Research Department, Woolwich Arsenal); Dr. W. H. Thompson (Scientific Adviser, Ministry of Food).

Commanders (*C.B.E.*): Mr. F. J. Cheshire (Optical Munitions Branch, Ministry of Munitions); Mr. F. W. Garnett (President, Royal College of Veterinary Surgeons); Dr. G. H. Fowler (Hydrographic Department, Admiralty); Prof. W. R. Hodgkinson, F.R.S. (Ordnance College, Woolwich); Mr. R. G. K. Lempfert (Superintendent, Forecasting Division, Meteorological Office); Prof. W. J. Pope, F.R.S. (Board of Invention and Research, Admiralty); Mr. E. Russell Clarke, M.B.E. (Expert Adviser to Naval Staff on Wireless Telegraphy); Prof. T. B. Wood (Chief Executive Officer, Army Cattle Purchase Scheme).

Officers (*O.B.E.*): Mr. G. S. Baker (National Physical Laboratory); Mr. G. T. Bennett, F.R.S. (Scientific Assistant, Compass Department, Admiralty); Dr. H. E. Cuff (Principal Medical Officer, Metropolitan Asylums Board); Mr. C. Cuthbertson, F.R.S. (Contraband Department, Foreign Office); Mr. M. J. R. Dunstan (Principal, South-Eastern Agricultural College and Food Production Commissioner for Kent and Surrey); Dr. R. C. Farmer (Research Department, Woolwich); Mr. C. L. Fortescue (Professor of Physics, R.N. College, Greenwich); Dr. J. A. Harker (National Physical Laboratory); Mr. A. Hutchinson (Demonstrator in Mineralogy, Cambridge University); Lieut.-Commander T. C. Irwin, R.N V.R.; Prof. T. M. Lowry, F R.S. (Technical Adviser on Amatol, Ministry of Munitions); Mr. A. F. Macallan (Director of Ophthalmic Hospitals, Egypt); Dr. O. E. Mott (Head Chemist, H.M. Factory, Oldbury); Mr. C. C. Paterson (National Physical Laboratory); Dr. E. J. Russell, F.R.S. (Director of Rothamsted Experimental Station and Technical Adviser to Food Production Department); Mr. F. E. Smith (National Physical Laboratory); Mr G. Stubbs (Superintending Analyst, Government Laboratory); Lieut.-Commander H E. Wimperis, R.N.V.R.; Mr. J. Young (Senior Instructor in Science, Royal Military Academy); Mr. G. Undy Yule (Director of Requirements, Ministry of Food).

Prof. H. R. Kenwood and Prof. E. H. Starling, F.R.S. (both holding the rank of temporary Lieut.-Colonel), have been awarded the C.M.G.

Sir J. J. Thomson has been appointed Master of Trinity College, Cambridge.

The Boyle Medal of the Royal Dublin Society has been awarded to Prof. J. A. McClelland, F.R.S., for his distinguished work in science, especially on ionisation, and on the radiation associated with radioactivity.

The gold medal of the Royal Astronomical Society has been awarded to Mr. John Evershed for his investigations of radial motion in sunspots and other contributions to astrophysics.

The Council of the Meteorological Society has awarded Dr. H. R. Mill the Symons Gold Medal for 1918 for distinguished work in connection with meteorological science.

Sir Archibald Geikie, O.M., has been elected Associate Member of the Paris Academy of Sciences.

Dr. Arthur Keith has been appointed Fullerian Professor of Physiology at the Royal Institution.

Among those whose decease has been announced during the last quarter we note with great regret the following: Dr. Garrett Anderson; Sir John Wolfe Barry, the eminent engineer; Prof. C. Christiansen, Professor of Physics at the University of Copenhagen from 1886–1912; Dr. A. M. W. Downing, F.R.S., lately Superintendent of the Nautical Almanac Office; Prof. Emile Durkheim, editor of the *Année Sociologique*; 2nd Lieut. Entwistle, Second Assistant at the Observatory, Cambridge; Dr. G. P. Girdwood, Emeritus Professor of Chemistry, McGill University; Lieut. Cyril Green, of the Botanical Staff, University College, London; the Rev. Dr. Greenwell, archæologist and angler; Mr. Charles Hawksley,

ex-President of the Institution of Civil Engineers; Dr. F. P. Mall, Professor of Anatomy in the Johns Hopkins University; Prof. R. Nicklès, Professor of Geology in the University of Nancy; Dr. Maryan Smoluchowski de Smolan, Professor of Physics at the University of Cracow and world famous for his statistical work on the kinetic theory of matter.

The following *communiqué*, which appeared on January 24, will be of interest here:

"The attention of the Secretary of State for War has been called to a statement made on January 8 by Sir Harry Johnston at the annual meeting of the public schools science masters, in which the public was informed that his lecture 'would have been delivered by Sir Ronald Ross had it not happily occurred to the War Office, after three years of warfare in very malarious regions, that the greatest and most practical authority on malaria might be of some use in directing the operations of local hygiene.' It is important that it should be generally known that Sir Ronald Ross has been working with the War Office throughout the war, and has had the supreme control and direction of advice in all matters relating to his special branch of knowledge."

It is reported that the British Science Guild proposes to hold next June an exhibition illustrative of the advances that have been made in scientific appliances since the commencement of the war. No definite public announcement had, however, been made at the time of writing, and certainly in the present congested state of scientific work a representative collection would seem impossible.

The dye question has been somewhat to the fore of late. On November 21 the *Evening Standard* published a strong article by Prof. Armstrong criticising the methods of the company subsidised by the Board of Trade, and suggesting in particular that one dye-stuff company was in the hands of a Swiss company, itself under German control. Happily this has proved not to be the case, and when Prof. Armstrong was informed of the true facts he withdrew his allegations in a letter of apology published by the *Standard* on January 23. In consequence a libel action against him was settled, Mr. Justice McCardie, before whom the case was heard, remarking that "The plaintiffs have stated their case and the defendant has acted with great fairness. The record will therefore be withdrawn." Meanwhile, the daily press had made some considerable pother over the supposed "capture" of 257 German dye "recipes" in circumstances crowded with desperate adventure. As was sufficiently obvious from the first, the story was almost entirely untrue, and the incident closed with an admirable article, also published by the *Evening Standard*, pointing out to the public how alone success in such matters could be attained.

The Institution of Civil Engineers completed the hundredth year of its existence on January 2, having been established in 1818 at a meeting of eight engineers at the Kendal Coffee House in Fleet Street. Present-day conditions precluded the formal celebration of the centenary, but a statement commemorative of the founding of the Institution was made at the Ordinary Meeting held on January 8.

Science reports that Vilhjalmur Stefanssen, the Arctic explorer, last heard from in a letter received in March 1916, has arrived with his party at Fort Yukon. Stefanssen has been leading the Canadian Arctic Expedition in the far North since 1913, and there was some anxiety as to his safety. It is also stated that the English Government buildings on the Pelican Islands (West Indies) have been placed at the disposal of the expedition of Iowa scientists, which will carry out scientific research in that neighbourhood next summer under the direction of Prof. C. C. Nutting.

The total eclipse of the sun which occurs on June 8 will be visible in the

U.S.A. over a narrow strip of country extending from the State of Washington through Colorado to Florida. Careful estimates of the cloudiness in June have been made along the path of the shadow for several years, and as a result it has been decided that the principal party from the Yerkes Observatory shall be stationed at Green River, Wyoming, a town in the so-called Red Desert, situated at an elevation of 6,000 ft. A secondary site for minor instruments, near Matheson, Colorado, has also been selected in case clouds should interfere with the observations from Green River.

The Ramsay Memorial Appeal is making satisfactory progress, the donations and promises received up to date amounting to £30,500 (the sum required being £100,000). This does not include contributions which are already collected or are being collected outside the United Kingdom. A munificent gift of £5,000 was received by the Treasurers from Mrs. Wharrie shortly before Christmas. The Lord Mayor of London has consented to issue an appeal in the City of London at an early date. The objects of the Fund should appeal strongly to London citizens in view of Sir William Ramsay's long connection with London as a Professor of Chemistry.

The Ramsay Memorial Committee have recently issued two Memoranda signed by the Rt. Hon. H. H. Asquith, President of the Fund; the Rt. Hon. H. A. L. Fisher, one of the Vice-Presidents; and the Rt. Hon. Lord Rayleigh, Chairman of the General Committee. These Memoranda contain important proposals for the foundation of Ramsay Memorial Fellowships by each of the Governments of the British Dominions, Colonies, and Dependencies on the one hand, and by each of the Governments of the chief Allied and Neutral Powers on the other hand. It is proposed that the Fellowships thus to be founded should be tenable in the United Kingdom by a chemist trained in the country providing the Fellowship. These proposals have been communicated by Lord Glenconner to the principal Governments concerned, and are now under consideration.

In their report, dated February 1, the Executive Committee of the Provisional Association of British Chemists state the results of their preliminary negotiations with the Council of the Institute of Chemists. They proposed—

(*a*) That the Institute should become the sole registration authority for chemists, apart altogether from the granting of the diplomas A.I.C. and F.I.C.

(*b*) That, failing this, it should adopt as a minimum qualification for A.I.C. the standard demanded by the Association for its own membership.

(*c*) That Local Sections of the Institute should be formed, and that the Council should consist largely of representatives from such Sections.

(*d*) That should the Institute decide on having a separate registration body, the latter should be self-governed and should carry out the objects of the proposed British Association of Chemists.

The Council of the Institute in their reply consider that the objects of the Association are practically included in the aims and objects of the Institute, and deprecate the formation of another body of chemists for carrying out those objects. They do not deem it advisable to adopt proposal (*a*). They are willing to modify the existing regulations so as to include as many chemists as possible as far as their charter permits; also to permit the formation of local branches, and finally to revise the present system for the election of the Council in order to give the general body of members greater freedom of nomination. The executive of the B.A.C. are continuing the negotiations, and will call the National General Meeting after their Local Sections have had an opportunity of discussing the final proposals.

In an interview with a representative of *The Observer* on January 19, Lord

Leverhulme discussed the question of the future of Capital and Labour after the war. The arrears in shipbuilding and housebuilding and the depletion of our stocks of goods, both for home use and export, will create an enormous demand for labour and machinery, so that the men who are returning from the war will all be required in their former places in our industries, and as many of the women also as can be spared from their home duties. The engineering works will be engaged on our requirements for ships and railways, and it will not therefore be possible at once to replenish the machinery required in other industries. In order to cope with the demand existing machinery will need to be worked for a longer number of hours. This can best be done and the biggest output best secured by reducing the eight-hour day to a six-hour day for workers and working from two to four shifts every twenty-four hours for machinery. It is estimated that capital invested in machinery is accountable for more than 90 per cent. of the cost of production apart from the cost of raw materials. By working the machinery longer and increasing the output this proportion will be reduced and, provided increased production follows from fewer hours, there should be a margin left over wherewith the workman can be paid actually higher wages for the six-hour day than he now receives for the eight-hour day. The leisure so obtained could be made beneficial in many ways, in particular education of an appropriate kind should be compulsory up to the age of twenty-four. It might then be possible to educate workmen in such a way that they could take a seat on boards of directors, from which the gain to the industries of the country would be incalculable.

Mr. A. A. Campbell Swinton delivered a most interesting address entitled "Science and its Functions" to the Royal Society of Arts at their opening meeting on November 21 last in which he traced the indebtedness of the human race from the earliest times to scientific method and discovery. To arrive at some measure of the vast changes that have been brought about, the position in 1754, the year in which the Society of Arts was founded, is compared with that of to-day. At that time travel of all sorts was no more rapid or convenient than in the days of the Romans; public lighting was non-existent; printing slow and expensive. There were no proper systems either for water supply or sewage disposal. Little or nothing was known of the causes and nature of illness, of infection by bacilli or of treatment by inoculation. Anæsthetics had not been applied and the marvels of modern surgery were undreamt of. In fact, the general mode of life showed little improvement on the conditions in civilised Europe in the days of the Antonines. The quotation which follows is very much to the point: "Since the beginning of the world it is not to the masses but to the few exceptional individuals that all great advances have been due, and it is greatly to be deprecated that politicians who must, or at any rate should, know better, continue to flatter the so-called working man by telling him that he alone is the creator of wealth. . . . Still, it is highly necessary that the masses should be educated to learn that unless those who have the requisite capacity are afforded the necessary leisure and facilities to work at research and invention, industries can be neither developed nor even maintained in the face of the world's competition, and that the working man himself will be the principal sufferer from the resulting stagnation and decay."

In his Presidential Address delivered to the Röntgen Society on November 6, 1917, Capt. G. W. C. Kaye gives an account of some of the most recent advances in the theory and practice of X-rays. The war has caused a wide extension of their usefulness, *e.g.* for the detection of contraband metals, the examination of autogenous welds, and the detection of faults and blow-holes in steel and other

metal castings. With high voltages and heavy outputs steel plates more than 1 in. thick have been examined successfully. Extraordinary outputs have been obtained by Coolidge on experimental water-cooled models of his tube; one was run continuously for many hours at 0·2 ampere and 70,000 volts, the power output being equivalent to 19 h.p., and he anticipates shortly being able to increase this to about 70 h.p.! It is the medical application of X-rays which is, however, all important. Their use has extended enormously and stereoscopic methods have attained such delicacy that the location of small foreign bodies near the eye or actually in the eyeball can be carried out to the hundredth of an inch. The rays have proved their value in diagnosing chest complaints, especially incipient tuberculosis; they are used in dental work and aid plastic surgery in its work of repairing the terrible mutilations caused by shell wounds of the face and head. Turning to the theoretical aspects of the subject Rutherford has recently given evidence for the belief that the wave length of the hardest γ rays from RaC is about ·01 Å.u. while the longest X-ray so far measured by Siegbahn has a wave length of 12 Å.u. We are thus acquainted in this region of the spectrum with an unbroken range of frequencies extending over 10 octaves. There is then a gap of 5 octaves till we reach 420 Å.u., Richardson and Bazzoni's limit in the Lyman region.

The First Report of the Bristol Grammar School Scientific Society, which has been forwarded to us by the Secretary, shows that the science work in that school is in a very healthy condition and that masters and boys alike are full of enthusiasm. Two evenings each week are given up to practical meetings in the chemical laboratories, each member being expected to draw up for his own use an account of the experiments he performs—a most essential feature but a real criterion of enthusiasm. Lectures are given at frequent intervals by the boys themselves. They are forming an admirable library, including early editions of the works of the eighteenth and nineteenth century chemists and also a herbarium.

We have received two pamphlets dealing with the Channel Tunnel question. The first, written by Mr. Arthur Fell, M.P., Chairman of the House of Commons Tunnel Committee, deals with the political aspects of the matter. The Cabinet, after consultation with their naval and military advisers, are still opposed to its construction after the war is over. No reasons for this decision have been made public and it has, apparently, caused some feeling of disappointment and disquietude in France. The commercial advantages of the scheme are obvious, for it would bring London within six hours of Paris and put it into direct railway communication, without change of gauge, with all parts of Europe except Russia and Scandinavia. The interchange of passenger traffic between England and the Continent is extraordinarily small in comparison with that between any two continental countries. For instance, in 1911 it was only 1,600,000 as against 2,800,000 between France and Germany and 4,350,000 between France, Belgium, and Holland. Moreover, for three English passengers who go to the Continent only one continental passenger comes back; and if the traffic could be equalised by the construction of the tunnel, it is estimated that England could practically pay the whole cost out of the savings for one year. These geographical aspects of the matter are dealt with in the second pamphlet, which is a reprint of a paper read by Sir Francis Fox, M.Inst.C.E., before the Geographical Society. There are no geological difficulties to be overcome, for the tunnel would be cut through Grey Chalk except near the two coasts, where it would pass through the Gault, and both of these beds are almost if not quite impervious to water. Two tunnels would be bored each about 20 ft. in diameter with cross tunnels every 200 yards;

the depth below the channel bed being not less than 100 ft. The tunnels could be worked, ventilated, and pumped by electricity from a power station in Kent ten miles inland. As to its defence, the Commandant of Dover Castle would control a water lock formed by a dip in the rail level whereby, in case of emergency, the tunnel could be filled with water from floor to roof for a length of a mile, while the entrance would be dominated by the guns of the Dover forts. As Mr. Fell states, the tunnel would be an outward symbol of the permanent friendship between the two peoples, and it is to be hoped that the present decision will not be final.

The Lancet (January 26, 1918) contains an interesting article on the "Cause and Prevention of Myopia" by Dr. F. W. Edridge Green. It is generally agreed that myopia is an acquired defect resulting from the elongation of the eyeball, but its causation has not yet been determined; the evidence for the assumption that near work produces near sight being unsatisfactory. Dr. Edridge Green claims that the primary and essential cause is an obstruction of the outflow of the lymph which should empty itself from the lymph space between the retina and choroid into that of the optic nerve. The accumulation of fluid increases the intra-ocular tension and, distending the sclerotic (whose posterior part is weak), causes the eyeball to become elongated The obstruction may be produced by severe muscular effort, *e.g.* by lifting heavy weights, when a feeling of tension is always felt in the eyes, which appear, in extreme cases, to be starting from the head. This is more especially the cause of progressive myopia among warehouse boys, porters, and others whose work involves excessive effort, while with those engaged in sedentary occupations the form of exercise taken may be responsible, *e.g.* wrestling, rowing, digging, and, also, coughing. Thus when signs of commencing myopia appear anything likely to increase the intra-ocular tension should be avoided while, on the contrary, young hypermetropes should take as much exercise as possible. There is no need to avoid reading. No comments on the theory have yet appeared in print.

The report of the Medical Research Committee on the causation and prevention of T.N.T. poisoning is a most interesting document. An immediate result of the absorption of T.N T. into the system, whether through the skin or otherwise, is a chemical change in the hæmoglobin of the red blood corpuscles which shows itself in a pallor or duskiness of the skin with blueness of the lips. This cyanosis does not by itself produce fatal results, and if fresh entry of the poison is prevented the body rapidly recovers. But if the effects are permitted to accumulate two incurable and rapidly fatal diseases may occur, namely, "toxic jaundice" as a result of the destruction of liver substance or, more rarely, "aplastic anæmia" due to the blood-forming organs failing in their function so that a progressive loss of blood takes place. These diseases, however, do not always result from even severe cyanosis; while on the other hand there seems to be evidence that they have developed without that danger signal first appearing. Different persons have vastly different susceptibilities; very many are quite immune. Thus one of the main causes of the decreased incidence of disease in 1917 as compared with 1916 is that workers whose power of resistance is small have taken up other work. The investigation, which was carried out by Dr. B. Moore, F.R.S., also showed that the main source of absorption was through the skin and not through the respiratory organs. Not only is the T.N.T. which is breathed in as dirt in the air much less harmful than that rubbed in the hands and forearms, but also the amount absorbed per shift is small and easily ejected from the system. Elaborate methods of ventilation are not so necessary as skin

protection. The hands and face can be covered effectively by a casein varnish (which also protects them against tetryl stain), while the legs and feet must be protected by suitable boots.

The Munitions Inventions Department has issued a report on the Nitrogen Problem. Apparently the Department was first made to realise the fundamental importance of the matter by the inauguration of the submarine campaign in February 1916, and also by a memorandum on the subject submitted to it at the time by the Faraday Society. As a result the Nitrogen Products Committee was formed in June 1916 for the purpose of exploring the whole problem and of carrying out the experimental work necessary for the investigation of such processes as might appear to be of value. This work has been carried on in the new Ramsay Laboratory at University College, the first item being an investigation of the Haber process (SCIENCE PROGRESS, January 1918), the commercial details of that process being almost entirely unknown. Later, the Committee decided to investigate the ammonia oxidation process as it is well adapted as an emergency measure for securing quickly a considerable output of nitric acid. An interim report was sent to the Minister of Munitions in February 1917, and he decided (*a*) to undertake the erection of one Government plant for the oxidation process; (*b*) to permit the erection of a full-sized trial unit for the synthetic (Haber) process; (*c*) that the Committee should investigate the relative merits of certain schemes for working the cyanamide process. In addition the Committee was requested to deal with the problem of by-product ammonia from gas works and coke-oven plants. This part of the work was subsequently handed over to the Explosives Department, which has also assumed the responsibility of establishing the oxidation process on a large scale. Studies of the synthetic process led to a method of working whereby the output of ammonia per unit of catalyst space has been increased to a figure which, so far as is known, exceeds anything hitherto obtained. A semi-technical unit has been built, and it is expected that its operation will enable the remaining problems connected with the chemical engineering details involved in the erection of the full-sized plant to be solved.

Bulletin No. 1 *of the Department of Scientific and Industrial Research* is a memorandum on the Preservation of Timber in Coal Mines written by Prof. P. Groom. The present shortage of timber supplies has rendered the prevention of wastage in pit timber a matter of national importance. A large amount of timber in mines, and especially in shallow coal pits, requires to be replaced prematurely because it undergoes decay and consequently collapses. Such decay or disintegration is, at least for the most part, caused by various fungi which permeate and devour the wood, and often clothe its surface with a fluffy or cottony material ("spawn"). This spawn, usually white but sometimes coloured, can rapidly grow over the timber, walls or floor, and may even be broken off and transferred elsewhere, and thus reach and infect sound wood. These wood-destroying fungi also produce fructifications, which emit countless microscopic germs ("spores"), which, conveyed through the air, may alight on, and attack, pit-wood. The wood can be rendered immune to infection by fungi by impregnating it with an antiseptic substance such as creosote or zinc chloride, the degree of protection depending on the depth of penetration, complete immunity being secured most economically by pneumatic pressure. As a remedial measure the superficial growth of the fungus can be checked by cutting away the fructifications in the accessible timber and washing with an antiseptic solution. Above all it is important to prevent infection of sound timber by contact or by wind-borne spores.

The Department has also issued a second edition of the Report on the Sources and Production of Iron and other Metalliferous Ores used in the Iron and Steel Industry (H.M. Stationery Office, Imperial House, Kingsway, W.C.2., price 2s. net), prepared by Mr. G. C. Lloyd, Secretary of the Iron and Steel Institute The report contains a most comprehensive survey of the history, composition, and statistics of ores from all known sources. The data concerning the Briey mines are of particular interest. In 1913 they produced over 15,000,000 tons of ore; the total from the whole of France being 21,750,000, and from Germany 36,000,000. In the same year Germany imported 14,000,000 tons—so that if she retained the Briey basin (and it is now entirely in enemy possession) she would just be independent of imports (on the 1913 basis), and would reduce the French supply to one-third of its original amount. Fortunately, however, the supplies of tungsten ore available for the Central Powers are very small, and, apart from Russia, their position with regard to manganese and chromium is almost equally bad. In pre-war days Russia was the main source of manganese; but her ore output fell from 1,171,000 tons in 1913 to 9,770 in 1916!

The Statistics Bureau of the International Institute of Agriculture (Villa Umberto, Rome) has issued a pamphlet descriptive of their publication entitled *International Year Book of Agricultural Statistics*, 1907-1916, which contains some interesting information. It appears that the ascertainable annual yield of wheat throughout the world exceeds 1,000,000,000 quintals (10 quintals are approximately equivalent to 1 ton), and represents at present value more than £2,000,000,000, while the aggregate value of the six chief cereals (wheat, rye, barley, oats, maize, and rice) is not less than £6,000,000,000. The yield of potatoes is over 1,500,000,000 quintals, and of sugar beet 500,000,000 quintals. Every year the world has at its disposal 150,000,000 quintals of beet and cane sugar, nearly 150,000,000 hectolitres of wine (1 hectolitre is equivalent to 22 gallons), 10,000,000 of coffee, 8,000,000 of leaf tobacco, and 1,000,000 quintals of hops. Textile industries account annually for nearly 50,000,000 quintals of cotton, 8,000,000 of flax, and 7,000,000 of hemp, while silkworm breeders in Europe and Asia deliver to the trade more than 200,000,000 in cocoons. Turning to livestock, there are in Uruguay eight head of cattle to each inhabitant, in Argentina more than four head, in Australia more than two, in the United States and Canada about one, and in Europe only one to two persons.

In these notes last quarter some doubts were expressed as to the optimistic views of the Australian Council of Science and Industry on the future of research workers in that country. Apparently they were only too well founded, for in the *Australian Manufacturer* (November 3, 1917) it is stated that the Department of Agriculture for New South Wales was advertising for "an assistant experimentalist with sufficient training to enable him to assist in planning and carrying out scientific experiments—salary £75 per ann."—less than that of a grocer's assistant. Coming from a Government department of a semi-scientific character such an advertisement is shameful in the extreme, though it is, of course, only too characteristic of present-day conditions in the British Empire. It is almost matched by another advertisement inserted in *Nature* (January 24, 1918) by the L.C.C. Education Officer desiring applications for a visiting teacher of science at a trade school, the remuneration being no less than 7s. 6d. for an afternoon's attendance. The proposed Union of Scientific Workers is, indeed, long overdue, and it is to be hoped most earnestly that the organisers of the Union will be able to guide it to complete success.

ESSAYS

MEAT PRODUCTION (J. Alan Murray, B.Sc., University College, Reading)

BUTCHER'S meat consists generally of the dressed carcases of oxen, sheep, and pigs. Two phenomena contribute towards its production—viz. growth and fattening of the animal. In a popular sense these terms are well understood, but they have not been defined, and the phenomena are often confused or imperfectly distinguished. They are, however, distinct; either may occur apart from the other.

Growth implies, essentially, extension of the frame and, normally, in conjunction therewith, development of other tissues associated with the same. Fattening implies the formation of new tissue apart from and independently of any extension of the frame. Growth is always positive; the skeleton develops gradually to its maximum size, and does not again contract at any stage within the lifetime of an animal destined for butcher's meat. Fattening may be either positive or negative; lean animals become fat, and those which are fat may again become lean. Such changes are referred to, technically, as alterations in the "condition" of the animal. Any alteration in condition or in size alters the mass of the body, but a negative change in the former may be wholly or partially compensated by increase in the latter. The live weight, by itself, therefore, tells nothing. The test of condition is the ratio of mass to size, M/V—*i.e.* the apparent density of the body. Determination of size by direct linear measurement is a matter of considerable practical difficulty.

The term "fattening" is employed, technically, to describe what happens when animals consume considerable quantities of readily digestible foods in excess of maintenance requirements. Under these conditions the animals increase in weight and become visibly fatter. But the conditions are also favourable to growth, and if the animal has not already attained its full stature it develops in this direction also. The whole of the increase produced is commonly referred to as "fattening increase," though, as a rule, in modern farming practice, much of it is due to growth.

In experiments intended to establish relations between food consumed and increase produced, failure to discriminate between growth and fattening reduces the scientific and economic value of the results almost to vanishing point. The two kinds of increase differ in composition and in the rate at which they are formed. Apart from these considerations the total quantity has but little significance.

Lawes and Gilbert slaughtered certain lean and fat animals and estimated the composition of the entire bodies by chemical analysis. From these data and the recorded weights of various groups of animals in lean and fat condition they calculated the composition of the increase as a whole. The results varied, of course, according to the amount of increase made by the several groups. As a rule only the averages are quoted in textbooks, and this seems to have given rise to a widespread impression that the composition of the increase is constant. That it is approximately constant under like conditions is more than probable; but conditions alter and farming practice varies. A rather drastic change has recently been recommended.[1] Other inferences may be drawn from the analytical

[1] *Journal of Board of Agriculture*, vol. xxiii. No. 10, p. 986.

data when considered in conjunction with the weights of the animals actually analysed, and these suggest generalisations on somewhat different lines.

TABLE I[1]

Net weight and chemical composition of the whole bodies after deducting contents of stomachs and intestines:

	Composition				Net weight.	Ratio of water to protein.
	Ash.	Protein.	Water.	Fat.		
	Per cent.	Per cent	Per cent.	Per cent.	lb.	
Half-fat ox	5·01	18·08	56·10	20·81	1131	3·10
Fat ox	4·15	15·43	48·40	32·02	1334	3·13
Half-fat old sheep . .	3·52	15·38	55·28	25·82	95·55	3·59
Fat sheep	2·98	12·97	46·17	37·88	119·38	3·62
Extra-fat sheep . .	3·06	11·50	37·13	48·31	239·37	3·23
Store pig	2·85	14·44	38·13	24·58	89·11	4·02
Fat pig	1·66	11·36	43·02	43·96	177·60	3·78

The actual weights of the several constituents in the body of each animal are given in Table II below. The differences between the quantities in the fat animals and those in the lean are regarded as the amounts in the increase represented by the difference in the net weights, and these amounts may be expressed as percentages of the latter.

TABLE II

Actual composition of whole bodies and of difference between lean and fat animals:

	Ash.	Protein.	Water.	Fat.	Total.
	lb.	lb.	lb.	lb.	lb.
Fat ox	55·36	205·84	645·66	427·14	1334
Half-fat ox	56·66	204·49	634·49	235·36	1131
Difference	−1·30	1·35	11·17	191·78	203
Fat sheep	3·56	15·48	55·12	45·22	119·38
Half-fat old sheep	3·36	14·70	52·82	24·67	95·55
Difference	0·20	0·78	2·30	20·55	23·83
Extra-fat sheep	7·32	27·53	88·88	115·64	239·37
Half-fat old sheep	3·36	14·70	52·82	24·67	95·55
Difference	3·96	12·83	36·06	90·97	143·82
Fat pig	2·95	20·18	76·40	78·07	177·60
Store pig	2·54	12·87	51·80	21·90	89·11
Difference	0·41	7·31	24·60	56·17	88·49

[1] *Rothamsted Memoirs*, vol. iii. pp. 520, 558 *et seq.*

TABLE III

Percentage composition of increase:

	Ash.	Protein.	Water.	Fat.	Increase upon 100 original weight.
	Per cent.	Per cent.	Per cent.	Per cent.	
Ox	−0·64	0·67	5·50	94·47	17·95
Sheep (fat)	0·84	3·27	9·65	86·24	24·94
Sheep (extra fat)	2·75	8·92	25·08	63·25	149·47
Pig	0·46	8·26	27·80	63·47	99·30

These results differ substantially from the averages published by Lawes and Gilbert, and they are applicable only to the particular cases. In general, the percentage composition of the increase may be computed directly by the following formula:

$$P = \frac{MF - mf}{M - m}$$

where M and m are the net live weights, F and f are the percentages of fat (or any other ingredient) in them respectively (see Table I), and P is the percentage in the increase.

All the factors in the above expression are variable. The live weight of cattle ranges from about 50 or 60 lb. at birth to over 2,000 lb. when fully grown. M and m may have any values between the extremes, and M may be equal to or even less than m. The data in Table I are not limits. Atwater and Bryant[1] found less than 1 per cent. of fat in sides of beef described as "very lean." Apparently, in live animals, the fat may vary from about 5 to 50 per cent. though the limits ordinarily attained are perhaps somewhat narrower. At all events, F and f may have any values between the extremes, whatever these may be, and F may be equal to or even less than f.

Variation in the percentages of the other ingredients is almost entirely limited to that due to variation in the percentage of fat. Therefore, in any given animal,

$$p' = p(100 - F)/(100 - f)$$

where p is the percentage of ash, protein, or water in the body when it contains f per cent. of fat, and p' is the percentage when it contains F per cent. The theoretical maxima (*i.e.* when $F = 0$), calculated from the data in Table I, are as follows:

TABLE IV

Composition of Non-fatty Matter:

	Half-fat ox.	Fat ox.	Old sheep.	Fat sheep.	Extra-fat sheep.	Store pig.	Fat pig.	Means.
	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.	Per cent.
Ash	6·4	6·1	4·7	4·8	6·0	3·8	2·9	4·96
Protein	22·8	22·7	20·7	20·8	22·2	19·1	20·3	21·23
Water	70·8	71·2	74·6	74·4	71·8	77·1	76·8	73·81

When $F = 50$ the percentages are just half those shown in the table, and these are approximately the limits.

It appears, therefore, that the composition of the increase may vary very widely. The percentage of fat in it may be zero, or even a minus quantity, but

[1] *Bull.* 28, *U.S. Dept. of Agric.* 1906.

it cannot be greater than 100. In the case of the ox (Table III) the increase contained 94·5 per cent. of fat. In the other cases it contained less fat but comprised considerable amounts of protein tissue in which 3 or 4 parts of water are associated with 1 of dry matter.

Formation of protein tissue is involved in the processes of growth. In fully grown animals it occurs only when they are in very low condition; it is gradually arrested as the condition improves and finally ceases altogether. Any increase beyond this point consists entirely of pure fat. For present purposes the condition at this stage may be called the par condition.

Normally, growth implies the formation of fatty as well as of protein tissues. The ratio between these may or may not be altered. In other words, growth may take place without alteration of condition, or it may be accompanied by a relative gain or loss of fat.

Growth and fattening may take place simultaneously or successively. In the latter case the composition of the increase depends upon the amount of increase considered. For example, suppose an animal, containing 20 per cent. of fat, to increase from 100 lb. to 1,050 lb. without change of condition, and that, when it reaches 1,200 lb., it contains 30 per cent. of fat; the increase, if reckoned from 1,050 to 1,200 lb., contains 100 per cent. of fat, but if reckoned from 1,000 lb. to 1,200 lb. it contains only 80 per cent., if reckoned from 900 lb. to 1,200 lb. only 60 per cent., and so on.

When growth and fattening take place simultaneously the case is similar. When there is no growth the increase consists entirely of fat. When there is no fattening the composition of the increase is the same as that of the original body. In any other case the composition of the increase depends upon the relative amounts of growth and fattening.

In young animals not below "par" condition, the whole of the non-fatty matter of the increase must be attributed to growth; and the amount of this non-fatty matter, plus fat in the same ratio as in the lean animal, gives the amount of the increase due to growth. The difference between the total increase and that due to growth is, of course, the true fattening increase. If the two parts be designated by F and G respectively, then:

$$F = M\left(1 - \frac{100 - F}{100 - f}\right) \text{ and } G = \frac{M(100 - F)}{100 - f} - m.$$

The amounts of growth and true fattening increase, calculated by these formulæ from the data in Table I, are as follows:

	Ox.	Pig.	Fat sheep.	Extra-fat sheep.
	lb.	lb.	lb.	lb.
Growth	14·1	42·85	4·42	71·25
Fattening	188·9	45·64	19·41	72·57
Total increase	203·0	88·49	23·83	143·82
Same per 100 original weight:				
Growth	1·24	48·08	4·6	74·9
Fattening	16·70	51·22	20·3	75·6
Total increase	17·94	99·30	24·9	150·5
Same per cent. of total increase:				
Growth	7·0	48·4	18·5	49·7
Fattening	93·0	51·6	81·5	50·3

The increase due to growth, being of the same composition as the lean animal, may be added to the weight of the latter, and the true fattening increase expressed as if growth and fattening had taken place successively, thus:

	Ox.	Pig.	Fat sheep.	Extra-fat sheep.
	lb	lb	lb.	lb.
Original net weight	1131·0	89·11	95·55	95·55
Increase due to growth	14·1	42·85	4·42	71·25
Total grown weight	1145·1	131·96	99·97	166·80
True fattening increase	188·9	45·64	19·41	72·57
Total weight of fat animal	1334·0	177·60	119·38	239·37
True fattening increase per 100 grown weight	16·5	34·08	19·42	42·22

To facilitate comparison with the ox, the data in the preceding tables may be summarised as follows:

	Ox.	Pig.	Fat sheep.	Extra-fat sheet.
	lb.	lb.	lb.	lb.
True fattening increase per 100 grown weight	1·0	2·2	1·2	2·6
Growth increase per 100 original weight	1·0	38·7	3·7	60·4
Percentage of fat in growth increase	20·8	24·6	25·8	25·8

The calculations recorded above were based on the assumption that growth includes formation of fat in the proportion in which it is present in the lean animal. This was not the same in all cases; the ox contained substantially less fat than the others. The meat produced by growth was, therefore, less valuable, weight for weight, in the former than in the latter. In fact, "lean" and "fat" are merely vague popular terms. In a scientific sense their signification is purely relative, and the percentage of fat in animals described by these terms is fortuitous.

Both the ox and the pig eventually became fatter than the lean sheep, and must, therefore, at one stage, have contained the same amount of fat. The live weight corresponding to that stage is easily calculated, and, had the increase been reckoned from that point, the percentage of fat in the growth-increase would have been the same in all. By a similar adjustment of the live weights the growth increase might have been reckoned, in each case, from the point at which the percentage of fat was the same as in the lean ox or from any other stage whatever.

The increase due to growth includes the whole of the non-fatty matter in it and possibly also some of the fat, but how much is uncertain. The remainder of the fat is true fattening increase. It is produced with increasing difficulty as the condition of the animal improves. Does this apply, in any degree, also to the fat hitherto regarded as part of the growth increase, or does the return (in shape of fat) for food consumed begin to diminish only after a certain stage—*e.g.* the "par" condition, has been reached? It seems scarcely probable that any sharp line of demarcation should exist between fat formed by growth and that called true fattening increase. But if it be impossible to distinguish between them, theoretically, then only non-fatty matter could be regarded as growth, and the absolute zero of condition would be represented by animals entirely devoid of fat. That such an animal does not in fact exist makes the proposition somewhat unacceptable, but does not necessarily render it untenable as a working hypothesis.

The "par" condition—the point at which formation of protein tissue ceases in

fully-grown animals—might perhaps be determined by reversing the fattening process—*i.e.* by gradually reducing the rations of a fat animal so as to cause it to lose weight. It is to be expected that the loss would be, at first, confined to the fat, but that a point would ultimately be reached at which loss of protein would begin. As this would result in increased excretion of nitrogen consequent upon a reduction in the amount of food consumed, the critical point could probably be detected. If this should prove to be the case the whole question could be investigated experimentally.

If the whole of the fat be treated as true fattening increase, and only non-fatty matter as growth, the results would be as follows:

	Ox.	Pig.	Fat sheep.	Extra-fat sheep.
	lb.	lb.	lb.	lb.
Original weight	1131·00	89·11	95·55	95·55
Fat in same	235·36	21·90	24·67	24·67
Net weight of "lean" animal	895·64	67·21	70·88	70·88
Growth increase (non-fatty matter)	11·22	32·32	3·28	52·85
Weight of grown "lean" animal	906·86	99·53	74·16	123·73
Total fat in fat animal	427·14	78·07	45·22	115·64
Final weight of fat animal	1334·00	177·60	119·38	239·37
Growth per 100 lean weight	1·25	48·09	4·62	74·56
Same compared with ox	1·00	38·47	3·70	59·64
Fattening increase per 100 grown weight	47·1	78·4	60·9	93·5
Same compared with ox	1·0	1·7	1·3	2·0

Whichever point of view is adopted it is clear that in the ox the increase was due almost entirely to fattening, whereas in the pig and extra-fat sheep it was largely due to growth. Fattening increase consists of pure fat. Growth increase consists mainly, if not entirely, of non-fatty matter of which about three-fourths is water. Growth increase is, therefore, less valuable and less food is required to produce it.

The importance of these facts with reference to the question of comparative efficiency of animals as meat-producers is obvious, but even now it is not always fully appreciated. Thus, many farmers hold that cattle should be fattened while still young "because they put on weight more rapidly." This conclusion may be sound, but the argument is false, for it ignores the fact that the additional gain of weight is largely due to growth and that the composition of the increase is not the same. It has not been established that young animals fatten more rapidly than those which are fully grown. That question remains open.

Again,[1] Warington has declared that "the pig is the most economical meat-making machine at the farmer's disposal." This may be true, but the experimental evidence on which it is based seems too slender to support the inference. In order to justify the statement it would be necessary to show either (1) that pigs require less food for maintenance, or (2) that they produce equal amounts of growth or of fattening increase from less food or in less time than in other animals under similar conditions.

According to the surface law pigs and calves of the same weight should require about the same amount of food for maintenance, but ten pigs, each of 100 lb., should require about twice as much as one ox of 1,000 lb. live weight. These inferences are consistent with the results obtained in Sanborn's experi-

[1] *Chemistry of the Farm.*

ments,[1] but the maintenance requirements of fully grown pigs have not been exactly determined. As regards the quality of the food required, pigs compare unfavourably with ruminants.

The digestive powers of pigs are inferior to those of ruminants, and no evidence is forthcoming to show that pigs retain, either as growth increase or as fattening increase, larger amounts of the digested nutrients given in excess of maintenance requirements.

Warrington has also declared that, "as a natural result of the larger consumption of food, the pig increases in weight much more speedily than either the sheep or ox." This, however, is contrary to the fact. If a pig of 100 lb. and a calf of the same live weight be fed for a year the latter will be the heavier at the end of that time. This comparison is perhaps unsound. To compare the amount of increase made by a young growing pig with that of an ox that is already fully grown, or nearly so, is absurd.

There is some reason to believe that young pigs produce true fattening increase more rapidly than cattle or sheep. It remains to be seen whether they fatten more rapidly than calves or lambs of the same or corresponding age. With regard to the rate of fattening of fully grown pigs—*e.g.* old sows—data are lacking.

These considerations afford grounds for scepticism as to the alleged superiority of the pig as a converter of fodder into meat, and much careful research will be required before the question can be finally determined. Available data bearing on questions of meat production in general are incomplete and unreliable, and it is difficult to place much confidence in some of the inferences which have been drawn.

A committee on Food Supply, appointed by the Royal Society at the request of the President of the Board of Trade, has recently issued a Report[2] in which it is stated that "considerable economy of fodder can be effected by varying the kind of animal to which it is fed. All animals are not equally good 'converters,' as is shown by the following figures: starch equivalent required to produce 1,000 calories in the form of—pig meat, 3·0; baby beef, 7·0; steer beef, 9·0." The committee also recommends that cattle should be slaughtered at the age of seventeen months.

No notice is taken of the "inferior quality"[3] of baby beef. In time of war, no doubt, that is a matter of minor importance; but if calories for starch equivalent be accepted as the sole criterion of economy of fodder the committee might have gone further. It seems clear that the greatest economy, in the sense referred to, will be attained by slaughtering the animals unfattened—*i.e.* in the "par" condition, at or about the point of maximum daily growth. This point does not seem to have been exactly determined, but, in the case of cattle, it will probably be found in the neighbourhood of from nine to twelve months—at all events earlier than the age recommended. The general effect of all the points made by the committee in this connection is to magnify the importance of growth increase as compared with that due to fattening. Hitherto, practically all recorded experiments which have attempted to distinguish between the two have dealt exclusively with the latter. Few, if any, experiments on growth increase have been described, and exact information on this subject is lacking. Even what is covered by the term is still uncertain.

[1] *Bull.* 28, *Missouri Agric. Coll.*

[2] *Journal of the Board of Agriculture*, vol. xxiii. No. 11, p. 1046.

[3] *Standard Cyclopædia of Agriculture.*

ESSAY-REVIEWS

THE SHAPING OF THE EARTH: A Study in Geological Physics, by G. W. BULMAN, M.A., B.Sc.: on Papers by M. ÉMILE BELOT, Directeur des Manufactures de l'État, Paris:

(1) **Le premier de tous les Déluges. Lois de répartition des mers et des continents primitifs.** Extrait du volume des *Comptes Rendus de l'Association française pour l'Avancement des Sciences*, pp. 354–67, 1914;

(2) **L'Origine cosmique des formes de la Terre.** *Revue scientifique*, May 27 to June 3, 1916, pp. 327–34.

WHEN the British Association has just appointed a Committee for the investigation of geological physics—or, as they call it, geophysics—the time seems opportune for calling attention to the work of a French *savant*, M. Émile Belot, on this important subject.

The future progress of geology imperatively demands an amalgamation with the other sciences. Or rather, perhaps, a recognition of the fact that the study of the Earth *includes in itself all the sciences* For at the very beginning of geological history we find ourselves in the great *terra incognita* of the chemistry and physics of a mass of intensely heated vapours. It is to be feared that geologists in general have ignored rather than attempted to solve the problem thus presented. Most of them have been content, with Hutton, to see no trace of a beginning in the matter of rock making. The past of the globe has been for them a lotus-land where "all things always seemed the same." Or as M. Belot expresses it, they have given themselves up to a "*quietisme bien peu scientifique*." Chemists and physicists have also, on the whole, avoided this interesting region of thought, so that one hardly knows where to turn to get any authoritative statements as to what probably happened when the incandescent vapours first condensed to form the solid Earth.

M. Belot, however, takes the view that the early making of rocks *must have been profoundly influenced* by the chemistry and physics of an incandescent globe, solidifying from a primitive nebula. He even traces the present position of the continental areas, the relative proportion of land and water at different latitudes, the tapering of land masses towards the south, etc., to forces acting at the time of the Earth's solidification. As regards the general features of the Earth, M. Belot rejects somewhat scornfully the *réseau pentagonal* theory of his illustrious countryman Elie de Beaumont, as well as the *tetrahedron* hypothesis of other writers. He claims that all the interesting and puzzling features in the distribution of land and sea, and the general geological structure of the Earth, can be explained on his view of what happened in the very beginning of Earth history—in the chemistry and physics of a cooling and condensing nebula.

A good idea of M. Belot's position may be gathered from one of his papers, "The First Deluge" (*Le premier de tous les Déluges*), read July 31, 1914, and published in the *Comptes Rendus* of the French Academy.

Something like half of an ordinary textbook of geology, M. Belot points out, is taken up with the work of water in its various forms. But geologists have not, he maintains, concerned themselves with the question, "Where and how did the constructive action of water on the Earth begin?" And he sets himself to solve the problem. It began, he believes, with a great Deluge in which nearly half the waters of the ocean descended on the South Pole of the Earth. Hence it proceeded as a mighty flood towards the North. To get this great Deluge, we start with a nebulous mass which has condensed sufficiently to have a liquid core in an atmosphere containing, among other things, all the waters of the globe. This core has a motion of translation in its atmosphere, and as a consequence of this, and of the general physics of the situation, a hollow is formed at the North Pole, and a protuberance at the South. The latter cools first, and a solid crust is formed on it. The motion of the core in the nebula, etc., determines that when the temperature has sunk below the critical temperature of water (364°) the Deluge shall occur at the South Pole. As it rushes in a mighty flood northwards it tears up masses of the already solidified crust and bears them along As the velocity lessens, some of the masses are deposited. These form barriers at which the waters divide V-wise, and in the *widening* areas behind them the land masses are built up by deposition of sediment. Hence the tapering of the land masses towards the south. But the waters descending at the South Pole are heated again by contact with the hot Earth, and as they go north ascend as vapour. So the circulation goes on, the heated waters falling chiefly at the South Pole. And it is interesting to note that to-day the South Pole is colder than the North, and that some of the water of the southern hemisphere flows into the northern as part of the equatoria current. One suggests that the South Polar showers gradually, in the course of geological ages, extended themselves so as to fall over the entire globe, and that the amount of water flowing from south to north gradually diminished, leaving only these present traces of a former state of things. And if we suppose that other agents of geological change were equally magnified in the past we are in a much better position to explain the phenomena presented by the rocks of past ages. Present-day forces do not explain the geological past. "It is vain," says M. Belot, "to seek in present-day phenomena the equivalent of those of the past, entirely different not only in quantity but in nature." So we reach the inevitable conclusion that the rate of rock formation must have been much more rapid in the past. "It is then," says our author, "the *intensity* of erosion (and not time) which has accumulated the enormous thickness of sediments of Primary age of which the duration must thus be much shortened."

As assistants to the great Deluge in shaping the Earth, M. Belot brings in three small satellites, which, he says, can be proved to have revolved round the Earth near the equator. These consisted largely of water, and when they fell to the Earth at three different geological epochs would be potent influences in modifying the Earth's crust. They would, one suggests, increase the *temperature*, as well as speed up the *rate* of rock-making. Possibly we have in them the *vera causa* of certain puzzling changes of climate familiar to geologists. They would also act as revivers of seismic and volcanic activity.

Among other things it is claimed that the great Deluge explains the universal crumpling and contortion of the Archæan rocks. The mighty rush of waters would, it is said, heap up the sediments in this way. And the fluid inclusions in granite rocks also receives a reasonable explanation. According to Dr. H. Sorby, the inclusions in certain granite rocks indicated a pressure which showed that they must have been formed at depths of from 5 to 15 miles. The new view enables

us to suppose them formed under the then existing atmospheric pressure—some 300 atmospheres—at the surface.

And if we accept the great Deluge as the first act in the drama of geological history, we must give up the idea of an originally saltless ocean. For the chlorides, and other haloid salts, in the primitive nebula would condense and be precipitated before the water. According to M. Belot, they would form a deposit round the South Pole. Then the hot water falling would dissolve them and give us a saturated ocean from the beginning. If this be accepted, then Prof. Joly's ingenious attempt to estimate the age of the Earth from the amount of salt in the ocean fails.

M. Belot gives us much food for thought.

Le premier de tous les Déluges. Lois de répartition des mers et des continents primitifs. Extrait du volume des *Comptes Rendus*.

L'Origine cosmique des formes de la Terre, *Revue scientifique*, May 27 to June 3, 1916.

Les volcans expérimentaux et les lois de la volcanicité, *Comptes Rendus*, August 1916.

Contribution à l'étude des causes du volcanisme, *ibid.* April 1916.

L'hypothèse satellitaire et le problème orogénique, *ibid.* January 1917.

Tracé provisoire de la courbe décrite par le Pôle magnétique boreal depuis 1541, *ibid.* January 1917.

Sur l'origine possible du magnétisme terrestre, *ibid.* April 1916.

ZOOLOGICAL RESEARCH IN INDIA, by J. T. Jenkins, D.Sc.: on **First Annual Report on the Zoological Survey of India for the Year 1916–17.** [Pp. xliii.] (Calcutta: Superintendent Government Printing, India, 1917. Price 1 rupee, or 1*s.* 6*d.*)

Although there has been a great change in the attitude both of the India Office and the Central and Provincial Governments in India during the last few years towards the organisation of scientific research in India, there still remains much to be accomplished before the best advantage can be obtained from the systematic investigation of Indian resources. India is no longer practically the sole country capable of turning out products of a peculiarly tropical kind, and consequently she is, and in the immediate future is likely to be still more, subject to the competition of other tropical countries. Until quite recently India produced tropical products in abundance which were exported in exchange for manufactured goods. Of recent years the opening up of tropical Africa and Java, and the remarkable development of the Philippines, have challenged the ancient supremacy of India in a striking manner. The development of European science has also, in by no means a minor degree, adversely affected Indian products, of which indigo and sugar are two examples.

This serious competition necessitates the immediate facing of the problem of the organisation of science in India, and at the outset two alternatives suggest themselves:

(1) Either the application of science to Indian conditions is of no practical value; or

(2) Science can be applied in India to the study and improvement and conservation of Indian products, whether animal, vegetable, or mineral.

There is not much need, in the light of recent events, to debate these alternatives. The slightest reflection forces us to conclude that the latter is the only one possible.

It has already been proved beyond cavil that science is able to accommodate itself to the conditions prevailing in India, and to make the most of the natural resources and the climatic advantages which there exist. What is wanted, therefore, is a thorough organisation of the various branches of scientific research so that our knowledge of Indian resources may be made the most of.

Consider the existing organisation. At the India Office there is no officer of high standing at the head of a branch or department of scientific work. The India Council itself—though a body of men of great experience and ripe judgment in administrative, legal, and military affairs—comprises no one whose authority on Indian scientific matters is of sufficient weight to carry influence in scientific circles. It is highly desirous that there should be a scientific member on the India Council, and it goes without saying that such a member should be a man of high scientific attainments with a wide experience gained in India of the necessities and requirements of Indian science.

In India itself there is a "Board of Scientific Advice" which meets annually under the chairmanship of one of the higher civilians attached to the Central Government, usually the Secretary for Revenue and Agriculture. The Board consists of the heads of the various services more particularly engaged in work of a scientific nature; for instance, the Surveyor-General, Inspector-General of Forests, Director of the Geological and Botanical Surveys, Director-General of Observatories, the Agricultural Adviser, and now a Director of the Zoological Survey. It has always been difficult to estimate to what, if any, extent the deliberations of this body assist the progress of scientific investigations in India.

Although the results of the deliberations of this Board are practically unknown outside of India, the work of the various departments referred to are well known, except that of the Zoological Survey, which is a new-comer.

The Zoological Survey of India was founded in July 1916. Previously zoological research had been carried on by naturalists "who had laboured in official obscurity for nearly one hundred years." The only official or semi-official body which concerned itself with a zoological survey was the Trustees of the Indian Museum. The staff and organisation of the Zoological and Anthropological section of the Museum have now been taken over by the Central Government as a Zoological Survey, a scientific Department of the Government of India under the direct control of the Department of Education just as the Botanical Survey is under the Department of Revenue and Agriculture. The gazetted staff for the new survey consists at present of a Director, a Superintendent, and two Assistant Superintendents, who occupied analogous positions on the Museum staff.

The first annual report of the Zoological Survey, though covering a period of eight months only, is a document of extraordinary interest to all naturalists. The first Director (Dr. Annandale), to whose energy, common sense, and tact we attribute the foundation of the Survey, is inclined to emphasise the purely scientific nature of the Survey, and in this we think he is wise. It is a mistake for scientific men to guarantee, as was done by the British naturalists (or some of them) who commenced the fishery researches for the International Council for the exploration of the North and neighbouring seas, definite results of economic importance within a brief and definite interval. An error of that kind seriously discredits scientific investigation.

On the other hand we hope that Dr. Annandale will not go to the other extreme

and neglect any branch of zoology simply because it promises to give results of economic importance. The inauguration of the Survey is to be welcomed in the first instance because it gives zoology and zoologists in India a definite status. People visiting India, whether for purposes of private research or as expert advisers to the Central or Provincial Governments on zoological products, have now a definite official bureau to which they may apply for assistance.

The great defect in the constitution of the Zoological Survey at present is the miserable inadequacy of the staff. We hope to hear of proposals for the immediate appointment (or as soon as suitable men may be available) of at least half a dozen additional officers of the rank of assistant superintendent, and of one who would occupy a post as Superintendent or Deputy Director. This is the bare minimum necessary to ensure an adequate treatment of the subject, and unless appointments of this kind can be made we think, to be quite frank, that the Government would have been better advised to leave the whole question of zoological research in its original condition.

It would be presumption on our part to attempt to advise Dr. Annandale, whose knowledge of Indian and Asiatic zoology is probably unsurpassed, as to what is required in the shape of additional staff; but for the information of the Indian Government there is no harm in saying that a zoological survey for a country like India which does not contain specialists on such groups as the Diptera, Protozoa, Higher Vertebrata, and Mollusca is hardly worthy of the name.

If any justification were needed for the creation of the Survey it is to be found in this, the first, annual report. Prior to the establishment of the Survey what may be called semi-official reports on Indian zoology were published under the title of "Records and Memoirs of the Indian Museum," and these reports are to be continued in the same form and under the same titles, but presumably with the imprimatur of the Survey. Both the Records and the Memoirs are well known to naturalists, and we agree that it is wise to continue them in their original form. Valuable information on the Indian fauna is also to be found in the "Fauna of British India," a work of some forty volumes published in London under the authority of the Secretary of State for India in Council. The chief defect of the "Fauna" is that the major part of it has been written and published in England by naturalists who, in many cases, were quite unacquainted with Indian conditions. We believe we are correct in saying that it was as recently as 1910 that volumes of the "Fauna" were first actually prepared in India. Considering that in many groups observation of the living animals is, to say the least, highly desirable, and considering also that the collections of some groups in the Indian Museum are perhaps the best in the world, it is to be hoped that in the future we shall see more and more of the volumes prepared in India itself under the direct influence of the Zoological Survey. In fact the future volumes ought to be prepared by, or under the authority of, the Survey.

The surpassing interest of the first annual report of the Zoological Survey of India lies in the great promise it holds out of considerable additions to the list of Asiatic animals of more than ordinary interest. How little is really known of the details of the Indian fauna may be gathered from one or two examples culled from the report:

"In a worm-eaten post on the shore of the Hughli we found the first specimen of a new and interesting genus of crabs, we obtained other new and interesting crustacea from the river itself. . . . Considering the fact that the Hughli has been explored by naturalists for the last fifty years, and that my

two immediate predecessors made a special study of its crustacea, nothing could better illustrate our present ignorance of even the better-studied elements in the fauna of even well-explored parts of India."

The progress of the survey of Lake Chilka has led to the discovery of a limbless snake-like lizard which was found burrowing in dry earth between the buttresses of a banyan tree.

The comparison between the bottom fauna of the Mutlah river and that of the deep sea is remarkable, and we confess, in spite of trawling experience in and around the Mutlah river both in the deltaic area and the open sea, that the similarity had escaped our attention.

The investigation of the Inlé Lake in the Southern Shan States is also rich in results. The discovery of an eel-like fish so peculiar "as to be regarded as the type of a new family" is in itself an indication of the extraordinary development of zoological science likely to result as the exploration of India becomes more thorough. Although much of the value of the work already accomplished is attributable to the enthusiasm of Dr. Annandale (with whose conclusion that the Director can only direct we heartily disagree), we are glad to record that he is supported by a capable if small staff, and it would be unfair not to recognise and record the efforts of Messrs. Kemp and Gravely and Dr. Chaudhuri.

MATERNITY AND CHILD WELFARE, by T. N. KELYNACK, M.D.: on **The Carnegie United Kingdom Trust Report on the Physical Welfare of Mothers and Children.** Vol. I. England and Wales. By E. W. HOPE, M.D., D.Sc., Medical Officer of Health for the City and Port of Liverpool, and Professor of Public Health, University of Liverpool (pp. xvi + 434). Vol. II. England and Wales. Part I. Midwives and Midwifery; Part II. Voluntary Work for Infant Welfare; Part III. Play Centres and Playgrounds. By JANET M. CAMPBELL, M.D., M.S., one of the Senior Medical Officers Board of England and Wales (pp. viii + 190). Vol. IV. Ireland. By E. COEY BIGGER, M.D., Medical Commissioner of the Local Government Board for Ireland and Crown Representative for Ireland on the General Medical Council (pp. xi + 213). Vol. III., dealing with Scotland, is being compiled by W. LESLIE MACKENZIE, M.D., LL.D., F.R.C.P.E., F.R.S.E., Medical Member of the Local Government Board for Scotland, but it is not yet published. The volumes are issued from the Headquarters of the Carnegie United Kingdom Trust, East Port, Dunfermline, Scotland.

WAR has convinced even the most non-progressive of the vital necessity for a scientifically directed preparation for parenthood, safeguarding of Maternity, and wise conservation of Child Life. The ultimate issue of the present gigantic struggle will depend mainly on the quality of the children which are being born and bred in these fateful years of conflict. The Child has for long been acclaimed the greatest asset of the nation, but it is only through the demands of service and by the roll of sacrifice that the real meaning of man-power and child-potentialities can be rightly appreciated and justly gauged. The destiny of our race lies in great measure in the keeping of the mothers of the coming citizens and future defenders of our Commonwealth.

Among patriotic purposes and national endeavours there is none wiser and

more fruitful than that which is devoting thought and action to the safeguarding of the future by the protection of Motherhood and Childhood in the present. While social reformers are discussing principles and practices for reconstruction and politicians are devising Bills to circumvent the working of natural laws, and idealists of every school are promulgating doctrines full of enticing words, the Carnegie Trust of the United Kingdom have quietly provided ways and means whereby it has been possible to procure reliable data and collect serviceable information which will go far to assist in the development of methods and measures for the furtherance of Maternity and Child Welfare. The volumes of the Reports on the Physical Welfare of Mothers and Children in the United Kingdom and Ireland issued by the Carnegie Trust form a monumental work which will be of permanent value and must assist greatly in the establishment of scientifically guided statutes and schemes and systems of supervision, instruction, and general welfare.

Mr. A. L. Hetherington, the secretary of the Trust, in the prefatory note to these volumes indicates that the Carnegie Trustees have provided for their issue because they are convinced that the existing risk of infant mortality and the need for a health supervision of children from infancy to the age of admission to school are among "the most important questions of the present day relating to the well-being of the people." Dr. E. W. Hope, in the introduction to the first volume of the series, explains that the work has grown out of the following resolution of instruction, adopted by the Carnegie Trustees early in 1916:

> "To investigate and report upon the existing provision for promoting the Physical Welfare of Mothers and Children, with special reference to the existing provision of Schools for Mothers, Health Houses, etc., the legislation that exists for governing the administration of such centres, and the extent to which municipal authorities have availed themselves of the powers they possess in the matter. Further, to suggest whether any, and if so what, steps might be taken by the Trustees to encourage the provision of such centres and under what conditions."

The work, so far as it has been completed, is a notable compliance with the terms of the reference. But the work, epoch making and marking though it may well be, is but the beginning, if we mistake not, of a far-reaching educational endeavour the benefits of which will be seen in the years of the coming generations. Sir Arthur Newsholme, Chief Medical Officer of the Local Government Board, in his suggestive introduction to the first volume declares, "To my mind the chief burden of the reports is the immediate need for further national effort to reduce sickness and mortality among infants before birth and in the first month after live birth." We congratulate the Carnegie Trustees on their statesmanlike action in arranging for the preparation and publication of these thought-stirring and effort-guiding volumes; and we also warmly congratulate all those who have participated in the production of so patriotic an experiment. The volumes should be studied in their entirety by all medical officers of health, school medical officers, responsible workers in connection with all forms of Maternity and Child Welfare Centres, as well as by those who are seeking to secure betterment for the future generations of Britishers. We trust means will be taken to place a set of the complete work in every library in the country, where it may be available for reference by all earnest seekers for better things.

It is difficult in the limited space allotted to this notice to give any adequate idea of the ground covered. We can only refer to some few of the more conspicuous features. Dr. E. W. Hope, in the first fifty-six pages of Vol. I.,

provides an admirable introduction to the essential features of the whole problem. It is a statesmanlike summary by one who has enjoyed unique opportunities and has proved himself a master in organisation and administration. Dr. Hope concludes: "a careful survey of the whole subject reveals many needs, some of which may be, or are being, dealt with by local authorities, local philanthropy, or both. It is, however, perfectly obvious that there remain some passing necessities for which there is as yet no adequate provision nor prospect of making it." Among the pressing necessities the following are enumerated: (i) The provision in suitable localities, and under appropriate conditions, of maternity homes for the benefit of outlying rural populations. (ii) The establishment of welfare centres to meet the recognised needs of pre-maternity and infancy, with which may be associated day nurseries. The need for provision for the better care of illegitimate infants calls for careful consideration. (iii) Provision for the improvement and better equipment of the means of education in the various branches of the science of public health, and for the encouragement of further research into the circumstances adversely affecting Infancy and Motherhood.

Dr. Hope supplies data and arguments, warnings and suggestions, and a careful study of his recommendations will go far to initiate enterprises and endeavours along sure lines. It is shown that had the annual wastage of male infant life during the last fifty years been no greater than it is at the present time, at least 500,000 more men would have been available to-day for the defence of the country. No less than 90,000 of the infants born in England and Wales annually do not survive the first year. It is interesting to find that the number of survivors per 100,000 of the population is higher, both actually and relatively, when the birth rates and infant mortality rates are both high, than when these figures are low; or in other words, under existing conditions it is the high birth rate, notwithstanding its accompanying waste, rather than the low birth rate and the greater saving associated with it, which dominates the increase of the population. Dr. Hope shows that much of the infantile mortality is due to the following factors: (i) The presence of some antenatal condition in the mother which requires treatment; (ii) the need for emergency medical assistance before, during, and after confinement; (iii) the presence of syphilis in the stillborn or in the parents; (iv) lack of skilled midwifery assistance; (v) the need for improvement and care in the feeding and hygienic management of infants.

Many will be glad to note that Dr. Hope is a convinced supporter of judicious co-operation between voluntary and official workers. The first volume contains a series of charts and diagrams which graphically portray some of the most conspicuous facts and features of infant mortality rates, distribution, and relationship to various areas. The co-ordination and relationship of special effort, voluntary and official, in promoting the welfare of mothers and little children are strikingly brought out by cleverly designed scheme-diagrams. A praiseworthy feature is the very full and effectively arranged abstract of the main legislative enactments in operation in England and Wales bearing on maternal and infant welfare and particularly on the protection of child life. Of special value at the present time are the condensed and skilfully arranged epitomes compiled from Reports furnished by Medical Officers of Health of various Counties and Boroughs of England and Wales. The information thus provided will be invaluable for reference and should go far to stimulate local authorities to undertake comprehensive and practical schemes.

Vol. II., prepared by Dr. Janet M. Campbell, contains much valuable material relative to Midwives and their Practice and the service which is being accomplished

by voluntary workers for Infant Welfare. The development of midwifery in this country is carefully traced and there is a reliable presentation of midwifery practice in England and Wales as existing at the present time. After a short section on Midwifery in other European countries Dr. Campbell sets forth a series of valuable suggestions for raising the standard of midwifery. A section is devoted to a consideration of midwifery in rural areas—a problem which urgently needs fullest consideration. There is also a study of Maternity Homes. We venture to reproduce Dr. Campbell's general summary:

"Three main conclusions emerge: *first*, that a large proportion of midwifery practice is, and is likely to remain, in the hands of midwives; *secondly*, that the midwives as a whole are not sufficiently well educated and trained to do justice to the heavy responsibilities which devolve upon them; and, *thirdly*, that the midwives are not properly distributed in urban and rural districts. This position is clearly unsatisfactory, and is inevitably prejudicial to the health and well-being of numerous mothers and infants. For many reasons it is neither desirable nor practicable to attempt to dispense with the services of midwives; thus the only alternative available would seem to be so to improve their technical capacity that they may become of true economic value to the State. For this it is necessary: (*a*) To attract well-educated women to the study and practice of midwifery; (*b*) to provide a training which is sufficiently prolonged, thorough, and comprehensive to fit them to carry out their subsequent work with due skill and judgment; and (*c*) with the assistance of Local Authorities to secure for them a position consistent with the dignity to which their profession should entitle them, and a wage commensurate with the exacting and arduous mode of life which they are called upon to follow. The end cannot be attained immediately, nor can it be secured without the co-operation of the midwives themselves, but it seems of vital importance that the claims of the midwives, and thus indirectly the needs of their patients, should not be forgotten or disregarded by those responsible for the initiation or administration of schemes for the promotion of infant and maternal welfare."

The section on Play Centres and Playgrounds deserves the study of all interested in the medico-educational study of Childhood. Vol. II. also contains a most instructive series of illustrations of day nurseries and other new forms of enterprise making for child betterment.

Vol. IV. is devoted to a consideration of maternal and child-welfare problems in Ireland and is issued under the name of Dr. E. Coey Bigger, Medical Commissioner of the Local Government Board of Ireland, and Crown Representative for Ireland on the General Medical Council. In the preparation of this Report Dr. Bigger has been assisted by his son, Dr. Joseph W. Bigger of Sheffield University, and by various inspectors and other officials of the Local Government Board in Ireland. Mrs. Dickie, LL.D., Insurance Commissioner, has dealt with matters relating to maternity benefit. Four medical women have also provided memoranda: Dr. Ella Webb on work in Dublin; Dr. Marion Andrews on enterprises in Belfast; Dr. Alice Barry on measures in Cork; there is also a short communication from Dr. Prudence E. Gaffikin. Ireland requires men, and yet of every 100 children born 9 die before they reach the age of twelve months. The death rate and damage rate are high. In 1915 95,583 infants were born in Ireland, and 8,753 died under one year of age—that is, at the rate of 92 deaths per 1,000 infants born alive. In England in the same year the rate was 110, and in Scotland 126. Since the beginning of the century England's rate has been rapidly and steadily declining, while Ireland's rate in the same period has also declined, but to a less degree than that of England. The reduction in the rate for England has been 24 per cent. Ireland's reduction has been just one-third of

this. In Ireland a child just born has about the same chance of surviving his first year as an old man of seventy-seven years of age has of living till his seventy-eighth birthday. The deaths of infants under one year form about one-eighth of the total deaths of the country. Of every 1,000 children born, almost as many die in the first year as in the succeeding fourteen years. In 1915 576 mothers died in Ireland from conditions caused by or associated with pregnancy or child-birth. The death rate from puerperal septic disease was 2·12 per 1,000 births. In discussing factors in the causation of deaths of infants and children the Report enumerates the following: (1) The care of the mother before, during, and after labour, and the advice given as to the care of the child; (2) the economic conditions of the family; (3) the domestic surroundings of infant and child; (4) the extra-domestic surroundings of the home; (5) the health and habits of the mother and father; (6) the affection of the mother for the child: her education and fitness for motherhood; (7) legitimacy or illegitimacy of offspring: (8) the size of the family; (9) the ages of mother and father at marriage; (10) the supervision of milk and food supplies. No Midwives Act applies in Ireland, and there is no Central Midwives Board. The greater part of the people of Ireland are engaged in agriculture, and as the weekly wage of an agriculture labourer was about 11*s.* to 15*s.* before the war, it is easy to understand that the majority of mothers are habitually under-fed, and many of the children actually on the verge of starvation. The economic factor is clearly an important one in the case of agricultural labourers. The mother often works almost to the day of her confinement; she is badly fed, she cannot provide the necessary outfit, and being ill-nourished cannot continue to suckle her child. Moreover, she cannot provide necessary food or adequate clothing for the child. Although something has been accomplished to improve domestic conditions in country districts, mainly through the administration of the Labourers (Ireland) Acts, much still remains to be done. The conditions in the towns and cities are very bad. Nearly a third of the total population of the city of Dublin live in one-room tenements. Under the Housing of the Working Classes Acts about 9,130 houses or tenements have been erected in Irish towns by local authorities at an expenditure of, approximately, one and a half million sterling. Alcohol exerts a baneful influence in many centres. There is much mother-love in Ireland, but ignorance still keeps the Irish woman in thraldom.

"All that she knows about her functions, childbearing, and child nurture is what she has learned from her mother; it has been handed down from generation to generation, and is a blend of good and bad, a mingling of useful knowledge and harmful superstition. Though she went to the national school she is taught nothing that would fit her for her work of bringing into the world and rearing a strong and healthy family of sons and daughters. She knows, however, just one thing, and that perhaps the most valuable of all—that she was intended to suckle her children herself, and doing this, she saves her children from the many dangers surrounding the use of the bottle. The practice of breast-feeding is almost universal among the poorer mothers in the country, and is still very common in town."

In Ireland in 1915 there were 3·1 per cent. of illegitimate births, varying from 8 per cent. in Connaught to 4 per cent. in Ulster; and the mortality among the illegitimate infants was more than two and a half times that of the legitimate. Although the birth rate is low in Ireland, the marriage rate is also very low. The average number of children born alive per family is 4·09, while the corresponding figures are in England and Wales 3·55, and in Scotland 4·01. The number of

births in Ireland in 1904 was 103,811, and in 1914 98,806. In 1915 the number of births was over 3,000 less than in 1914. In 1915 6·81 per cent. of the women married were not of full age, and 1·95 per cent. of the men were also under age. The Report provides much valuable information regarding legislative measures and numerous voluntary enterprises which have tended directly or indirectly to maternal and child welfare. Particulars are provided of existing maternal and child-welfare schemes, and numerous recommendations are made which all true friends of Ireland would wish to see carried out.

We hope the volume relating to Scotland will be issued at an early date, and that we may have the opportunity of reviewing it in this journal.

The Carnegie Trust have taken the first step in the establishment of a truly national effort to provide scientific data, which should form a sure foundation on which future maternity and child-welfare work may be surely based. We trust it may be found possible for the Trust to establish one or more central Bureaux, which will serve as centres for all parts of the United Kingdom from whence information and guidance may flow. We should like to see a representative Consultative and Advisory Committee established in connection with each centre, and if efficiently directed these "brain centres" should render invaluable service in securing co-ordination of work and co-operation of workers for the welfare of mothers and the well-being of children of all ages and all ranks of the British Commonwealth.

REVIEWS

MATHEMATICS

Elementary Mathematical Analysis. By JOHN WESLEY YOUNG, Professor of Mathematics, Dartmouth College, and FRANK MILLETT MORGAN, Assistant Professor of Mathematics, Dartmouth College. [Pp. xiv + 548.] (New York: The Macmillan Company; London: Macmillan & Co., Ltd., 1917. Price 11*s*. net.)

THE purpose of this book is to present a course suitable for students in the first year at American colleges, universities, and technical schools. "It presupposes on the part of the student only the usual minimum entrance requirements in elementary algebra and plane geometry" (p. v). The most interesting feature is that more emphasis is "placed on insight and understanding of fundamental conceptions and modes of thought, and less on algebraic technique and facility of manipulation" (p. v). A good example of this will be found on p. 259; after some instructions are given about the working of the slide-rule, we read: "*These rules are not to be memorised.* They will be used almost instinctively by one who has made the reason for each rule thoroughly clear to himself and who is in practice." "The concept of *functionality* and the mathematical processes developed for the study of functions are precisely the things in mathematics that have most effectively contributed to human progress in more modern times; and the thinking stimulated by this concept and these processes is fundamentally similar to the thought which we are continually applying to our daily problems" (p. viii). Thus: "This course in mathematics is primarily concerned with the study of certain of the simpler kinds of functions and their applications" (p. 2). The conception of function is introduced at the very beginning and treated with a great wealth of illustration; then certain elementary functions are considered, such as simple algebraic, trigonometric, and logarithmic functions; then there is a chapter on numerical computation. The third Part is on applications to geometry and constitutes a fairly complete elementary course of plane analytical geometry. The fourth Part is on miscellaneous algebraic methods, and includes much of what we in Great Britain understand by "higher algebra" and the "theory of equations." The fifth Part is on functions of two variables and solid analytic geometry. The book ends with several useful tables.

A feature of the method of exposition is the continual interruption of the text by the word "Why?" (see, for example, p. 61), and this is not unlikely to stimulate inquiry. The successive treatment of "continuous" functions from a very intuitional basis (pp. 19, 102, 451) seems quite good, and the treatment of logarithms (pp. 220–35) seems excellent with the exception of the mention of the number *e* on p. 224. It would possibly have been an improvement to have introduced on p. 34 a simple proof that the measure of the diagonal of a unit square is incommensurable. This is an excellent working text-book of the same nature as Mr. Hardy's book, but far more elementary.

PHILIP E. B. JOURDAIN.

A First Course in Higher Algebra. By HELEN A. MERRILL, Ph.D., Professor of Mathematics in Wellesley College, and CLARA E. SMITH, Ph.D., Associate Professor of Mathematics in Wellesley College. [Pp. xvi + 247.] (New York: The Macmillan Company; London: Macmillan & Co., Ltd., 1917.)

THE novelty in this book is the fact that it is "an outgrowth of the conviction of the authors that Higher Algebra, to be worthy of the name, must employ advanced methods, and that the method which chiefly marks advanced work in analysis is that of limits." Thus, besides the usual chapters on the algebra of schools, we have a chapter on the elementary theory of integration as well as a chapter on the differentiation of algebraic functions. The authors state that the proofs are made "as rigorous as seems advisable for immature students," and that it is their hope that there is "nothing to be unlearned in later work" (p. vii). But surely we are all supposed to know now that logically the theory of irrational numbers precedes the theory of convergence, and that the theory of irrationals is used implicitly in the theorem of comparison of series (p. 103). Also I think that the student would have to unlearn the ideas of Kronecker (pp. 1, 181) on the indefinability of the integers. The views of the authors that zero, negative numbers, and fractions are "symbols" (pp 2, 3, 6) seem to be not wholly unrelated with Kronecker's views; but complex numbers, constants, and variables are "quantities" (pp. 50, 169). The contrast between "it has been proved" and "it is true" on p. 154 does not seem to be quite wise, and there are analogous difficulties in the note on p. 115 and on p. 126. The introduction of determinants in the third chapter seems to be good, and so do certain of the remarks on the calculation of logarithms (pp. 150-52) and the beginnings of the theory of functions of a complex variable (pp. 182-4). At least these things are interesting, and that seems to be the main thing in education. There are good and interesting examples on pp. 87-90.

PHILIP E. B. JOURDAIN.

Finite Collineation Groups: with an Introduction to the Theory of Groups of Operators and Substitution Groups. By H. F. BLICHFELDT, Professor of Mathematics in Leland Stanford Junior University. [Pp. xii + 194.] (Chicago: University of Chicago Press; London: Cambridge University Press. Price $1.50 or 6s. net.)

THE University of Chicago Science Series owes its origin to a feeling that there should be a medium of publication occupying a position between the technical journals with their short articles and the elaborate treatises which attempt to cover several or all aspects of a wide field. Thus, since the theory of finite collineation groups (or linear groups) is at present to be found mainly in scattered articles, in addition to a few books on group theory, Prof. Blichfeldt has given in the present volume an outline of the different principles contained in these publications, and has at the same time made an effort to depend upon a minimum of abstract group theory. "In this and in many other respects the present volume differs from Part II. of" the *Theory and Applications of Finite Groups*, written by Profs. Miller, Blichfeldt, and Dickson, and published in 1916; "in particular, the present volume contains more of the theory of linear groups" (p. vii). The theory of linear groups of finite order may be said to have been originated by Klein in 1876, and subsequently Klein extended the Galois theory of algebraic equations by the introduction of linear groups. At about the same time the solutions of Schwartz, Fuchs, and Jordan of a problem connected with linear differential equations were published: these solutions hinge upon the discovery of

the invariants of certain corresponding linear groups or the groups themselves (p. 174). The theory has since that time been greatly developed in scattered memoirs. Thus, the present exceedingly able exposition will be of great value. The eight chapters deal with elementary properties of linear groups, groups of operators, and substitution groups, the linear groups in two variables, advanced theory of linear groups, the linear groups in three variables, the theory of group characteristics, the linear groups in four variables, and the history and applications of linear groups. There is an appendix and indexes.

PHILIP E. B. JOURDAIN.

Lectures on Ten British Mathematicians of the Nineteenth Century. By ALEXANDER MACFARLANE, Late President of the International Association for Promoting the Study of Quaternions. (Pp. 148.) (New York: John Wiley & Sons; London: Chapman & Hall, Ltd., 1916. Price 5*s*. 6*d*. net.)

THIS volume is No. 17 of the "Mathematical Monographs," edited by Mansfield Merriman and Robert S. Woodward. During the years 1901–4 the late Dr. Macfarlane delivered at Lehigh University some lectures on twenty-five British mathematicians of the nineteenth century, and in this book ten lectures on ten pure mathematicians are given in essentially the same form as delivered. "In a future volume it is hoped to issue lectures on ten mathematicians whose main work was in physics and astronomy" (p. 3). A short biography and portrait of Macfarlane (1851–1913) are given (p. 4), and the volume is adorned by a page of portraits of the ten mathematicians mentioned in the excellent and fairly popular lectures. The mathematicians are George Peacock (1791–1858), Augustus De Morgan (1806–1871), Sir William Rowan Hamilton (1805–1865), George Boole (1815–1864), Arthur Cayley (1821–1895), William Kingdon Clifford (1845–1879), Henry John Stephen Smith (1826–1883), James Joseph Sylvester (1814–1897), Thomas Penyngton Kirkman (1806–1895), and Isaac Todhunter (1820–1884).

The work of many of the British mathematicians of the early half of the nineteenth century forms in a sense an almost complete whole: the work of Peacock, Gregory, Hamilton, Boole, De Morgan, and a few others, on the foundations of algebra, symbolic logic, and generalisations of algebra form a closely connected chapter in science in which foreign influences are not very perceptible. It was lucky that Macfarlane, whose interest largely lay in the directions just mentioned, should have given these excellent lectures. It may be noticed that the works of Lagrange and Laplace were of direct influence in the beginning of the work of Hamilton, Boole, and Cayley (p. 65). The foundations of algebra and the calculus of logic are treated, and some critical remarks are made, on pp. 14–18, 24–30, 41–42, 54–63; and these passages will be found exceedingly useful contributions to the history of mathematics. It is perhaps rather disappointing that neither the work of Jevons nor Hankel is mentioned, while surely the names of Gauss and Weierstrass might have been mentioned on p. 29. It is a mistake to attribute to Kant the idea of Hamilton of algebra as the science of pure time (pp. 41, 85). Surely some mention of Woodhouse should have been made on p. 10 (cf. p. 136). The remark about Newton on p. 9 seems to be an error, and De Morgan's attitude can hardly be described as "morbid" (p. 24). The remark about Boole and Cambridge on pp. 51–2 is not quite accurate, and Hamilton's biographer is wrongly stated to be Charles Graves on pp. 34 and 54: it was Robert Perceval Graves. There is a misprint for "rigorousness" on p. 95. The extract from Todhunter on p. 140 is perfectly delicious.

PHILIP E. B. JOURDAIN.

ASTRONOMY

Celestial Objects for Common Telescopes. By REV. T. W. WEBB, M.A. Sixth Edition, revised by Rev. T. E. ESPIN, M.A. In two volumes. [Pp. xx + 253 and vii + 320, with numerous illustrations.] (London: Longmans, Green & Co. Price 7*s.* 6*d.* net each volume.)

THE volumes under review are a new and revised edition of the well-known work by the late Rev. T. W. Webb, first published in one volume nearly sixty years ago. The present revision has been undertaken by that well-known amateur double-star observer, Rev. T. E. Espin, who has been assisted in the portions dealing with lunar and planetary observations by some of the foremost amateur observers. Thus, for instance, Mr. W. Goodacre has supplied some excellent notes on lunar details and also a new and accurate map of the moon, while Mr. W. S. Franks has contributed an interesting and useful note on the double-star micrometer. Mr. Espin is himself responsible for the revision of the double-star portion.

The addition of new matter in successive editions has resulted in the size of the book considerably exceeding that of Mr. Webb's original work, so that it almost seems as though the original intention—"to furnish the possessors of ordinary telescopes with plain directions for their use, and a list of objects for their advantageous employment"—has been lost sight of. He desired to point the way to the "inexperienced possessor" of a telescope to the chief objects of interest in the heavens. The addition of a large mass of detailed matter, much of which is not within the range of "common telescopes," rather detracts from this aim, and although it undoubtedly increases the value of the work to the experienced observer, for whom it was not primarily intended, it detracts from its value to the beginner, for whom it was intended.

It is a difficult matter to graft a large amount of new matter on to an old work without at the same time practically rewriting the whole, if a homogeneous result is to be obtained. The present revision suffers rather badly from this patchwork effect, in some instances Webb's original words being mixed up with added matter with, at times, strange results. There are several instances also where the reviser might with advantage have cut out some of the old matter in favour of more up-to-date information; for instance, Secchi's classification of stellar types has been almost entirely superseded by the more recent and much more convenient Harvard classification, yet the latter is scarcely mentioned. There still remain one or two remarkable omissions in the work; thus there is no branch of astronomy in which amateur observers can more readily obtain results of real value, even with very small instruments, than in variable star observations. Yet there are no hints at all as to the method of making such observations and the precautions to be taken. Surely the book would have benefited by the inclusion of such information at the expense of some of the more detailed matter.

The reviser is perhaps not altogether to blame for the above defects, as the general lines along which the revision was to be carried out may have been laid down for him. We can, however, hardly excuse him for the rather numerous typographical errors which have remained undetected. Some of these are of a serious nature, as 70° 15′ instead of 7° 15′ for the value of the inclination of the solar equator.

Yet in spite of all that we have said, the fact remains that the book is the best of its kind, and should be in the hands of every amateur observer. Its value has been considerably increased by the present revision, which must have occupied Mr. Espin for a considerable time and have given him a large amount of work.

Astronomers, particularly amateurs, will be grateful to him for having undertaken what was not an easy task, and for having, on the whole, achieved success.

H. S. J.

Astronomical Consequences of the Electrical Theory of Matter. By Sir Oliver Lodge. (From the *Philosophical Magazine*, Vol. XXXIV. August 1917.)

In the year 1859 Leverrier, from a discussion of all the observed transits of Mercury, found that the apsidal line of its orbit has a movement, which cannot be accounted for by the action of the known planets. It amounts to 43″ in a century. This has long been a difficulty between gravitational theory and observation. On the electrical theory of matter, a body in motion at a high speed through the æther has an extra inertia, not dependent upon its mass, but on the æther, and hence not subject to gravity. From this extra inertia due to motion through the æther there results virtually mass without weight But an increased inertia means a diminished acceleration, just as if the gravitational pull on a planet were diminished. It follows that the acceleration acting on a planet will not only be less than in the Newtonian law of variation as the inverse square of the distance, but will vary in different parts of the planet's orbit round the Sun. This must give rise to a perturbation of its orbit.

Starting with this ascertained fact, Sir Oliver Lodge, in the paper under review, discusses mathematically the effect of this extra and varying mass upon a planet's orbit. He shows that it introduces a revolution of the orbit in its own plane, which is equivalent to a progression of its apsidal line. According to modern determinations the line of the Sun's way is directed towards an apex near Vega, and the velocity of the Sun is about 12 miles a second This is tantamount to regarding the star group of which the Sun is a member, and relatively to which the direction and velocity of the Sun's way has been determined, as fixed, in which case the apse of Mercury would, according to Sir Oliver Lodge, progress 2·4″ in a century.

But if the whole system, or group of stars of which the Sun is but one member, was advancing through the stagnant æther, as the background of reference of motion, towards longitude 294°, with a velocity of about 56 miles a second, then the revolution of the orbit of Mercury would square with observation. Such a motion would also reconcile with observation the motion of the apsidal line of Mars. Sir Oliver Lodge also asserts that, according to this theory, there would be nothing excessive in the apsidal progressions of the Earth and Venus. Prof. Eddington, however, in the September number of the *Philosophical Magazine*, demurs, and shows that this supposed value of the solar motion relatively to the æther would introduce corrections into the observed elements of the orbits of the Earth and Venus, which are quite inadmissible.

Einstein's Theory of Relativity equally explains this anomaly in the motion of the perihelion of Mercury, without introducing any anomalies into those of the Earth and Venus. This has considerably cheered the upholders of a mathematical conception, which entirely transcends, and is independent of, any preconceived physical notions. In fact, according to one of its protagonists, De Sitter, no one has ever measured a pure distance, or a pure interval of time. However this may be, a straightforward physical theory, as that advanced by Sir Oliver Lodge, to account for the one great puzzle of gravitational astronomy, may be preferred by many to a mathematical hypothesis, which demands that we should abandon the whole of our previous conceptions about space and time. At any rate, it is a

notable advance of the theory of the Sun's motion in space, which, like that first established by Sir William Herschel in 1783, may lead after correction to a still further development in our knowledge of the mechanism of the Universe.

A. L. C.

PHYSICS

Laws of Physical Science. By EDWIN F. NORTHRUP, Ph.D. [Pp. vii + 210.] (Philadelphia and London: J. B. Lippincott, 1917. Price 8*s*. 6*d*. net.)

IN the preface to this book it is pointed out that, while the data and mathematics used in physical science have been classified and are readily accessible in many reference-books, there is no handbook which summarises the general propositions and important facts of the science. This gap the book is intended to fill, though since it seems to have been written primarily for the guidance and "inspiration" of students in other branches of Natural Science, the subject-matter is almost entirely of an elementary character. It contains little that would not be discussed in any standard college text-book. The laws are dealt with under the usual sub-headings including mechanics, and each is followed by one or more references, generally to familiar text-books, the author rightly considering that the treatment given there is more likely to appeal to those not fully conversant with the subject than that in the original papers. He has, however, made a conspicuous number of exceptions to this excellent plan in favour of his own papers—even in one case referring to results he has not yet published elsewhere!

The arrangement of the subject-matter in the section devoted to Heat and Physical Chemistry is extraordinary, not to say careless; for example, Carnot's theorem is given before either law of thermodynamics and is separated from them by facts concerning the platinum resistance thermometer, the expansion of liquids and Dulong and Petit's law. Van der Waal's equation is given twice on consecutive pages, and the equation attributed to Clapeyron is not that usually so known. The other sections of the book are less open to criticism, and there is a satisfactory index. There are, however, a good many typographical errors, and the letter l and the numeric 1 are confused in several places (*e.g* pp. 13, 37, 41 and 42). These adverse comments, however, refer in the most part to minor points, and the book may possibly be found a useful compendium by students in other fields whose work brings them only into occasional contact with this branch of science. It would also serve as a useful summary of facts for a prospective intermediate or pass examinee.

D. O. W.

CHEMISTRY

The Theory and Use of Indicators. An account of the chemical equilibria of acids, alkalies, and indicators in aqueous solution, with applications. By E. B. R. PRIDEAUX, M.A., D.Sc. [Pp. vii + 375, with diagrams and tables.] (London: Constable & Co., Ltd., 1917. Price 12*s*. 6*d*. net.)

IN view of the fundamental mportance of indicators from the standpoint of theoretical and, more especially, practical chemistry, it is somewhat surprising that English chemists should have had to wait so long for the appearance of a reliable book on the subject: doubtless this has been due to our dependence on German chemical literature, which latter formed in its way a "key-industry" as regards chemical activities, and for that reason, if for no other, one would welcome the appearance of Dr. Prideaux's work, as being a further step in freeing us from our bondage.

The book deals in a comprehensive fashion with the theory of equilibria in solution, colorimetry, theories of colour in their relation to the ionic theory, chemical constitution and salt formation, the determination and use of indicator constants, the theory of neutralisation and titration, and treats in a thorough-going manner with the practical application of indicators, and last but not least, Chapter VIII. gives a summarised list of the properties and absorption-spectra of the principle indicators.

The work bears evidence of careful compilation, and should gain for itself a recognised place in the literature of the subject discussed.

It should be studied by every one interested in the subject of indicators, and, moreover, the numerous and well-printed tables and diagrams render it a most useful reference book for the analytical laboratory.

F. A. M.

A Text-book of Inorganic Chemistry. By A. F. HOLLEMAN, Ph.D., LL.D., F.R.A. Amst., Professor Ordinarius in the University of Amsterdam, etc. Issued in English in co-operation with Hermon Charles Cooper. Fifth English edition, completely revised. [Pp. viii + 521, with coloured table of spectra, and diagrams.] (London: Chapman & Hall, Ltd., 1916. Price 10*s*. 6*d*. net.)

PROF. HOLLEMAN'S text-books are, perhaps, better known and appreciated abroad than in this country, but nevertheless they are so familiar that the issue of a new English edition of his *Text-book of Inorganic Chemistry* calls for little comment, save to congratulate the author and the translator on their continued success.

Very many descriptive portions have been re-written, whilst the development in the direction of physics and physical chemistry such as atomic weights and atomic structure, gaseous equilibrium and so on, furnish new material. Although the translator hails from America, we are glad to note that "sulphur" continues to remain itself, and has not been converted into "sulfur"—a contraction which grates somewhat on the ears of British readers; nor do we have to suffer from "thru" and other Rooseveltian innovations; on the other hand the spelling of "aluminium" has been abandoned for "aluminum," which may or may not be desirable, but looks curious to unaccustomed eyes.

As regards new matter one may note that the Haber process is discussed on page 173, but would be all the better for a diagram of the plant used in the process, which, after all, represents one of the greatest advances in applied chemistry, whilst it is somewhat surprising that the processes of Ostwald, or Frank-Caro—by which many hundred thousand tons of nitric acid are made each year in Germany by the oxidation of ammonia—are dismissed in a three-line paragraph at the bottom of page 190, and, in fact, we are told on page 175 that, in the combustion of ammonia, only traces of ammonium nitrite and nitrogen dioxide are formed, no mention being made of the influence of catalysts nor of Ostwald's interesting theory on the subject. We venture to think that by the time a new edition of this book is issued it will be necessary to replace fig. 36 on page 188 (the production of nitric acid) by a diagram of the ammonia-nitric acid contact plant.

The statement also on page 191 that the Chile nitrate fields "bid fair to be exhausted in ten years" is hardly in accordance with the most recent statistics, according to which they have a century or more before them; if they cease to be worked it will be due to the competition of synthetic nitric acid and not to the exhaustion of the fields themselves. Under Iron again one looks in vain for the

latest productions, such as "non-rusting" chrome-steel, or the resistent silicon-iron alloys which play such a large part in modern chemical industry. These, however, are relatively minor points, and the volume as a whole maintains the position the previous editions have won amongst text-books of Inorganic Chemistry.

F. A. M.

Introduction to the Rarer Elements. By PHILIP E. BROWNING, Ph.D., Assistant Professor of Chemistry, Kent Chemical Laboratory, Yale University. [Pp. xii + 250, with tables and diagrams. Fourth edition.] (London: Chapman & Hall, Ltd., 1917. Price 7*s.* net.)

THE fact that Prof. Browning's book has already reached a fourth edition shows on the one hand that the work continues to be in demand and on the other that interest in the rarer elements is in no way diminishing.

As some fifty elements are mentioned, including such diverse substances as the radio-active elements, the rare earths, gallium, vanadium, tungsten, selenium, gold, and the inert gases of the atmosphere, it will be realised that it is no light task to discuss all these within the pages of a relatively small book.

Of necessity, therefore, one finds that some points have been enlarged upon at the expense of others, and that one cannot look upon Prof. Browning's book as a substitute for the more comprehensive works devoted to special groups of elements. As an introduction, however, the work is of distinct value, and as, moreover, it has been prepared from the material used by the author in a course of lectures for students at Yale, it possesses distinct educational interest. The uses to which the rare elements are put increase from day to day, and it is very desirable that senior students should gain an insight into the properties and uses of those elements not generally treated in much detail in the usual text-books. In this connection Chapter XII. on some technical applications, is very informing.

It would, of course, be comparatively easy to criticise certain aspects of the work, such as the absence of quantitative experiments in the practical work on analysis, or the almost excessive condensation of facts and figures which is apparent here and there; but, as the book undoubtedly fulfils its avowed object of serving "as a convenient handbook in the introductory study of the rarer elements," it would be unfair to exaggerate what are, after all, quite minor defects in a really useful book.

F. A. M.

Our Analytical Chemistry and its Future. By WILLIAM FRANCIS HILLEBRAND, Ph.D., Chief Chemist of the Bureau of Standards, Washington, D.C. [Pp. 36.] (New York: Columbia University Press; London: Oxford University Press, 1917. Price 1*s.* 6*d.* net.)

THIS pamphlet contains the Chandler Lecture for 1916 delivered at Columbia University and, both by reason of the interest and importance of the subject and the weight which Dr. Hillebrand's words necessarily carry, it deserves to be read with care by those who are concerned in the future welfare of chemistry, equally in this country as in the United States.

Whilst one cannot agree with Dr. Hillebrand's criticisms in their entirety, most chemists will consider the views and aims set forth as worthy of serious consideration. The author points out the necessity for greatly increased accuracy in analytical determinations: methods which were considered suitable even quite a few years back are to-day no longer of sufficient accuracy. One has only to

recall the extraordinary influence exerted on many chemical reactions by most minute traces of impurities to realise that analyses accurate to, say, 99·9 per cent. may be quite inadequate if, as is often the case, the remaining 0·1 per cent. be some catalyst capable of upsetting completely the course of a reaction; it becomes increasingly necessary that analytical methods should be refined day by day, and it is probable that in many cases the cult of quick and only moderately accurate "routine" analyses may have to give way to more tedious but more accurate methods as the operations of chemical technology become increasingly complex and delicate.

Dr. Hillebrand also discusses the question of standardised reagents, and calls attention to the well-known abuse of "purifying" reagents by the simple process of transferring them to new bottles bearing the mystic words "Puriss" or the like!

A considerable portion of the lecture is devoted to a discussion of means for providing new standardised methods of analysis, and generally encouraging the development of analytical chemistry by the creation of a new chemical institution in the United States which, working in conjunction with the Bureau of Standards, would assist in developing this branch of chemistry, and enable it to take its proper position in national affairs

It is not, of course, possible here to show the workings of the scheme in detail, but the whole lecture is full of interesting points, and will well repay careful study.

F. A. M.

The Chemical Constitution of the Proteins. Part I. Analysis. By R. H. A. PLIMMER, D.Sc. (Monographs in Biochemistry.) [Pp. xii + 174.] (London: Longmans, Green & Co., 1917. Price 6*s*. net.)

THE earlier editions of this monograph are too well known to those interested in biochemistry to require any detailed review for the present one; suffice it to say that the new edition brings the subject of protein analysis up to date. As new methods of analysis are being continually produced, whereas the descriptions of the methods for determining the constitution and synthesising amino acids are not liable to much change, the author has decided in this edition to divide the subject matter of Part I. of the old monograph into two: Part I. Analysis, and Part II. The Aminoacids. The present volume therefore deals with analysis only, and contains all the important additions to the subject contributed since the issue of the last edition in 1912. By this arrangement it should only be necessary to reissue Part I. at intervals in order to keep the monograph up-to-date, since the subject-matter of the remaining parts does not, for obvious reasons, require such frequent revision.

P. H.

A Technical Handbook of Oils, Fats, and Waxes. By PERCIVAL J. FRYER, F.C.S., and FRANK E. WESTON, B.Sc., F.C.S. Vol. I. Chemical and General. [Pp. x + 279, with 33 illustrations and 36 plates.] (Cambridge: at the University Press, 1917. Price 9*s*. net.)

THIS volume forms one of the excellent Cambridge Technical Series published by the University Press, and appears to be well up to the standard of the volumes already issued.

It has been designed primarily to meet the needs of the technical worker, the works chemist, and others less directly concerned in the technology of the oils, fats, and waxes.

The authors have endeavoured to effect, probably for the first time, a survey of the whole subject of oils, fats, and waxes in a single treatise. Those familiar with the complete but necessarily bulky works of Lewkowitsch and others will welcome the attempt made by the authors to summarise the present state of knowledge in the industry within a few hundred pages.

Section I. gives a general introduction to the subject, Section II. discusses the chemistry of the oils, fats, and waxes, Section III. deals with their testing and analysis, Section IV. gives the classification on a basis of glyceroids and non-glyceroids, Section V. discusses the production and refinement of oils, fats, and waxes, and Section VI. mentions shortly the oleo-resins and essential oils.

A large number of excellent diagrams are given at the end of the volume showing the latest form of apparatus and machinery used in manufactures, and in addition an endeavour has been made, by the preparation of a number of coloured tables, towards assisting in the rapid identification of unknown oils and fats. The printing and general arrangement of the book are good, and the volume, with the forthcoming second volume on the practical analytical work, should make it a very useful addition to the chemist's library.

F. A. M.

The Method of Enzyme Action. By JAMES BEATTY, M.A., M.D., D.P.H., with an Introduction by Prof. E. H. Starling, M.D., Sc.D., F.R.S. [Pp. vi + 143.] (London : J. & A. Churchill, 1917. Price 5*s.* net.)

THE author formulates the hypothesis that all enzyme action, whether of hydrolysis, synthesis, oxidation, or reduction, can ultimately be traced to the same causes—namely, the addition or removal of hydrogen or hydroxyl radicles in the hydrolysis or synthesis and the replacement of H by OH or – OH by H in oxidation or reduction respectively. All these processes can take place in the absence of catalysts, but as life developed and increased in complexity the necessity for catalysts, such as the enzymes, arose in order to increase the speed of reactions and also to limit their sphere of action, since a reaction which at one place would be beneficial might at another be injurious. This limitation of sphere of action is secured by the colloidal nature of enzymes whereby reactions are brought about as the result of surface adsorption. The elaboration of this hypothesis is preceded by a number of interesting sections devoted to the subjects of catalysis, colloids, adsorption, and the properties of enzymes, each section being followed by a useful summary. Each one of these subjects is dealt with in a most lucid and instructive manner, and the book is worth reading for these sections alone if for no other.

The ultimate proof of an hypothesis is, according to the author's own words, "the demonstration of the deductions made from it, and if the hypothesis . . . is a correct explanation of enzyme action it ought to be possible to manufacture enzymes"; and in support of this statement he points to the experiments of Dony Herault and others who have succeeded in producing artificial oxidases.

A special interest attaches to the fact that the book was written in the intervals of active service at Salonica, and in view of these circumstances the very greatest credit is due to the author for producing what must undoubtedly be regarded as an able and suggestive monograph which deserves to receive the serious attention of those engaged in work on enzymes, and may be thoroughly recommended to all who wish to have a clear and concise account of the known properties of these substances.

P. H.

Artificial Dyestuffs: Their Nature, Manufacture, and Uses. By ALBERT R. J. RAMSEY and H. CLAUDE WESTON. Illustrated by the Authors. [Pp. vi + 212.] (London: G. Routledge & Sons, Ltd., 1917. Price 3*s*. 6*d*. net.)

ACCORDING to the foreword this book "aims at providing the foundations for a knowledge of the highly important branch of Industrial Chemistry with which it deals. Whilst not claiming to cover the ground exhaustively, it affords a most useful survey of the whole subject in language as little technical as possible."

Meritorious as are the objects thus set forth, it must be admitted that the performance falls far short of the aim. Exactly how it may have occurred is not our affair, but, whether through carelessness in proof-reading or ill-advised haste in preparing the volume, it shows so many and so serious errors of commission and of omission that it defeats its own ends.

The general plan is fairly reasonable; thus, Chapter I. gives a general introduction, Chapter II. deals with the distillation of coal and coal-tar, in Chapters III. to VIII. the various classes of dyes are discussed—though the arrangement is not very satisfactory—and Chapter IX. shows the application of the Artificial Dyestuffs.

(By the way is it really necessary when discussing dyes and colouring matters to adopt invariably the very crude translation of the German "Farbstoff"—namely, dyestuffs? One does not refer to explosives as "Springstuffs" nor to hydrogen as "Waterstuff"; why not use plain English and term them dyes and colours? The authors of the volume under discussion are not, however, alone in this unfortunate piece of phraseology.)

The diagrams of apparatus are fairly clear and well-drawn, and if some of them show signs of similarity to the diagrams in Ullmann's *Enzyklopædie*, that is no drawback. When, however, one comes to examine the letterpress, formulæ, and general diagrams in detail, one is amazed at the variety and extent of the errors and omissions. Thus, in the diagrams on page 98 we are solemnly assured that phthalimide is prepared from phthalic anhydride by treatment with strong *nitric acid!* Again, on the next page we are told that the halogen indigos dye reddish shades; with the exception of *Indigo Rathjen*, these particular dyes yield actually more greenish shades than indigo.

As examples of misprints we may quote the word "soda-salt" instead of "silver-salt" on page 76, whilst at the bottom of the same page we are told that in the production of alizarin, at one stage "the supernatant liquor is run off and washed"; perhaps the authors may understand what this means, but it conveys nothing to any one else.

It would occupy too much space to give all the obvious errors which can be noted on reading the work. The formulae are strangely inconsistent and often incorrect, as on p. 82 (Alizarin Cyanin R.); and on the same page we are told that fuming sulphuric acid contains "sulphurous acid, SO_3," whilst no explanation is given for the change from the ordinary formula for anthraquinone on p. 28 to the centric formula on p. 71.

The formula for Bismarck-brown, as given on p. 63, would cause the Iron Chancellor to turn in his grave. It will be of interest to dye-manufacturers to learn (p. 49) that "salicylic acid . . . is a substance manufactured from anthranilic acid!" Shades of Kolbe! The choice of dyes is curious; the chapter on sulphur dyes gives no mention of sulphur-black, although this colour composes considerably more than half or two-thirds of the total output of sulphur colours,

whilst the chapter on anthracene dyes contains no mention of the extremely important Algol vat-dyes.

One cannot honestly congratulate either the authors or the printers on the production. It is unfortunate that such a carelessly constructed book should have been issued at the present juncture, particularly when supplies of paper are short.

F. A. M.

BOTANY

Cotton and Other Vegetable Fibres: Their Production and Utilisation. By ERNEST GOULDING, D.Sc. (Lond.), F.I.C., Scientific and Technical Department, Imperial Institute. Preface by WYNDHAM R. DUNSTAN, C.M.G., LL.D., F.R.S., Director of the Imperial Institute. (Imperial Institute Handbooks.) [Pp. x + 231, with illustrations.] (London: John Murray, 1917. Price 6*s*. net.)

IF this volume is to be accepted as a measure of our achievement in studying the raw material of a group of plant industries, it is rather depressing.

It purports to be "a general summary of the position and prospects of the world's production and utilisation of fibres," though incidentally we may mention that the utilisation process called Spinning is not even indexed. "Issued under the authority of the Secretary of State for the Colonies," dealing with "one of the most important industrial problems arising from the war," after these problems have "engaged the attention of the Imperial Institute for many years," "especially in connection with cotton cultivation," it ought to be the best book available.

There are two ways of writing such a book. One is to aim at real progress by constructing a coherent exposition on the basis afforded by original researches which the author has made, and this we regret that Dr. Goulding has not attempted. The other is to make a compilation of existing information, and since this is the course which has been followed we must judge the book as a compilation pure and simple. But as a compilation it fails in its purpose through the inexplicable absence of any references to the sources of information which have been collated to make its pages. If we exclude "internal" references to the Imperial Institute publications, and one to the *Kew Bulletin*, there remains only *one* other citation of authority which we have been able to discover, namely Todd's *World's Cotton Crops* (p. 71), and this in such a form as to lead the unwary to imagine that this standard work is little more than a list of prices.

Frankly, the technical literature of textiles, scanty as it is, has been already overloaded with books of this type on the raw materials. The present volume would have been a real advance on most of these, though necessarily very superficial, had not its duty in the matter of references been so flagrantly neglected To those who are familiar with some of the original bulletins and memoirs which are utilised at first or second hand without acknowledgment in these pages, it is clear that specific references even to a fraction of them, much less to them all, would have made this book a standard work of reference. As it stands, we do not know what use to make of it, except perhaps for conveying a general savour of knowledge to the class of dilettanti. Even for this purpose there are several minor inaccuracies, the fault of the source of information, but for which the author is responsible in the absence of references.

Presumably "*Agrostis ypsilon*" (p. 41) is a *lapsus calami*, and "Sharguira" (p. 59) merely bad copying for Sharqia, but the suggested use of lead arsenate for controlling Pink Boll Worm has for some time been known to be as impracticable as it seems. To assign 1935 A.D. for the complete invasion of the

U.S.A. by boll weevil is an anachronism (p. 36), seeing that the pest damaged Georgias and Floridas severely in the year now ended. The converse of this is the assertion that the Nile Delta deposits are as much as 110 ft. thick, whereas at least one bore of 1,000 ft. has failed to penetrate them. On page 20 we find no mention of the revolutionary change in agricultural practice and theory now being caused in the American cotton fields by the introduction of close planting; the old practice of wide planting was based on unsound *à priori* reasoning, on prejudice, and on platitudinous precepts, the like of which are too frequent in these pages to satisfy a later generation. How can the most thorough breaking up of the surface soil, even to the depth of 9 in. reached by a European plough, in any way directly affect the subsoil so as to assist "the long tap root . . . to penetrate it freely" (p. 19), when that taproot reaches more than 7 ft. in vertical length? Or take p. 30 literally, test it on the specific example of a single cotton seed put into a letter at Barbados and posted to Khartum, and consider whether it is not meaningless to say that "when a variety of cotton is introduced into a new country, the conditions of its environment are changed, and the constitution of the plant receives more or less of a shock."

If the blame for these blemishes could be allocated to their true origins, the author would escape criticism, but whether we refer to his pages as scientists, business men, economists, statesmen or politicians, as consumers or as growers of the textile raw material, we are limited exactly to the text before us, and can prosecute our studies no further by any help the author gives us.

Now, nobody dare pretend that all the wisdom of all the ages concerning the world's production and utilisation of all plant textiles and papers, from cotton to marram grass, can be compressed within 231 pages, so as to be of value to all the interests we have just enumerated, for all of whom the book is apparently written. Such pages can only make a preliminary outline sketch and then tell us where to go next, and this is what this book deliberately omits to do.

The trouble is that there is urgent need for books which really fulfil the purpose for which this one claims to be written. It is, as the preface says, a matter of national importance. It will be one of Imperial importance when we come to reconstruction. The textile industries are in no way on the same level as the comparatively smaller engineering industries in regard to their organised technical knowledge and literature; they have necessarily lost much of their initial advantage over imitators and competitors in other lands; they are now striving to take up the running again, not only within the walls of their mills, but also in the fields which supply their raw materials, and they need the best assistance. Were the need not so great we would not have criticised so adversely, but our Empire is not so well off for scientific institutions and research workers that we can afford to allow the platitudinous method to be followed without protest.

L. B.

Morphology of Gymnosperms. By JOHN M. COULTER, Ph.D., and CHARLES J. CHAMBERLAIN, Ph.D. [Revised Edition. Pp. xi + 466, with 462 figures.] (The University of Chicago Press, 1917. Price $5.00 net. Postage extra. Weight 3 lbs.)

BOTANISTS owe much to the authors of this volume, not merely for the production of a comprehensive work of reference upon the Gymnosperms, but also for the instigation of numerous researches in their laboratories, which have been the means of filling some of the more obvious gaps in our knowledge. Originally a

modest account of some 180 pages, which appeared as Part I. of the *Morphology of Spermatophytes*, the text and figures together now occupy more than two and a half times that space, an increase of thirty-six pages on the edition of 1910.

It is just because the *Morphology of Gymnosperms* has been a valuable book to both teacher and student in the past, that we deplore the absence in the present edition of those changes requisite to bring the subject-matter abreast of modern work. Indeed the chief alterations comprise certain changes in the treatment of the Cycadales, a few additions and corrections, and a supplementary Bibliography.

The need for a revision of the text in the light of recent research is most patent in the chapter devoted to the Cycadofilices. Here, for example, we find the statement still remains that *Conostoma* and *Sphærostoma* agree with *Lagenostoma* and *Physostoma* in the possession of a beaked pollen chamber. The bibliography, too, of this section, in spite of the Appendix to Literature, is very incomplete, and among the papers that have appeared since the last edition, of which no mention is made, we may cite those of Bertrand ("Les fructifications de Neuropteridées"), de Fraine (*Medullosa pusilla*), Gordon (*Rhetinangium*), Kidston and Gwynne-Vaughan (*Stenomyelon*), Kidston and Jongmans (*Neuropteris obliqua*), Kubart (*Lyginodendron* and *Heterangium*), Oliver and Salisbury (*Conostoma*), Salisbury (*Trigonocarpus Shorensis*), Scott (*The Heterangiums*) and Scott and Jeffrey (*Calamopitys*). A number of important papers dealing with the Gnetales have appeared since 1910, and here again the results achieved require to find adequate expression in the body of the volume.

It is to be hoped that in the near future the authors and publishers will see their way to the production of an entirely revised edition such as is warranted by the advances in our knowledge. Even, however, in its present form this is by far the most complete account extant, and as such should be in the hands of every one engaged in the study of this group.

E. J. SALISBURY.

Histology of Medicinal Plants. By WILLIAM MANSFIELD, A.M., Phar.D. [Pp. xi + 305, with 54 figures and 127 plates.] (London: Chapman & Hall, 1917. Price 13*s*. 6*d*. net.)

THE object of this work "is to provide a practical scientific course in vegetable histology for the use of teachers and students in schools and colleges."

The profuse and, in most cases, excellent illustrations should render the book a useful atlas to the student of pharmacy, since the characteristic tissue elements of many common drugs are portrayed in detail. But for the student of vegetable histology the treatment is not sufficiently comprehensive, and, we regret to say, there are several inaccuracies and ambiguities of description, which, though perhaps not of serious importance for the pharmacist, render the book quite unsuited for botanical readers. Thus, in describing the arrangement of the vascular bundles, we read (p. 292), "the radial type of bundle is met with most frequently in monocotyledonous roots." Again, on p. 298, "this type of bundle (the open collateral bundle) is characteristic of the stems and roots of dicotyledonous plants." Such statements entirely ignore the essential similarity of construction in the primary structure of the roots of both groups, and the fundamental differences which exist between stem and root on the one hand and dicotyledon and monocotyledon on the other. The description of the bordered pit is also very inadequate, and the same may be said of the characterisation of the phloem or the treatment of the two types of laticiferous elements.

Even for the pharmacist there are omissions of some moment. For instance,

we find no mention of the characteristic compound starch grains of several cereals, or the distinctive laticiferous vessels of the official *Chelidonium*.

Indeed one cannot but feel that the text does not do justice to the quality of the admirable illustrations. E. J. SALISBURY.

Soil Conditions and Plant Growth. Third Edition. By E. J. RUSSELL, D.Sc. (Lond.), F.R.S. [Pp. viii + 243, with 14 diagrams.] Monographs on Biochemistry. (London: Longmans, Green & Co., 1917. Price 6*s*. 6*d*. net.)

THE publication of a third edition of this excellent survey of our present-day knowledge of the soil in its relation to plant life is sufficient guarantee not only of its proved usefulness but also of the growth of interest in the subject. Since the first appearance of this work five years ago, numerous alterations and several important additions have been made to the text, consequent upon the considerable advances that recent years have witnessed in this field.

A chapter treating of the micro-organisms of the soil in relation to plant growth was added in 1915, and the present edition includes a much-needed account of the colloidal properties of the soil. This latter affords the author an opportunity of dealing with adsorption, and so to fill what constituted a very obvious gap in the earlier work. The subject of chemical reaction, especially in relation to soil acidity, is here also dealt with, we venture to think too briefly, having regard to its influence on vegetation and economic importance.

Apart from minor alterations, the remaining seven chapters maintain much of the form and scope of the original work, and deal respectively with: The History of the Subject; The Requirements of Plants; The Composition of the Soil; The Carbon and Nitrogen Cycles in the Soil; The Soil in Relation to Plant Growth; Soil Analysis and its Interpretation; and The Biological Conditions in the Soil.

It is a matter for regret that, in so comprehensive a volume, the author did not make the last-named chapter the occasion for treating, however briefly, of the biotic relations between plant and plant, since many of the edaphic factors are often probably in the main indirectly operative through this agency.

The Appendix on methods of Soil Analysis has been brought up to date by the inclusion of Hutchinson and McLennan's method for determining the lime requirement of soils, and the Bibliography has also been extended by the addition of references to recent papers.

The subject-matter generally is treated of more from the chemical than the biological standpoint, and here and there the latter aspect tends to become too much subordinated. This it is that constitutes the chief weakness of the chapter on "The Soil in Relation to Plant Growth."

The book as a whole is, however, one that can be confidently recommended to all students of plant life, and, indeed, is an almost indispensable item in the equipment of the bookshelf, either of the agricultural student, the ecologist, or the general biologist. For either of these Dr. Russell has provided an excellent book of reference, containing a large amount of information in a condensed form, a phase of usefulness that would be greatly enhanced by the provision of an adequate index. E. J. SALISBURY.

British Grasses and their Employment in Agriculture. By S. F. ARMSTRONG, F.L.S. [Pp. viii + 199, with 175 illustrations.] (Cambridge: at the University Press, 1917. Price 6*s*. net.)

AS the author states in his preface, this volume has been written primarily to supply the needs of agricultural students. With the object of meeting the special

requirements of this class of reader the descriptions of the different species lay particular emphasis on characters of foliage and fruit. The so-called seeds of most of the common grasses are figured, and artificial keys are supplied based respectively on the foliage, the floral characters, and the "seed." The first named of these loses much of its usefulness through the omission of such species as *Koeleria cristata* and *Triodia decumbens*, both of which are so common as to render their exclusion indefensible.

In the Botanical section the student would have been greatly assisted if important diagnostic features had been brought into prominence by the use of distinctive type. In some instances useful characters have been omitted, as for example in the description of *Holcus mollis*, where we find no mention of the exserted awn, a feature which, in the fruiting condition, offers the most facile field distinction from its congener. We deplore, too, the rather cursory treament of critical forms such as the species and varieties of *Festuca* and *Agrostis*.

The short section on the distribution of grasses might well have been fuller and, on some points, better informed. For instance, on p. 23 we find the statement that *Psamma* and *Elymus* are Halophytes, whilst no mention is here made of the really halophytic grasses such as *Glyceria maritima*, *Agrostis maritima* and *Lepturus incurvatus*. Surely, too, among the species characteristic of calcareous soils *Poa rigida* and *Poa compressa* might have been included rather than such atypical species as *Nardus stricta* and *Triodia decumbens*, which occur almost exclusively on siliceous soils or, if in calcareous areas, where the surface layers have become leached.

In Part II. the agricultural value and characteristics of farm grasses are dealt with, and here a considerable amount of data is given relative to commercial "seed" and its common impurities.

Taken as a whole the book furnishes much of the information that is required by an agricultural student and, in so far as stress is laid on points of economic importance, marks a distinct advance on the current works on agrostology.

E. J. SALISBURY.

The Grasses of Ohio. By JOHN H. SCHAFFNER. [Pp. 79, with two plates.] (Ohio State University, 1917. Price $0.50.)

MR. SCHAFFNER has provided what may be described as a Graminaceous Flora of the State of Ohio. Two synopses and a key to the tribes and genera precede descriptions of all the grasses native to this district together with those which have been introduced or are commonly cultivated, a total of about 180 species. Artificial keys are provided for all the larger genera, but unfortunately there is no attempt to distinguish the segregates of polymorphic types, such as *Festuca ovina* and *Koeleria cristata*, and in many cases the diagnoses might well have been amplified. Economically the recognition of the variety or microspecies is often more important than the separation of the larger aggregates, since the former in general have a more restricted distribution in relation to the habitat conditions.

Floral characters are chiefly relied upon, and only occasionally is data furnished with respect to ligular morphology, etc., though vegetative distinctions are, here and there, embodied in the specific keys. The appeal of the text is thus rather to the systematist than to those interested in the group from the agricultural or economic standpoint.

E. J. SALISBURY.

ZOOLOGY

Modern Whaling and Bear Hunting. By W. G. BURN MURDOCH, F.R.S.G.S. [Pp. 320, with 110 illustrations, chiefly from drawings and photographs by the Author.] (London: Seeley, Service & Co., Ltd., 1917. Price 21*s.* net.)

THIS book, as its expanded title points out, is "a record of present-day whaling with up-to-date appliances in many parts of the world, and of bear and seal hunting in the Arctic regions." The author who, as artist and historian, accompanied the Scottish Antarctic Expedition which sailed from Dundee in 1892, has every claim to be considered one of our leading authorities on the subject about which he writes. The result is a most interesting volume full of the smell of whale-oil, the blare of the Arctic ice, and the smack of the salt ocean. It is, on the whole, decidedly entertaining to read, though we must confess it suffers in places from a little unnecessary repetition. To those interested in either whaling or bear hunting it will prove a mine of information and experience, and it has also a strong appeal to any outdoor naturalist or sea lover.

In the course of much knocking about the writer gathered an amount of knowledge which enabled him, in company with a Norwegian, Henrikson, to design an oil-motor whaler. This vessel appears particularly seaworthy and well arranged, and in the opinion of a nautical friend to whom we showed the plans should be a very serviceable boat for its purpose.

Perhaps one of the most remarkable points that stands out in connection with the whaling industry is the extraordinary way in which the British have regarded it as a "speculation" from the commercial aspect, and the Norwegians, on the other hand, have turned it to account. This is a point made much of in the present book, and one that deserves to be hammered into the "business man." Apart from the employment it provides and the utility of the products, neither points to be disregarded, we read of a Norwegian company paying a dividend of 150 per cent., and another recently established which started with a 70-per-cent. profit in its first year. The latter, too, operated from the Falkland Islands—British dependencies. A year before the war the author and others started a whaling British industry in the Seychelles, but owing to the "war economy" of transport and coal imposed on them by the Government, were obliged to go into liquidation. Is it an accident or is it Fate playing one of her ironical jests, and so bringing it to pass that two of the most-needed products in this country now should be oil and fertiliser—the two staple products of the whaling industry?

We hope the author will return to the attack after more normal times have been restored, and then we trust he will meet as much success in his venture as in the production of this book—which is considerable.

C. H. O'D.

The Life of Inland Waters. An Elementary Text-book of Freshwater Biology for American Students. By J. G. NEEDHAM and J. T. LLOYD. [Pp. 438, with a frontispiece, 244 figures, and 19 initial and tail-pieces.] (Ithaca, New York: The Comstock Publishing Co., 1916. Price $3.)

THE primary appeal of this volume, as its sub-title indicates, is to American students, and as the forms studied are all local ones, it can never be used as a text-book in this country. Limnology, or more particularly the study of the ecology of the fauna and flora in and adjacent to fresh water, is a branch of biology that has been comparatively neglected in this country. Here, of course, no place is far from the sea, from which we draw enormous supplies of food, so

that our biological stations, with the exception of the private one of Mr. Gurney in Norfolk, are on the seaside. Quite different conditions prevail in North America, where vast tracts of land are remote from the sea, but, on the other hand, have access either to enormous lake areas or large rivers. Both these are of considerable economic importance, and consequently we find the United States supporting a score or so fresh-water biological stations, whose distribution is indicated on a map on p. 22. By the way, the numbering of two of the Canadian stations has been transposed, and this slip is not indicated in the somewhat long list of Errata given at the end of the book. The little work that has been done here, *e.g.* the survey of the Scottish lakes and the work of Dakin on the Plancton of Lough Neagh, are on quite different lines from those indicated in the present book. It evidently forms the basis of a course carried out in the station of the Cornell University at Ithaca, and lucky, indeed, is the student who can do his work under such enthusiastic guides as the authors.

It has a decided value in this country, since most of the phenomena treated can be paralleled here, only the species concerned being different, and it brings home in a very striking manner the interest and thoroughness of this aspect of biology. With comparatively simple equipment it is possible to obtain a great deal of pleasure and scientific knowledge from the study of fresh-water life.

The book, as a whole, is well illustrated and printed, and a good bibliography adds much to its usefulness. It is certainly one to be read by all biologists, and, indeed, by all interested in nature-study for the freshness of its outlook and the clarity with which it is written. We can heartily concur with the appeal at its close asking that efficient steps should be taken to preserve the wild fauna of the fresh water, and also to make the fishing of lakes and rivers a question of husbandry instead of, as it is at present, pure exploitation.

C. H. O'D.

The Natural History of the Farm. A Guide to the Practical Study of the Sources of our Living in Wild Nature. By J. G. NEEDHAM. [Pp. 348, with 140 figures and numerous schedules.] (Ithaca, New York: The Comstock Publishing Co., 1916. Price $1.50.)

THIS book is obviously intended for the use of nature-study pupils in a school. It takes the farm supplying our food as its central point, and the whole of the surroundings of this are treated at various seasons of the year; but in the main from the point of view of the part the various species play in the life of the farm. The course is, on the whole, very well planned, and indicates a very useful supply of knowledge which the students are to obtain for themselves. It is, of course, not of great use save for the drawing up of a similar course in this country, as the conditions and surroundings are so different. Ample directions and tabular schedules are given to aid in the practical work; they are purposely left blank in order that they may be filled in by actual observation.

C. H. O'D.

Rustic Sounds and other Studies in Literature and Natural History. By SIR FRANCIS DARWIN. [Pp. 231, with illustrations.] (London: John Murray, 1917. Price 6*s.* net.)

SIR FRANCIS DARWIN was well advised in publishing or re-publishing this collection of charming essays. The stranger, having once taken the book up, can scarcely

put it down again until he has read every page; and whatever may be the subject the author treats of, the reader is not only interested, amused, and entertained, but finds himself continually reflecting what a delightful person he is making acquaintance with. The subjects of the essays are miscellaneous. Four are personal; three deal with education; and science, music, and rural scenes each claim two. The fourteenth is upon dogs and dog-lovers.

To award the palm would be as unnecessary as it would be difficult. Each seems the best that has been last read, and of course the reader's special tastes go for much; but if a choice must be made, for this reviewer it would fall upon the essay on Jane Austen. No lover of that exquisite novelist can afford to miss this appreciation of her, many as its predecessors have been. The whimsicality of treating her characters as historical persons, tracing imaginary relationships amongst them, and showing the workings of the principles of heredity, the classification of them as a natural order into genera and species, the attractive into the dull and the interesting, and the grave discussion as to the species in which any individual is to be placed—all this is as delightful as it is novel. One feels that Sir Francis Darwin is competent to set an examination paper on Jane Austen comparable with that set by Calverley on Pickwick. Is it too much to hope that such a paper may be added to the second edition that will assuredly soon be called for?

C. MERCIER.

Studies in Indian Helminthology, Nos. 1, 2, 3. By F. H. STEWART, M.A., D.Sc., M.B., Capt. I.M.S. (*Records of the Indian Museum*, Vol. X. Part III. Nos. 9, 10; Vol. XII. Part VI No. 18.)

THESE three Studies deal respectively with nematode, trematode, and cestode forms.

No. 1 is a systematic paper describing five new species, and one new variety, of roundworms parasitic in Indian vertebrates, and five larval nematodes, the adults of which are unknown. One free-living form, *Oncholaimus indicus*, already recorded by v. Linstow, is redescribed. The new species named are *Oxysoma macintoshii* from the rectum of *Rana tigrina* and *Bufo stomaticus*, *Oxysoma kachugæ* from the intestine of *Kachuga lineata*, *Heterakis macronis* from the intestine of *Macrones aor*, *Dacnitis callichroi* (two females only) from the intestine of *Callichrous macrophthalmus*, *Atractis kachugæ* in enormous numbers in the intestine of *Kachugæ lineata* and *Spiroptera denticulata* Rud. var. nov. *minor*, (two males) from the stomach of *Wallago attoo*. All of these species were collected at Lucknow. Of the larval forms one, diagnosed as a *Physaloptera*, was found in the bladder wall of *Bufo stomaticus*, the other four occurred in *Wallago attoo;* two of these are diagnosed as *Ascaris* larvæ, the other two were not identified. The various species are carefully differentiated from closely allied forms.

No. 2 deals wholly with the anatomy of *Polystomum kachugæ* n. sp., a trematode from the urinary bladder of a water tortoise, *Kachugæ lineata*, obtained at Lucknow. This is carefully differentiated from the six other species of *Polystomum* that comprised the genus at the time of the completion of the paper.

No. 3 records an unsuccessful attempt to infect white rats with the dwarf tapeworm of man, *Hymenolepis nana*. It is a matter of practical importance to ascertain whether this is the same species as *Hymenolepis murina* of the rat. The author concludes, perhaps somewhat hastily, from his negative result that these

two species are distinct, and that consequently the rat is not a source of infection to man.

R. T. L.

The Mosquitoes of North and Central America and the West Indies. By Leland O. Howard, Harrison G. Dyar, and Frederick Knab. Vol. IV. Systematic Description (in two Parts), Part II. (pp. 525–1064). (Washington, D.C.: Published by the Carnegie Institution of Washington, 1917.)

This work, the second part of the systematic description, forms the concluding volume of the authors' monograph of the American Mosquitoes. The subject-matter is in direct continuation of that of Vol. III. It is equally comprehensive, and the multitudinous and frequently complex details pertaining to this aspect of the subject are marshalled and deployed in the same methodical sequence as before.

The authors relegate mosquitoes to a subfamily of the Culicidæ, and consider that the numerous forms fall naturally into two sections or tribes, the Sabethini and Culicini. The Sabethini and later Culicine genera are treated in Vol. III.; the Megarhinines and Anophelines are grouped with the more generalised Culicini and are discussed with the latter in the present work. In many instances the authors' conceptions of Culicid genera differ widely from those of previous writers. Owing, more particularly, to a careful and minute study of genitalic and larval structure natural relationships have been more clearly demonstrated and the limits of some of the older genera much extended. Hence, many characters which formerly were deemed to be of generic value are rendered invalid, and synonymic lists of appalling dimensions are unfortunately not uncommon. This is specially noticeable in the case of the genus *Aëdes*, where some fifty additional names swell the list. However, if the authors' interpretations be correct, such results are unavoidable and must be accepted.

Psorophora, the first genus dealt with, is enlarged to include *Janthinosoma* and that part of Theobald's genus *Grabhamia* typified by *G. jamaicensis*. The structure of the female claws in *Grabhamia* is not considered of sufficient importance to warrant separation from *Janthinosoma*, which takes sub-generic rank. The genus (*Stegomyia*) in which the well-known carrier of yellow fever occurs is among those which have been incorporated with *Aëdes*, but the generic change of name consequent upon this is apparently not the only one necessary. We learn that, in accordance with the laws of priority, the specific name of this mosquito is now *Aëdes argenteus*, since it was described as *Culex argenteus* by Poiret in 1787, thus antedating the *two* names in common use. In the Anopheline group the genera *Anopheles* and *Cœlodiazesis* are recognised; the latter contains a few forms, allied to our *A. plumbeus*, whose larvæ live in hollow trees.

The mosquito fauna of the regions covered by this monograph at present consists of 398 species, and of these 380 are treated in detail in the systematic volumes. The present volume deals with nine genera and 168 species, and contains an appendix in which notes are given on some of the species described too late to receive full discussion in the work.

The issue of this volume must be a source of great satisfaction to the authors, and enables dipterists in general and students of the Culicidæ in particular to offer their sincere congratulations to Messrs. Howard, Dyar, and Knab on the successful completion of a most laborious work.

H. F. C.

Shells as Evidence of the Migrations of Early Culture. Ethnological Series, No. 2. By J. WILFRED JACKSON, F.G.S. [Pp. xxviii + 208.] (Manchester: The University Press; London: Longmans, Green & Co., 1917. Price 6s. net.)

WITHOUT doubt, the more we can discover of the mainsprings of the "behaviour" of pre-historic peoples, the more readily shall we be able to estimate the factors which have moulded the evolution of civilisation. For we are swayed, more than we imagine, by the customs and beliefs of our forbears.

A great stumbling-block to progress in this matter has been thrown in our way by those who have seen, in widespread customs and beliefs, evidences of the "similarity of the working of the human mind." The all too ready acceptance of this quite unwarranted assumption has seriously distorted our mental vision, thereby giving us a false standard of the factors of social evolution. Our customs, beliefs, and organisations, like our bodies, have come down to us by genetic descent: they have not come into being by "spontaneous generation," nor as the sporadic outbursts of "intuition." This much Prof. Elliot Smith has been striving to bring home to us for some years past; and he has produced some extremely convincing evidence in favour of his contentions by his studies on mummification, and the heliolithic culture. It is idle to contend that the practice of mummification in Torres Straits, and America, was in each case independently devised, and not derived from Egypt. Prof. Elliot Smith has surely established his case in this matter: and now further, and no less striking, testimony of the soundness of his views has been furnished by one of his pupils—Mr. Wilfred Jackson—in regard to the important part the search for shells has played, in the diffusion of the elements of culture, and in the upbuilding of civilisation.

The cult of the cowry must apparently be reckoned the oldest of these factors in the evolution of civilisation, for cowry shells have been found interred with Cro-Magnon man. Possibly to him, and certainly to very primitive peoples, we must attribute the belief that the cowry-shell, in some way, imparted fertility to women and help in parturition: a belief suggested, as we learn from ancient writers who held it, by the likeness of the under-surface of the shell to the human vulva. So profound was the belief in its life-giving powers that the dead were interred with cowries to ensure their resurrection; while as a charm against the Evil Eye this shell was unfailing. Hence it came to be associated with good luck, and used in games of chance. These shells came, indeed, to be regarded as pearls of great price, to be parted with only in exchange for some coveted possession; and hence the place they came to hold among primitive peoples as currency.

Prof. Elliot Smith, in a preface to this book, suggests that the earliest conceptions of a deity arose out of these beliefs connected with the cowry; and that the Red Sea and the Eastern Mediterranean constitute the original home of the now world-wide cult of shells. The point of his argument is not that shells are universally esteemed by primitive peoples, but that everywhere, both in the Old and New World, the same forms of shells are associated with the same usages and beliefs eastward from Crete, their supposed birth-place, to China, and from China to Peru; and westward to our own islands. It is hardly credible that such associations of ideas should have developed spontaneously over areas so enormous.

What is true of the cowry-shell is true also of the purple-dye industry and the use of conch-shells for ceremonial purposes. Mr. Jackson, in the main body of this book, has brought together a mass of evidence to show that wherever these

are found they are associated with usages and rites which must have had a common origin, even though superficially modified in some regards.

A tremendous amount of labour has been expended on this work; and it may seem ungracious to suggest that it has in any way failed of its purpose. But it would unquestionably greatly have enhanced the cogency of its arguments if a summary had been given setting forth, so far as is possible, the precise relationship of this shell-cult with the heliolithic cult, and the search for gold and copper; which seem always to have been closely associated. As it stands the book loses some of its force in the bewildering mass of facts presented without sufficient co-ordination to enable them to be quickly assimilated.

W. P. P.

ENGINEERING

The Education of Engineers. By HERBERT G. TAYLOR, M.Sc., Assoc.M.I.Mech.E. [Pp. viii + 64.] (London: G. Bell & Sons, Ltd., 1917. Price 2*s*. net.)

WE cannot but very greatly regret the whole tone of this ill-considered effort.

The author finds serious fault with science in general and engineering professors in particular, but the following extract will serve to place his opinions at their proper value: "Armed with exponential lassoos and integrated boomerangs, science prefers the pursuit of new-fangled rays and electrons *à la mode*. I would suggest that it take a trip on the next passing comet and betake itself to the uttermost ends of space, and, when Vega crown the pole, return, leading captive all the rays from the infra-alpha to the ultra-omega, and bearing skeletons of micro-electrons from every stratum of the universe ftom Polaris to the Southern Cross. Then, and not till then, will we believe that Röntgen and the Curies were not afflicted with a particularly crooked mentality."

J. WEMYSS ANDERSON.

MISCELLANEOUS

Hyperacoustics. Division I. Simultaneous Tonality. By JOHN L. DUNK. [Pp. vi + 311.] (London: J. M. Dent & Sons; New York: E. P. Dutton & Co., 1916. Price 7*s*. 6*d*. net.)

THIS volume forms the first part of a large work upon which the author is engaged on "Hyperacoustics." The whole work is to be a treatise on music which will embrace the consideration of tonality, rhythm, organisation, and significance, and this first volume is devoted to the consideration of tonality—the science of musical sound in pitch and quality. We live in the days of hyperbole, so the title of the work may not strike the reader as strange. The author says that "the name 'HYPERACOUSTICS' may be proposed, as indicative not only of something beyond, but also of a presumption requiring justification as to the existence and rationality of something beyond the known facts of acoustics." This presumption is certainly not justified in the present work. He also says, "Between the region of phenomena (undefined) comprised in the science of acoustics, and the experiences of music considered as phenomena, there appears a great gulf, which invites attempts to bridge. . . . In order to form a concept of this vast and mysterious region, a name is required." He does not stop to consider whether the "experiences of music" can in any real sense be considered as phenomena, nor does he give any grounds for the statement.

Samuel Pepys said, more than two centuries ago, that music might be enjoyed to much better effect "were the doctrine of it brought within the simplicity, perspicuity, and certainty common to all other parts of mathematical knowledge, and of which I take this to be equally capable with any of them, in lieu of that *fruitless jargon of obsolete terms, and other unnecessary perplexities and obscurities*, wherewith it has been ever hitherto delivered." The italicised words show that this fault was patent even then, but this book is written in a manner which puts such books as Pepys had on music quite into the shade. The author says, "The scientific aspect of Hyperacoustics as yet lacks the general language to connect its concepts with common thought," and he thereupon invents a new terminology, and says, "It is a somewhat bold and solemn undertaking to attempt to supply this deficiency of language, whose necessity may not even command assent." His attempt has made quite ordinary and well-known facts quite unrecognisable. There is, perhaps, no science in which our ideas are so much swayed by words as the science of music. With an imperfect knowledge of the phenomena we frame inexact terms, and then, taking these shifty terms for standpoints, build up a rickety system. This result is shown in this book in which the nomenclature is corrupted partly by the pressing of various languages into one and partly by the employment of old terms in new meanings, which causes endless confusion in the mind. This amounts to the creation of a new language which very few musicians or scientists will take the trouble to acquire, unless the new knowledge gained is far greater than that obtainable from this book. Moreover, when you have, as in this attempt, obscurity of thought hiding under obscurity of language, it becomes extremely difficult to unravel anything useful.

The book begins with a study of tones, pitch, and intervals, and then goes on to discuss scales and chords. The subject of intervals, which is complex, is made much more complicated by the dragging in of the means of another art—namely, by the dragging in of the names of colours, and the formation of what is called a "chrome system." The "chromes" are simply colour-names for intervals, for which there is no justification whatever, and their use is persisted in right through the book. It does not simplify intervals, or make them clearer, to say a major-third is green, a minor-third violet, and a major-sixth yellow: it is of no help for demonstration, and is a useless burden to the memory. The author says ingenuously, "Colour symbolisation may, of course, be carried to a considerable extent, but nothing is gained by complexity." He then invents the term "fluent," which may be regarded "as the operator of conversion between one chrome and another," and says, "We might use the analogy of agencies converting colours from one to another, by any well-known optical, chemical, thermal, etc., processes, to represent the particular fluents—*e.g.* acid-alkali, hot-cold, hæmoglobin-chlorophyll, spectral left-right, etc., etc.": the confusion in this sentence shows where colour symbolisation has led the author and into what perplexities such ardent changes of nomenclature may lead us.

Of such facts as can be dug out of this book there is not much to be said by way of criticism, for, as the author owns himself, there is very little that is new to be found in it, the material being mainly "a collection and arrangement of the thoughts of others." There is, on account of the modern developments of the art of music, a need of such a book, or series of books, such as the author has planned out, and if only he would translate this first volume from the "fruitless jargon" in which it is written into English, and would write the remaining volumes in the same language, such a work might be very useful.

H. G. P.

French Scientific Reader. Edited, with Introduction, Notes, and Vocabulary, by FRANCIS DANIELS, Ph.D., Assistant Professor of Modern Languages, University of Missouri School of Mines and Metallurgy. [Pp. xviii + 748, with illustrations.] (New York: Oxford University Press, American Branch; London: Humphrey Milford, 1917. Price 10*s*. 6*d*. net.)

THIS admirable scientific "reader" gives a very good picture of the part the French have taken during three centuries in the development of modern science. As the editor remarks in the preface, "There is no finer French prose than can be found in some of these papers. . . . With the exception of Lamarck, whose barren style kept him for half a century from the recognition due to his genius, all the great French scientists have written well; some, like Buffon and Fabre, have written extremely well, and their work is an imperishable part of the literature of France." The contents consist of extracts from famous works dealing with nature in general, the system of the world, the earth, the ocean, minerals, physics, chemistry, life, plants, animals, man, the soul and senses, the theory of probabilities, and scientific method. The authors of these extracts are Buffon, Laplace, Lapparent, Loti, Thoulet, Becquerel, Mme. Curie, Berthelot, Lamarck, Von Tieghem, Candolle, Michaux, Humboldt and Bonpland, Pasteur, Fabre, Lesson, Cuvier, Pascal, and Taine. There are various illustrations, including seven of these authors. "One may smile at my including Loti among French scientists, and with reason, but he knows the desert as few scientists do, and he describes it as no one else can. Nor let any one be indignant if I have admitted one stranger to a place here. Humboldt's *Essai*, which created a new science, is in itself a tribute to the superior fitness of the French language to serve as a medium of scientific expression. I have one other name to mention here, that of André Michaux, whose indefatigable toil in the field of American botany makes an inclusion of a specimen of his work a duty to an American editor" (pp. iii, iv). The book includes, certainly, one article which was purposely chosen to enable the student to realise the vast advance made by science during the last century and a half. "For it seems to me that one of the functions of a scientific reader should be to give the student some adequate knowledge of the development of science itself" (p. iv). Under the heading "Le Nombre" is given an extract from Laplace's essay on probabilities, and this is the only extract dealing with mathematics. It might have been possible for the purposes of a scientific reader to give extracts from Lagrange's lectures on elementary mathematics, which are written in a very fine style and are quite easy of understanding. This is a most excellent book.

PHILIP E. B. JOURDAIN.

The Year-book of the Universities of the Empire, 1916 and 1917. The Universities Bureau of the British Empire. [Pp. xiii + 412.] (London: Herbert Jenkins, 1917. Price 7*s*. 6*d*. net.)

THE publication of this useful year-book was discontinued for a year, but it was thought that in spite of war conditions it would be better to reissue it, and with this decision we are in entire agreement. It is an extremely useful volume, and, in spite of the difficulties of printing and binding which have been responsible for its late appearance last year, is one whose value justifies its production even under adverse circumstances. In it will be found information respecting all the universities and university colleges of the Empire, with appendices giving summaries of the Beit Memorial for Medical Research and for Scientific Research, Royal

Exhibition (1851) Scholarships, and Rhodes Scholarships. Not only are details of the colleges provided, but also a list of the holders of offices in them. At a time when we are in closer touch with our Colonies than perhaps ever before, such a book is most opportune, and, indeed, it would be a magnificent thing for the future of the Empire if the universities themselves could be brought more closely together. The various colleges are still to a large extent isolated, and nothing but good would result from their being more firmly knit. Several points need immediate attention. The existing pension scheme of the universities of Great Britain could, with comparatively little modification and adaptation to local conditions, be extended to the whole Empire, and would be a large step in the direction of a freer interchange of their staffs. Some move should also be made in the matter of a fuller recognition of the examinations and degrees of other universities. This would probably mean a certain amount of standardisation of the examination, again making allowance for local conditions, but would be of enormous benefit.

These points are, of course, not dealt with in the present volume, but must inevitably occur to any one who uses it. After our daughter nations have sealed their allegiance to the mother country on the battlefield it would be little short of a calamity if they were to be allowed to drift away again, and any closer union of the various university institutions would undoubtedly be a very strong link to hold the countries together.

Once again we must congratulate the Universities Bureau on a useful book which is singularly free from slips, and whose information, so far as we have been able to check it, is accurate.

C. H. O'D.

W. E. Ford: A Biography. By T. S. BERESFORD and KENNETH RICHMOND. [Pp. vii + 310, with photogravure frontispiece.] (London: W. Collins, 1917. Price 6*s*. net.)

THE need that is gradually making itself felt in Science for unification and synthesis between the individual sciences is extending to all branches of civilisation, and even to civilisation itself. Analysis, no doubt, is essential to the development of knowledge, but practised by itself it inevitably leads to the creation of water-tight compartments, unless subordinated to the final process of redintegration in the corpus of human knowledge. This fissipareous tendency has in the past played havoc with education, unduly isolating subject from subject, and often totally obscuring the unifying aim of the school to which, if it exists, these subjects should be ancillary. These reproaches cannot be levelled against the highly interesting educational experiments described in the purported biography of W. E. Ford. Here at least, whether real or imaginary, was a school given over neither to the excessive cult of subjects no the memorising of facts, but to the understanding and *relating* of the latter. They were treated in such a way as to lead to their forming a more or less coherent whole in the pupil's mind, the ultimate aim of the education given being to make the knowledge thus intelligently mastered an instrument of culture and self-expression, the whole being linked on to a doctrine of vitalism of the Bergsonian type, which space forbids setting forth here.

In opposition to the ordinary public schools, "mere mental and physical tanneries," according to Ford's father, Ford set up a school for both sexes. From the outset the pupils were taught by practice that all knowledge is only approximate, and that knowledge gained is but a stepping-stone to the acquisition of

more exact knowledge, a point mathematicians, ever in search of more rigorous proofs, will appreciate. Scripture was merely taught as a development of the sense of right; stress was laid on the importance of learning language rather than languages. Mathematics was taught as a single branch of knowledge, and pupils encouraged to state problems as well as to solve them. Geography and history were regarded as two parts of one and the same problem, the interrelation between man's environment and his activities; physical geography proper went with the science lesson, and mathematical geography with mathematics. Science was taught as a whole and regarded as an outlook on life, and the practical was always taken as a basis for the starting of new work.

Thus the study of the formation of rocks and soils in the geography of the world was rendered real by an analysis of the commoner rocks and soils, specimens of which were collected from different parts of the country. These samples were utilised for experiments in plant life, and finally flower fertilisation was made to lead up to eugenics and the teaching of sex. Insistence was laid on character training being a positive thing, and not the mere acquisition of a certain number of social tabus. Morality was further taught not by ignoring the savage who lurks in the soul of every child, but by explaining to the latter that this primitive self needed training and manners to fit the individual for civilised society. Natural history was thus utilised to show its relation to morals. Highly interesting as the experiment was, and containing much of great value, it broke down for what appears to us a very simple reason. While having largely solved the problems of imparting general culture, Ford had not seen the necessity for the school to provide a preparation for livelihood as well as for life.

CLOUDESLEY BRERETON.

A Short History of Science. By W. T. SEDGWICK, Professor of Biology, and H. W. TYLER, Professor of Mathematics at the Massachusetts Institute of Technology, Cambridge, New York. [Pp. xv, with 14 illustrations.] (New York: The Macmillan Co., 1917. Price 12*s*. 6*d*. net.)

THE motto of this book is a quotation from Sarton, that "The history of science should be the leading thread in the history of civilisation." One would think that every man and woman should be so proud of the achievements of mankind in science that they would prefer to read of its triumphs rather than to read of the slaughter of their fellows, the actions of brigands and buccaneers, and the silly doings of th· nobodies about whom novelists like to tell us so much. The fact is, however, that not one person in a hundred knows anything about even the greatest epochs of scientific history.

The subject is a large one, and the present book pretends only to be a short history; but we can heartily commend it, not only to the perusal of young persons, but for the libraries of men of science. The authors generally adopt the plan of considerable quotations from leading authorities on each part of the subject, and we are very grateful to them for inserting a short bibliography at the end of each chapter. They also give an excellent appendix containing transcripts of some of the most important scientific pronouncements, such as the oath of Hippocrates, Newton's Preface to his *Principia Mathematica*, Jenner's *Enquiry into Cowpox*, an extract from Lyell's *Principles of Geology*, and so on; and add a table of the most important dates in science and in general history, literature, and art. There is a good index. The book itself begins with a brief account of the early civilisation as disclosed by archæology and anthropology. Surely the authors are very modest in ascribing an age of only 250,000 years to mankind, and there-

is but little reference to Chinese, Japanese, and American early civilisation. Naturally the older discoveries receive their closest attention—because it is these discoveries whose magnitude is better seen in the perspective of time. The mathematical advances are briefly but excellently recorded; and we are glad to see that due judgment is exercised in dealing with such a thorny subject as the Newton-Leibnitz controversy (ought not this to be spelt Leibniz?). Surely Sir William Hamilton's work on Imaginary Numbers, which is really the perfection of the theory and the most beautiful form of geometry possible, should have been mentioned, if not described. In biology, perhaps too little attention is given to pathology, which is, after all, the most immediately important science of all to humanity, being, in fact, the coping stone of the whole biological arch. But such a book must not be too long, and we do not think that the authors could have done better than they have done within the compass allotted to them. We forgot to note that there is an excellent appendix of some inventions of the eighteenth and nineteenth centuries and a number of interesting illustrations. It would have been useful if publishers' names had been inserted in the bibliographies, and if some indication had been given as to where classical writings on scientific subjects, or translations, can be obtained.

BOOKS RECEIVED

(*Publishers are requested to notify prices*)

An Introduction to Theoretical and Applied Colloid Chemistry. By Dr. Wolfgang Ostwald, Privatdozent in the University of Leipzig. Authorised Translation from the German by Dr. Martin H. Fischer Eichberg, Professor of Physiology in the University of Cincinnati. New York: John Wiley & Sons; London: Chapman & Hall, 1917. (Pp. xv + 232.) Price 11*s*. 6*d*. net.

Lecture Notes on Light. By J. R. Eccles, M.A., Assistant Master at Gresham's School, Holt. Cambridge: at the University Press, 1917. (Pp. 215.) Price 12*s*. 6*d*. net.

A Text-book of Physics. For the Use of Students of Science and Engineering. By J. Duncan, Wh. Ex., M.I.Mech.E., Head of the Department of Civil and Mechanical Engineering at the Municipal Technical Institute, West Ham, and S. G. Starling, B.Sc., A.R.C.Sc., Head of the Physical Department at the Municipal Technical Institute, West Ham. London: Macmillan & Co., St. Martin's Street, 1918. (Pp. xxiii + 1081.) Price 15*s*. net.

Annuaire International de Statistique Agricole, 1915 et 1916. Institut International d'Agriculture. Service de la Statistique Générale. Rome: Imprimerie de l'Institut International d'Agriculture, 1917. (Pp. xlix + 949.) Price 10 francs.

Worm Nodules in Cattle. Commonwealth of Australia, Advisory Council of Science and Industry, Bulletin No. 2. Melbourne, 1917: Published under the Authority of the Executive Committee of the Advisory Council. By Authority: Albert J. Mullet, Government Printer, Melbourne. C. 10020. (Pp. 29.)

A Year of Costa Rican Natural History. By Amelia Smith Calvert, Sometime Fellow in Biology, Bryn Mawr College, and Philip Powell Calvert, Professor of Zoology, University of Pennsylvania. New York: The Macmillan Company, 1917. (Pp. xix + 577, with maps and illustrations.) Price $3.

Organic Evolution: A Text-Book. By Richard Swann Lull, Ph.D., Professor of Vertebrate Paleontology in Yale University. New York: The Macmillan Company, 1917. (Pp. xviii + 729, with 30 plates and 253 figures in the text.) Price 16*s.* net.

The Linnean Lecture on the Law of the Heart. By Ernest H. Starling, M.D., Sc.D. (Cambridge and Dublin), F.R.C.P., F.R.S. Given at Cambridge, 1915. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta and Madras, 1918. (Pp. 27, with diagrams.) Price 1*s.* 6*d.* net.

Text-Book of Ordnance and Gunnery. By Lieut.-Col. William H. Tschappat, Ord. Dept., U.S.A. New York: John Wiley & Sons; London: Chapman & Hall, 1917. (Pp. x + 705.) Price 30*s.* net.

Telegraph Practice: A Study of Comparative Method. By John Lee, M.A., Postmaster of Belfast and late Deputy Chief Inspector of Telegraph and Telephone Traffic, G.P.O. London. London: Longmans, Green & Co., 39, Paternoster Row, and New York, Bombay, Calcutta, and Madras, 1917. (Pp. 102.) Price 2*s.* 6*d.* net.

Some Questions of Phonetic Theory. By Wilfred Perrett, B.A. (Lond.), Ph.D. (Jena), Officier d'Académie. Part I. London: The University of London Press, 18, Warwick Square, E.C., 1916. (Pp. 110.) Price 2*s.* 6*d.* net.

Studies in the History and Method of Science. Edited by Charles Singer. Oxford: at the Clarendon Press, 1917. (Pp. xiv + 304, with 19 plates and 23 figures in the text.) Price 21*s.* net.

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ERRATUM

On page 469 of SCIENCE PROGRESS for January 1918 the Author has written "succeeding Middle Glacial and pre-Crag specimens." This should read "preceding Middle Glacial," etc.